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SCIENTIFIC INTELLIGENCE.

ERRATA.

In part of the edition, (No. 50,) l. 10 from bottom, *for O. enatica*, read *O. erratica*.—P. 211, l. 10 from bottom for Ca S, *read* Cd S.—P. 220, l. 7 from top, for 1 : 3 : 4, *read* 1 : 3 : 4½.—P. 240, to the analysis, add, phosphorus 0·21.

P. 240, l. 16 from top, for tartaric acid, *read* titanic acid.

P. " l. 27 " " " " " " "

P. " l. 31 " " *erase* phosphorus.

P. 333, l. 10 and 11 from bottom, for Mr. *read* Dr.

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[SECOND SERIES.]

Art. I.—*Eulogy on Von Buch;* by Professor Cotta of Freiberg.*

At Freiberg on the 19th of March was held a funeral festival in honor of Leopold von Buch, the most celebrated geologist of our time, who died in Berlin on the 4th of the same month.

At 7 o'clock, p. m., the scientific audience collected in the Wernerian Hall of the Academy of Mines, which unfortunately did not possess extent enough to allow of the unusual attendance of the public at large. Professor Heuchler had decorated the cenotaph with the coat of arms and portrait of the deceased. Fitting geological specimens surrounded its base, which all had especial

* Professor Silliman.—*Sir*—The accompanying translation of an eulogy pronounced in the Academy of Mines here, by Cotta, Professor of Geology, I have taken the liberty of sending you for your scientific Journal.

It is taken from the Leipzig "Illustrirti Zeitung" of April 9th, where the proceedings at length, with the illustrations, are given. The speech on the part of the students I have left out; and you may also omit if thought better, the introductory remarks of Prof. Breithaupt accompanying the translation. I have translated the address because it gives a true portrait of the remarkable man by one who knew him intimately, and also that his labors may be better known than they perhaps are in America, where he desired so much in his latter days to go, and of whose munificent patronage of science he ever spoke in unbounded praise. I had the honor of making his acquaintance last winter through the kindness of Prof. Cotta, while in Berlin, and of learning from him more of the general features of American geology with which he had of late particularly employed himself, than I had expected to do in Europe. He expressed but one regret, and that was that he could not be young again to go there and devote himself to the development of her geological resources. I have the honor to be, Sir, your ob't serv't,

J. Graeme Ellery.

Royal Saxon Acad. of Mines, Freiberg, June 4th, 1853.

reference to his scientific labors—such as ammonites and terebratulas, lava masses and other eruptive rocks, jurassic limestone, and chalk, druses of crystals and fossil leaves from the brown coal.

The mining and smelting officers of the crown, the professors of the Academy, in uniform, together with the students, at present uncommonly numerous even from the most distant foreign lands, filled the academic halls.

Professor BREITHAUPT first ascended the tribune and opened the exercises with the following remarks:—

Honored Friends! An aged, venerated and lordly oak in the German grove of science has fallen. Leopold von Buch is no more! Deeply do we feel this painful loss, the more deeply, as we may justly say, that in many respects, he belonged especially to us. Already in 1790 he was matriculated at the Academy of Mines at this place, and here his gifted genius laid the foundation of its subsequent comprehensive attainments. He ever remained true and attached to his alma mater, kindly communicating the rich fruits of his profound observations and investigations, and supporting without intermission a friendly intercourse with the literati of Freiberg. Only three years since, at Werner's festival held at this place, where he was the foremost ornament among those then present, he proved his old and honorable attachments by a noble act of munificence. To such a man are we in duty bound to pay a public token of our reverent acknowledgment and of our warmest thanks; and for that purpose are we here to-day convened—at this festival to his memory.

Through Professor Cotta we shall forthwith learn how much and what signal service the great geognost and geologist has rendered to science. The extent of Von Buch's merit will ever cause his name to shine forth bright and brighter as a star of the first magnitude, not alone in Germanic literature, but also where-ever his favorite science may find a votary. By *us* can he never be forgotten—to us he will be ever peculiarly near and dear.

Then followed the eulogy by Professor COTTA.

We stand here before the manes and before the portrait of a man who devoted his whole life to Nature—of a man who once was Freiberg's and Werner's scholar, and whose fame now resounds far away over Germany's and Europe's boundaries, whose memory we will love, and so much the more ardently honor and celebrate, as he went forth from this school of mining life.

Leopold Baron von Buch was born on the 26th of April, 1774, probably at the old ancestral castle of the family at Stolpe, in the Uckermark in Prussia. Scarcely 16 years of age, he entered, on the 10th June, 1790, Freiberg's walls, where, under Werner's especial guardianship, and partially in his own house, he spent

three years. Here it was, that an intimate and long enduring friendship was formed between him, Alexander v. Humboldt, and Carl Freisleben. After the completion of his mining studies, he it was who most of all next to von Humboldt gave origin to that beautiful saying of d'Aubuisson de Voisin, that "Werner's disciples scattered themselves over every land and interrogated Nature as to her import from pole to pole." He also it was, however, who first of all brought back a negative answer from these wanderings. He went forth into the world a true and convinced disciple; soon, however, fact after fact accumulated before his clear vision, till he was convinced, at first, doubtless, painfully convinced, that his much loved master must have erred in one fundamental point.

We all are subject to error; and those who come after us will certainly know more, and know it much better, than we. It can therefore on that account never be made a subject of reproach to a disciple, that he has given up the system of his master from conviction; and it were also the worst way of honoring great men, to say nothing of its opposition to the spirit of true science, were we blindly to cling to all of their errors. On the contrary the acknowledgment of an error, or the discovery of a new truth on the part of the disciple, is ever a proof of the ability of both. The master has awakened a spirit of independent investigation—the disciple, by that, has laid down a proof of his own impartiality and of his independence. And never can a disciple in such a case act with more delicacy and forbearance against a master than did v. Buch, who never meanly attacked the *old*, but on the contrary presented the *new* only with more convincing power.

L. v. Buch wandered over,—and that too mostly on foot, as it behooves a naturalist to do,—one after another, not only all the mountain chains of Germany, the Alps from Nice to Vienna, the Appenines from Turin to their most southern spurs, the hilly chains of England and the highlands of Scotland, but he was almost as much at home in France as in Germany. He traversed again and again the Pyrenees, climbed the summit of Ætna, and the sublime Peak of Teneriffe. He had early wandered among the crystalline mountains of Scandinavia, and late in life even, when almost an old man, he wandered all over the Highlands of Greece, till then better known to the philologist than to the geologist. Everywhere, even where he only transiently tarried, he left behind him, as v. Humboldt says, luminous and radiant footsteps.

Thus was he ever, year after year, up to his advanced old age —to the very end of his days, on journeys. In early summer he wandered forth, and only with the storms of autumn did he return to his neat ground-floor study on the banks of the Spree,

where a cheerful domestic establishment surrounded the worker, with nothing however of that luxury which might have been expected from a man of his rank and opulence. The faithful Mrs. Baumgarten—known to almost every geologist—had been for more than twenty years his only attendant; and it very often happened that he himself opened the door to the one who rang. Were it a stranger who then had come, and asked, "Is Mr. v. Buch at home?"—he received an answer according to circumstances—"I will see"—or, "what do you wish of him"—or, "No." Even a well known acquaintance he sometimes also asked, "To whom do you come?"—or, began immediately even in the portal the conversation—generally with a question—on some determinate scientific subject. Once, while I was engaged in the geological survey of Thuringia, and visited him, he asked, even in the house-door, "Now does Ammonites semipartitus occur also in Eichsfeld?"

I have purposely made mention of these apparently insignificant circumstances, because they characterize the man, and because the last especially, shows the great activity of his mind, which, in moments of the greatest surprise, forthwith and omitting every introductory phrase, began at once at the very heart of the subject—at all times interesting to the new comer. However, I shall soon have occasion to speak of many peculiarities which not a little characterized the departed.

Permit me next to cast a glance at what science has gained from his life and genius. It would engage our attention for many an hour were I to enumerate all for which geology is indebted to him, who also in the departments of natural science as elsewhere, has given manifold proofs of a genius deep searching into the very heart of things. For it is especially characteristic of his labors, that he never lingered over trivialities, but knew how to discern at a glance the essential from the unessential, to render the characteristic distinctly characteristic, and to seek out the true connection of phenomena. It is less difficult even for an acknowledged obtuser mind, to make numerous and accurate observations, than through these to discern the legitimate in nature. This latter gift, however, was lent to L. v. Buch in a degree possessed only by the few.

The copiousness of his discoveries, however, will make it necessary for me to limit myself, at this time, to those alone, which appear the most important.

He it was who first of all in Germany proved with precision that the disturbance of the original relations in the deposition of strata cannot be explained alone by events on the surface, but that these have had in most cases a deep seated subterranean, plutonic or volcanic origin. He showed by little and little that not only the basalt, but also the other crystalline massive rocks have been

pressed upward from below in a molten condition like lava; and that by these same reactions of the earth's interior, the elevation of mountain chains, and of whole districts of country have been brought about.

He first directed attention to the determinate, and often reciprocally parallel, direction of lineal mountain chains, or extensive dislocations of strata. He distinguished four principal lines of elevation in Germany, upon which generalization Elie de Beaumont afterwards building, founded his artificial system of mountain lines, which, however, in its last form was not admitted even by L. v. Buch himself. He was the first of all to show that the large volcanoes had had their origin not alone from the simple heaping upon one another of lava streams and loose ejected masses, but that they had been elevated to a higher altitude together with the consolidated masses before present. In this manner he distinguished craters of elevation, and craters of eruption; and if in this last division he may in isolated cases have gone too far, still the most essential part of his doctrine will always remain of the highest importance.

Having once had his attention directed to the effects of volcanic activity, he investigated the mode of distribution of volcanoes upon the earth, registered upon charts all known ones, and showed that they were distributed partially in groups and partially in lines of which the last mentioned evidently were arranged upon long extended lineal cracks in the earth's crust. These investigations were first communicated in his splendid work on the Canary Islands.

Besides the local effects of the present volcanic activity as it is there developed where active volcanoes exist on the earth's surface, he early recognized also the effects of this same force in its more universal and less distinctly remarked phases. He it was who first of all in Germany proved that the continuous remarkable changes of level on many of the Baltic coasts cannot have their origin, as was generally believed to be the case, in the sinking of the sea, but that they are to be explained only by a gradual secular elevation of a great part of Sweden. And this view has since then been established beyond a doubt by an earlier opposer of it, Sir Charles Lyell.

I will not linger long over his theory on the part which the Melaphyre has played, and its influence on the formation of Dolomite, because this of all of his new views is perhaps most subjected to doubt. But even if this whole hypothesis should fall to the ground, still it was at any rate put forth with so much spirit and ingenuity, that it earned and obtained at the time, the highest attention, and in the most lively manner drew the attention of others to new investigations. In general the beautiful, lively and convincing manner of representation in all v. Buch's

labors is not the least part of their merit. In so simple and clear a style can no one write who is not perfectly master of his subject. Somewhat characteristic of him is it also, that he always shunned all references in the text, and with right, for they never belong to the embellishments of a book. They are generally only a consequence of the fact that the author is not able to spin out into one thread all that he would say, or that he has hung them on to prove his erudition, or they are entirely superfluous and do not belong to the subject. As they are impossible in the flowing speech, they should also be avoided as far as possible in written productions.

During the period of his long scientific career, occurred the discovery of the true meaning and the geological worth of organic remains; which till then had been looked upon as unessential things and had been but little noticed. Scarcely was their true import known than L. v. Buch entered upon this new department with the greatest zeal and happiest result.

First of all he devoted himself to the Ammonites, long however, before that peculiar delineations had been observed upon the surface of their interior petrified casts. Their true import and legitimate cause however had till then remained a mystery. He showed that they came from the walls of the interior concameration and at the same time pointed out the peculiar laws of their development, which for primeval zoology and for geology has become alike equally important.

He next turned his attention—ever seizing first of all upon what was at the time most important—to the genera Terebratula, Spirifer, and Productus, which, as paleontological remains, are found so extremely abundant in all formations, but which of all this division of the Brachiopods, formerly so numerous in species, are represented in the present seas only by a few species of the first named genus. Also here he discovered the determinate laws of form which stand in the most intimate relations to the mode of life of those remarkable animals. In a similar manner he later elucidated the Cystidea—a remarkable division of the Radiata—while earlier he had already described, in a splendid work, the fossils collected in America by Alexander von Humboldt and Ch. Degenhard, in these cases animating by his genius those long extinct forms of a primeval world as if they were still sporting amid the living.

The study of Organic Remains, which has given to geology an entirely new direction, also led him, who first introduced the conception of characteristic fossils for formations, to the more precise study of the strata in which they occur. In an extensive work he showed the nature and extent of the Jura formation in Germany. In this he proved that its strata had been deposited around a primeval continent, and by this pointed out the former division

of land and water in this part of the world at that time. He also subsequently showed, that the depositions at the period of the chalk—at least its organisms—are limited to a determinate zone of the earth's surface, extending, neither in the old world nor the new, beyond 60° of north or south latitude, which, if confirmed, will be the oldest proof of a division of zones upon our planet.

Last of all, the deposition and distribution of the brown coal formation in Germany as well also as that of the chalk of North America engaged his attention. With the investigation of the former were connected peculiar studies of a botanic nature which had also occupied him for many years, but which he had now so far perfected as to bring their result to bear upon the subject in question. The fossil impression of dicotyledonous leaves, whose exact determination is often extremely difficult, led him to the study of the living forms of leaves. He rested seldom under the shadow of a tree without accurately observing the structure of its leaves and numbering their nerves. He collected hundreds in a small herbarium and by continued comparison succeeded in discovering a determinate law in the arrangement of their nerves, according to which all leaves arrange themselves under the four divisions of "Randläufer, Bogenläufer, Spitzläufer, and Saumläufer."

Here permit me to leave the succession of weightier and in part more brilliant discoveries, for which the physical sciences, and especially geology, whose reformer he was, are indebted to him, to return once more to the personal, where I may not and cannot avoid a more subjective representation, inasmuch as I have had the fortune of not being a stranger to him, and in many ways know his high manly worth.

You all may have heard doubtless of many a peculiarity of L. v. Buch, who at times under a stern exterior, always, however, bore a deeply sensitive and noble heart. Great men are seldom without sharply defined and deeply stamped peculiarities, and these then belong obviously to the full completion of the portrait.

The custom of performing all his journeys as far as possible on foot, without guide, without knapsack, in black dress coat, and round hat, in shoes and (formerly silk) stockings, all of which articles of dress being often from the hardships of the journey far more jaded than their bearer, brought him many a time in peculiar conflict with travellers, police authorities and landlords, from which, of course, by intellectual superiority and a good passport, he ever came off at last victorious. Hundreds of original anecdotes which have happened to him on his journeys are known. He himself appeared not unwilling even to relate them, and much as he was accustomed also to be importuned by such misunderstandings, still one can scarcely believe that he always shunned them.

He never communicated in advance, when and where he should travel; and even as he began his extensive journey to the Canary Islands, no one in Berlin had the least intimation of it before his departure. The mechanician who had to construct his barometer, could only conclude that he meant to ascend to high altitudes, as it must be arranged for the determination of heights of 14,000 feet.

I believe myself not to be inexperienced in wandering on foot, but I must however acknowledge, that I was right glad late one evening to have reached the terminus as we once sixteen years ago, wandered thirty-five miles over the mountains from Schandau to Tharand. We halted for refreshment but once on the way, at a spring whose waters we quaffed from the goblet of Diogenes. Instead of stopping at an inn to rest, he halted with pleasure but once in a beautiful spot in the freedom of nature, but even there not without investigating a stone or a leaf in the meanwhile. At such a time he once said, "If we contemplate with attention any one subject of Nature, we can always find something new in it, should it have been investigated and described ever so often."

In this way he generally travelled, seeking however of course at evening as good an inn as possible, with whose signs and characters in the greatest part of Europe there was scarely any one so well acquainted as he.

If, when in a distant land, winter surprised him, then indeed the way home on foot was no more practicable. To travel with strangers however, in a stage-coach, was to him, on account of the possibility of coming in contact with a smoker, fundamentally out of the question. He therefore, before railroads were known, used in every case to purchase his own waggon and with extra post horses return home. But now as he did not possess the gift of selling these again in Berlin, whole collections of all sorts of travelling vehicles here collected on his hands, until at length some relative resolved upon selling them for him. But enough of these peculiarities, which easily could be communicated in much greater numbers. They often form, however, only the original exterior of one of the noblest hearts.

Unmarried as he always was, and notwithstanding his being ever on journeys, L. v. Buch made use for himself of not the half of his large income. Believe not however in the least, that he hoarded up or collected the other half! He collected only the medals of creation, none stamped by the hand of man. He supported what appeared to him worthy of support, with a lavish hand, and that too without having it easily remarked. Not alone in the cause of science, but also in the purest philanthropy, he expended, doubtless, thousands yearly. I myself have seen him moved to tears at the misfortune of another, and I know the satisfaction of

having witnessed it without being able to prize it highly enough. He was accustomed to say at such a time, "He must be helped," and he was helped, by an unseen hand helped.

Perhaps it may be said by one or another, that it were quite easy for a man so independently circumstanced as he, to devote himself exclusively to science, and with so many means to have accomplished great results. With such, however, I cannot agree. Birth and wealth had opened to L. v. Buch many an easy way that would have led to a pleasant, yes, even a brilliant life. Hundreds, I fear, who, struggling with necessity, have earned for themselves a name in science, would, in L. v. Buch's circumstances, have chosen a path in life leading more directly and easily to commanding influence.

The spur of necessity is with many not a small one. But for a man of fortune without such extreme urging, to devote his whole life voluntarily to earnest, pure investigation, and only for that end, there is something in it, as it appears to me, of the great, something of the uncommon; for it is one thing to cultivate a branch of science incidentally, for pastime or amusement, or to acquire a certain credit for erudition, and quite another to resign oneself entirely and undividedly to it.

With such zeal for scientific advancement he also knew well how to draw forth youthful talent wherever he found it, to captivate it, lead it into a fitting path, support it by counsel or assistance, and with such delicacy of feeling and manner, that it itself scarcely perceived how much it was indebted to its patron.

He never accepted a public office, but bore occasionally on festive occasions the key of a "Kammerherr" and many a high order of merit.

I may also not pass by without mentioning the uncommonly varied character of von Buch's attainments, and the retentiveness of his memory even for trifles; it was his custom to note in his small day-book, often embracing the wanderings of many years, only brief remarks in a microscopic hand. Perfectly at home in five or six languages, he was also deeply read in history and literature. Even trivial family circumstances and town occurrences his memory retained in all their details, and he knew how to rehearse them in the most felicitous manner. His conversation was on that account not less spirited than fascinating, and he could, when he was in the right humor, enliven even the gayest saloon in the highest degree.

Now, however, one word as to his death. On Saturday the 26th February, he was till late at the Humanitätsgesellschaft—a conversational meeting of literati of Berlin. Professors Poggendorf and Braun accompanied him thence to his dwelling. At the door he bade them adieu with some jokes as usual. Upon retiring to rest he felt himself slightly indisposed. The next day began

the malady with violent pains in the foot, in which for many years he had suffered from chilblains. He did not afterwards leave his couch, and a letter from Dresden remained unread. On Tuesday the physician was called. The pains had left him, but a general debility, a nervous excited condition, had taken their place. His acquaintances, however, still knew nothing of his sickness. On Wednesday, Prof. Beyrich accidentally visiting him, received for the first time information of the sufferings of the highly honored man. He found him in bed, but cheerful, and joking in his wonted manner, alluding often to the task he had undertaken on the chalk formation of North America, and which had engaged his attention for some time past. Upon his writing table lay the beginning of his work with the superscription, "Nebraska," but under this however, were only two lines, probably written on Saturday. On the same evening, Prof. Beyrich carried the intelligence of his illness to the meeting of the German Geological Society, of which von Buch was president. During the night of Thursday his condition became much worse. Debility and fever had visibly increased. However, on Thursday he could still converse with most of those who visited him during the day. When Prof. Beyrich visited him again at 10 o'clock on Friday, March 4th, he found Messrs. Ewald, Braun, and Papiz already at the couch where he lay unconscious, and they did not again leave it till his death, which occurred at twenty minutes before two o'clock.

On March 9th the funeral solemnities took place in the dwelling of the departed, which the Royal Botanic Garden had richly decorated with palms and laurel. His mortal remains were then transported to the family vault at Stolpe.

You thus have a few fleeting sketches of the life and death of the man to whose memory we are here convened to pay our homage, and of whom we are proud, not only as Germans, but also as citizens of Freiberg. Yes, we are something more than proud of him, we are much his debtors. He has never forgotten his Freiberg. Each of his works—of which all never came into the hands of the booksellers—is to be found from his own hand in the Library of the Academy, and besides this also in many of the private libraries of the place. Before the Kreuzthore stands as an ornament of the city, Werner's bust, (a present from Count Einsiedel,) but for whose erection and embellishments we are indebted to von Buch's liberality. He often came here, and only three years since when we celebrated the memory of his distinguished master, he was to our joy with us in this hall, where to-day his portrait presents to us only the noble lineaments which we shall never forget, as his whole service must forever remain unforgotten.

An account of the origin of this portrait may not be without interest. L. v. Buch had often received urgent requests to allow

his portrait to be taken, but had never consented. When his friend Friesleben on the remittance of his own portrait, once urgently requested his in return, he received in its stead a large lithograph of an Ammonite with the signature under it, "Leopold v. Buch." There was little hope then, of obtaining a portrait of the great geologist, aside from the imperfect failure in the Dictionnaire des Sciences Naturelles. Some years since, however, his King sent the celebrated portrait painter, C. Begus, to him, and told him, that he, the King wished his portrait. What remained for him to do? He must obey and sit still. From that portrait is this lithograph a copy.

And now permit me to address myself to you who are dedicacating yourself to the same studies which here once lured this distinguished man. If you all however have not proposed to yourselves the same course—if you are not all called to furnish similar results in your particular departments like a L. v. Buch, still may you ever take him as an exemplar. His example like that of Alexander v. Humboldt, and many others, teaches at the same time that even from an unpretending place of study great effects may go forth. May we all strive to imitate him in untiring zeal, system, and noble sentiments—this will be the highest honor we can show to his memory. For the immortality of his name, he himself has provided.

At the close of the exercises, the band of the cavalry regiment, in garrison here, played a Dead March.

Art. II.—*Extracts from the Report on the Geology of the Lake Superior Land District;* (Part II.) by J. W. Foster, and J. D. Whitney.*

In Part I, of this Report, communicated to the Commissioner of the General Land Office in 1850, and published in 1851, we have given a historical sketch of the exploration of the country bordering on Lake Superior, a description of its physical geography and climate, and so much of its geology as was necessary to the full elucidation of the copper-bearing rocks and their relation to the sedimentary formations; this being the subject to which that part of the report was principally devoted. The two concluding chapters contained an account of the drift phenomena so conspicuously displayed in the region of the great lakes.

In Part II, of this Report, we shall proceed to the detailed and systematic description, so far as our materials will enable us, of

* The appearance of this valuable Report, was announced in volume xv, page 295. We here cite some paragraphs from the chapters, on the general characters of the rocks, and on the Azoic and the lower Silurian systems.

the geology of the whole of the Lake Superior Land District, commencing with those formations which are the lowest in the scale of geological succession, or those which were first formed, and ascending to those which are now in the progress of accumulation. We shall only allude to the results of the former part of the report, so far as it may be necessary to enable the reader to form a connected idea of the geology of the whole region.

The following table exhibits the names and the order of succession of the geological groups which have been recognized as existing within the limits of our district.

Classification of the Rocks.

FORMATIONS.	IGNEOUS.	*Of Various Ages.*	Plutonic Rocks.	Granite. Syenite. Feldspar and Quartz rock.
			Trappean, or Volcanic Rocks.	Greenstone, or Dolerite, Porphyry. Basalt, Amygdaloid. Hornblende and Serpentine Rocks. Masses of Specular and Magnetic Oxyd of Iron.
	METAMORPHIC.	*Azoic System.*		Gneiss, Mica and Hornblende Slate. Chlorite, Talcose and Argillaceous Slate. Beds of Quartz and Saccharoidal Marble.
	AQUEOUS.	*Silurian System.*	Lower.	Potsdam Sandstone. Calciferous Sandstone. Chazy Limestone. Birds-eye Limestone. Black-River Limestone. Trenton Limestone. Galena Limestone. Hudson-River Group.
			Upper.	Clinton Group. Niagara Group. Onondaga Salt Group.
		Devonian System.		Upper Helderberg Series.
		Drift System.		Beds of Sand, Clay and Gravel rudely stratified. Transported Blocks of Granite, Greenstone, &c.
		Alluvial Deposits.		Sand and Pebble Beaches, Marshes, Flats, Hooks, Spits, Dunes, &c.

The New York geologists have divided the Silurian system, as developed in that state, into eleven groups, while some of the Western geologists recognize, in its western extension, but five. Between the two systems of classification there is no community of names.

The geographical position of our district is such as to form a connecting link between the east and the west. While, on the

one hand, the New York and Canadian geologists have traced the Silurian groups up to the eastern borders of our district; on the other hand, the geologists of Michigan, Iowa, Wisconsin and other states, have traced the same groups, though under different names, along their southern and western prolongation, without having attempted to identify them with their eastern equivalents, or to subdivide them according to the palæontological evidence.

Under these circumstances, we have endeavored to connect the two sets of observations and blend them into one harmonious whole. As the New York survey is the only instance in which the matured results have been communicated to the public, and as the volumes on the palæontology—a monument of the research and perseverance of the author, Mr. Hall—will form the standard of reference for the whole country, in determining the succession of the Silurian groups, we have deemed it advisable to adopt that nomenclature, so far as the same groups described by the New York geologists could be recognized in our district.

The designation of groups of strata by names derived from their geographical position, or from the locality in which the rocks are first investigated and their relative position clearly defined, seems to be of all the methods of nomenclature that which, for the present at least, is liable to the least objection. Names given solely with reference to lithological character, or to the presumed predominance at any particular point of a certain genus or class of organic remains, seem much more likely to lead to misunderstanding and confusion; and, however desirable it may be that a universal system of nomenclature and arrangement should be introduced, it seems quite impossible to hope for any such thing in the present state of geological science, a science which is so rapidly developing, and liable to such constant changes. The names introduced by the New York geologists, are in most instances derived from the locality where the group designated is particularly well developed, and the fact that those groups have, in their continuation through Canada, been described by Mr. Logan, the Provincial geologist, under the names recognized by the New York survey, seems an additional reason for their adoption, as far as possible, by us.

It will be seen from the details incorporated in a subsequent part of this report, that many members of the Silurian series, particularly the grits and conglomerates, which are clearly defined in New York, have but a limited range, and disappear altogether before reaching the limits of our district. These are conditions which we ought to expect would exist in deposits made along a shelving ocean-shore; but so far as these are persistent, it seems desirable that they should bear the same names throughout their whole extent.

The following is the synonymy of the groups of the systems developed in this region, according to the nomenclatures adopted in the reports of the different surveys.

New York and Lake Superior.	*Pennsylvania and Virginia.*	*Ohio, Iowa and Wisconsin.*
	AZOIC SYSTEM.	
AZOIC SYSTEM. (Not classified in New York.)	AZOIC SCHISTOSE SERIES.	METAMORPHIC ROCKS. (Wanting in Ohio and Iowa.)
	SILURIAN SYSTEM.	
Potsdam Sandstone.	Primeval Sandstone, or Formation No. 1.	Lower Sandstone, or Formation 1, wanting in Ohio.
Calciferous Sandstone.	Lower part of the Matinal Series, or part of No. II.	Lower Magnesian Limestone, Formation 2, wanting in Ohio.
Trenton Group (including Chazy, Birds-eye, and Black-river Limestone.)	Middle part of Matinal Series, or part of No. II.	Fossiliferous Limestone, No. 3. Blue Limestone and Marls of the West.
Galena Limestone (not recognized in New York.)	Not recognized in Pennsylvania and Virginia.	Erroneously regarded as the equivalent of the Cliff or upper Magnesian limestone.
Hudson-river Group.	Matinal Shales, or No. III.	Associated with No. 3, or the Blue Limestone and Marls of Ohio.
Medina Sandstone and Clinton Group.	Part of the Levant Series, or part of No. V.	Not recognized at the West.
Niagara Group.	Part of the Levant Series, or part of No. V.	Cliff limestone of Ohio and Indiana. Upper Magnesian Limestone.
Onondaga Salt Group.	Summit of the Levant Series.	
	DEVONIAN SYSTEM.	
Upper Helderberg Limestone.		Upper portion of the Cliff Limestone.

* * * * *

Azoic Series on the Northern Shore.—The rocks of which it is composed are developed on an extensive scale, both on the northern and southern margin of Lake Superior basin. Commencing on the northern shore of the lake, we find a series of talcose and chlorite slates with occasional beds of coarser grits, in immediate contact with the granite and gneiss. They have been divided by Mr. Logan, the distinguished Provincial Geologist of Canada, into two groups—a division which we have failed to recognize on the southern shore—the lowest of which consists of slates partially chloritic and talcose, and occasionally holding a sufficient number of pebbles derived from the hypogene rocks to constitute conglomerates. "These slates," he remarks, "are of a dark-green color, often dark-grey in fresh fractures, which at the base, appear to be occasionally interstratified with beds of a feldspathic quality, of the reddish color belonging to the subjacent granite and gneiss: sometimes they are a combination of feldspar and quartz, occasion-

ally with the addition of hornblende, making syenitic beds, and in some the hornblende predominating gives the syenite a general green tinge. Some of the beds have the quality of a greenstone, others that of mica slate, and a few present the character of a quartz rock."* These slates, he conjectures, attain a thickness of several thousand feet, and are well exposed at the mouth of the river Doré, about five miles from the Michipicoten river. The strike of the beds is very irregular and their dip highly inclined.

The upper group rests *unconformably* on the preceding, and towards the base presents conglomerate beds of no great thickness, the pebbles of which consist of white quartz, red jasper, and occasionally slate, the whole enclosed in an arenaceous matrix. Higher up are found layers of chert, occasionally approaching chalcedony. The plates are separated by thin calcareous seams, presenting a ribbon-like appearance.

In the vicinity of the disturbed parts, the chert sometimes passes into chalcedony and agate, and small cracks are filled with anthracite, which is also found forming the centre of minute globules, enclosed in a silicious matrix.

Higher up in the series, the argillaceous slates become interstratified with argillaceous sandstones, in such an altered condition that it is often difficult at first sight, to say whether the latter may not be trap layers. Calcareous bands occasionally occur of sufficient purity to be called limestone. Interlaminated masses of trap are found near the base and overlying the summits. They are composed of a black hornblende and greenish-white, horny-looking feldspar, in no instance assuming an amygdaloidal character, but occasionally presenting a porphyritic appearance. It exhibits a sub-columnar structure, and the crowning overflow of trap communicates a peculiar aspect to the whole region occupied by this formation. It attains a thickness of nearly 2000 feet, and, where it comes to the lake, rises in bold, overhanging cliffs.†

*   *   *   *   *

Azoic Series on the Southern Shore.—In the region included between the two great lakes, known as the northern peninsula of Michigan, this group constitutes the fundamental rock. The materials of which it is composed appear to have been thrown down in a comminuted state, since we rarely meet with those grits or conglomerates which occur on the north shore. They constitute alternating beds of great thickness, known as gneiss, hornblende, chlorite, argillaceous, silicious and talcose slates, quartz, saccharoidal, and crystalline limestones. They are highly inclined and much contorted, and nowhere exhibit the characters of a purely sedimentary rock; but the evidences of metamorphism are more striking as we approach the lines of igneous outburst.

* Report of Progress, 1846–7, p. 10.

† Ibid. pp. 13–14.

Gneiss generally flanks the granite, succeeded by dark masses of hornblende, with numerous joints, but obscure lines of bedding, which often graduates into hornblende slate or chlorite slate, as we recede from the purely igneous products.

The outlines of this class of rocks are extremely irregular, and a reference to the general map will give a clearer idea of their range and extent than a mere verbal description. * * *

In this district, the area occupied by these rocks exceeds eighty townships, or more than three thousand square miles. The configuration of the slates and granites may be compared to the contours of a rugged coast. The main granite masses form numerous projecting headlands, while the subordinate patches rise up like islands. The slates sweep round the promontories and form numerous narrow and deeply-indented bays.

The topographical features of the region occupied by the slates are striking. It is diversified by bold, rocky cliffs and narrow intricate valleys, with lakes and water-falls, with luxuriant forests and natural meadows. The culminating points reach nearly twelve hundred feet above Lake Superior or eighteen hundred feet above the ocean-level.

We commence our description of the local phenomena of this system of rocks where they intersect the lake shore between Riviere du Mort* and Chocolate river. The sketch entitled, "View near Carp river" forming the frontispiece [see Report], may serve to convey an idea of the contours of this region better than a written description. The lake here forms a spacious bay with gently curving shores. A range of quartzose hills rising to the height of six hundred feet, terminates abruptly by the coast and forms the background of the picture. The extremity of the point consists of sand-dunes rising to the height of fifty or sixty feet, with rounded outlines and highly inclined slopes towards the lake. Along its margin are to be seen the remains of ancient terraces which indicate its former limits. The middle ground is occupied by a range of trappean rocks interlaminated with the slates. The settlement represented, has been named Marquette, in honor of the early missionary, and has already become the main outlet of the Iron region. The foreground is composed of another rocky promontory which projects for some distance into the lake, and serves as a shelter to vessels against a west and northwest wind; while by the shore, vestiges of the Silurian sandstone are seen reposing upon the upturned edges of the azoic rocks. It would be difficult to select another spot, along the whole coast, where the

* This river is generally called *Dead* river. It cannot be from the sluggishness of its current, for in the distance of thirty miles, it falls more than a thousand feet, abounding in rapids and cascades. Its true name is the River of Death. There is a local tradition as to some act of violence here perpetrated, which we cannot now recall, aud from which the river derives its name.

rocks of so many epochs, from the oldest to the most recent, are represented. It contains an epitome of nearly the whole geology of the district.

The quartz zone exhibits two distinct ridges, where it approaches the lake, hemming in the valley of Carp river with rocky walls, from two to six hundred feet in height. As we trace it westwardly, it presents but a single ridge, and after having passed Teal lake, sinks down and becomes lost. Where exposed by the lake shore, it exhibits lines of bedding and obscure traces of ripple marks. These lines bear east and west, and dip 86° to the south, while the Potsdam sandstone abuts against the quartz, in a nearly horizontal position. Some of the quartz beds in this vicinity enclose fragments of jasper and slate, showing that they contain vestiges of prior-formed rocks. The southern ridge—using the notes of Mr. Hill—presents a number of conical knobs rising from two to three hundred feet above the surrounding country. In section 2, township 47, range 25, a granite boss rises above the quartz, over which the strata are folded like a mantle. In the northeast quarter of this section, a band of slaty limestone, somewhat silicious, is seen beneath the quartz, bearing northeast and southwest, with an inclination of 44° to the southeast. In the northern part of section 3, the quartz is observed, with another band of limestone interstratified, bearing nearly west-northwest. The protrusion of the granite has displaced the beds and broken their continuity; one portion shifted to the south, was traced as far as the line between sections 9 and 10, while another portion shifted to the north, was traced westward into section 4. The northern beds were found to be associated with a layer of limestone, or compact marble, only a few feet in thickness. The northern ridge attains a higher elevation than the southern, the highest point in section 6, being five hundred and ninety-two feet above the lake. The quartz has been so far metamorphosed as to destroy the lines of bedding, but in other portions of the range for instance, near the Jackson Forge, it assumes the character of a quartzose conglomerate, and exhibits distinct lines of bedding. A granite protrusion occurs along the line between townships 47 and 48, range 25, which has caused a displacement of the strata, many thousands of feet in a linear direction, throwing one portion of the ridge to the southwest, and another portion to the north; and it is instructive to observe how far the course of the Carp river has been determined by this dislocation. The main branch curves around the southern outlier, while the affluent, known as the Alder creek, finds its way between the granite and the quartz.

In section 31, township 48, range 25, near the west line, another band of compact limestone was observed, and thence traced westward, by Mr. Hill, through sections 33 and 36, being well exposed in the escarpments of the ridges. It is less silicious than that be-

fore described, variously colored, white, ash-grey and flesh-red, and beautifully veined with tints of a deeper hue. It calcines readily into lime, and affords beautiful ornamental materials.

Along the valley of the Carp, between Jackson Forge and Teal lake, beds of novaculite, or fine-grained silicious slate, are found interstratified with beds of quartz. It has been already quarried at several points, for hones, and there is, even now, a considerable demand for them. The beds are exceedingly fissile, and full of flaws at the surface, so that much of the mass is comparatively worthless; but it is believed that the blocks taken from a greater depth, and beyond the action of atmospheric agents, will be free from these imperfections. Messrs. Smith and Pratt have established a factory for the purpose of sawing these blocks, at the mouth of a small stream, near the Marquette landing, and are driving a thrifty business.

Between the quartz range and Dead river, the underlying rock consists, in the main, of chlorite and talcose slates, intersected by three belts of igneous rocks, ranging nearly east and west.

De la Beche, in reference to the greenstones and schistose rocks of Bossiney, Cornwall, remarks that, "there is so intimate a mixture of compact and schistose trappean rocks with the argillaceous that the whole may be regarded as one system, the two kinds of trappean rocks having been probably erupted, one in a state of igneous fusion, and the other in that of an ash, during the time that the mud, now forming slates, was deposited; the mixture being irregular from the irregular action of the respective causes which produced them; so that one may have been derived from igneous action, and the other from the ordinary abrasion of preëxisting solid rocks, and they were geologically contemporáneous."

This description is applicable to many of the igneous rocks of this region. They form neither long lines of dykes, nor axes of elevation, but broad sheets bearing the same relation to the slates that the trappean bands of Keeweenaw Point do to the conglomerates. Many of the slates appear to be composed of pulverulent greenstone, as though they might originally have been ejected as an ash, and subsequently deposited as a sediment, and they pass by imperceptible gradations, from a highly fissile to a highly compact state. In some places, for example, at Marquette and Presqu'isle, they assume a spheroidal structure, as though after their deposition they had been so far operated upon by heat as to allow a reärrangement of the particles. Along the lines of volcanic action, we frequently find, throughout this district, a green pulverulent substance, somewhat resembling chlorite, and containing a large amount of magnesia and lime, which is probably in the nature of an ash. The same ingredients enter largely into the composition of many of the trappean rocks, for they possess a soapy feel, and when reduced to a powder, effervesce feebly with acids.

There are undoubtedly, at this day, beneath the bed of the ocean, numerous "salses," which, from time to time, pour forth streams of pulverulent materials, but whose operation is concealed from human sight. We know that for weeks in succession, there flowed streams of chocolate-colored mud from the crater of Graham's island, before it finally sank below the surface. The contributions from this source, to the first formed stratified deposits, had not been duly appreciated. This volcanic mud is nothing more than the comminuted particles of trappean rocks, reduced by friction; and in the early history of our planet, when the fissures communicating with the interior were unfilled and volcanic energy was manifested more intensely than at this day, it would be reasonable to expect that igneous causes contributed as powerfully to the reduction of the preëxisting rocks, as the ordinary abrading action of water. The slates are composed essentially of the same ingredients as the trappean rocks with which they are associated, and the main difference between them may be, that the one was the product of salses, ejected in the form of mud, while the other was the product of volcanoes, ejected in molten streams. It has been supposed that the talcose nature of the slates associated with the igneous rocks was the result of metamorphism, but the supposition that they have resulted in some instances from the destruction of the latter, is quite as reasonable.

About a mile from the lake shore, on the road leading to the Jackson Forge, a low range of trappean rocks, of a compact texture, and of a dark-green color, is intersected. Between the Jackson and Marquette landings, by the lake shore, a similar belt is observed, which again appears near the junction of the roads, about four miles inland.

Another belt intersects the coast a short distance above the Marquette landing, which does not differ essentially from those before described. The slates in the vicinity of these belts are compact, of a greenish color, and traversed by different systems of joints, more distinct than the lines of bedding, cutting the mass into cuboidal blocks.

At Little Presqu'isle, another band of igneous rocks, of a highly crystalline character, projects into the lake. Distinct acicular crystals of hornblende are distributed in places through a paste of pure-white feldspar, while in others, these two minerals are disconnected, the latter forming beds of considerable thickness. Angular fragments of hornblende slate, chlorite slate; jasper and a green magnesian mineral, are seen enclosed in the mass near the water's edge which may be regarded as a volcanic breccia. These fragments seldom exceed a few inches in diameter.

Like most of the rocks of this region, its surface is smoothed and striated in a wonderful manner. Below the mouth of Dead

river, a highly crystalline mass of this character emerges in the form of an island fifty or sixty feet in height.

The main Presqu'isle consists of a dark-green trappean rock, rising in the overhanging cliffs to the height of a hundred feet. A description of this rock and the relations which it bears to the sandstone will be given when we come to treat of the Silurian system. Over this is deposited a volcanic tuff, imperfectly stratified, filling up the previous depressions, and attaining a thickness of twenty or thirty feet. It presents a complete net-work of veins, a few lines only in width, which penetrate but a short distance into the subjacent basalt. At one place, on the northwest side of the point an irregular vein bearing north and south is seen for two hundred feet in a linear direction, in this obscurely stratified tuff, which yields the sulphurets of lead, copper and iron, but not in sufficient quantities to render its exploration profitable. Asbestus is also sparingly distributed, and may be regarded as a metamorphic product resulting from the presence of lime. Traces of magnetic oxyd of iron are detected in some of the veins farther eastward.

Proceeding up the valley of Dead River, between sections 7 and 18, township 48, range 25, the stream is precipitated from a height of twenty feet over a ledge of schistose rocks, which exhibit distinct lines of bedding and abrupt convolutions of the strata.

In the next range west (27) the trappean and schistose rocks are frequently exposed in the bed of the stream, consisting of alternations of talcose and chlorite slates, and hornblende and feldspar rocks. They stretch out in numerous parallel ridges, bearing north of east and south of west, and present, for the most part, southerly escarpments. On the northwest quarter of section 16, the river is precipitated in a series of rapids over the former class of rocks, affording fine exposures for observation. On the west boundary of section 6, in a high ledge which rises from the northern bank of the stream, the slates are again observed dipping to the south at an angle of 70°.

The stream here bears west-northwest, conforming to the direction of the strata. After flowing along the northern line of township 48, nearly through range 27, it divides into numerous branches whose sources lie to the northwest, in the region of the granite.

Proceeding southward from Teal lake, we first encounter a ridge of trappean rocks which skirt its southern shore and rise abruptly to the height of two hundred feet above the lake-level, succeeded by chlorite slates and vast masses of specular and magnetic oxyd of iron. As we shall devote a special chapter to the character of these masses and their relations to the associated rocks, a more minute description is here deemed unnecessary.

We would merely observe that in this region the iron masses are invariably found in this association—never occurring in the granite.

These alternations of trappean and schistose rocks continue, to near the southern boundary of township 47, and are characterized in many places by the ores above described.

Section from Lake Superior to Lake Michigan.—The coast near the head of Keweenaw Bay (L'Anse) affords an admirable section of the slates and the overlying sandstone.

The following is the descending order of succession :

1 and 2. Fissile sandstone—the equivalent of the Potsdam—dipping slightly to the west-northwest, of a reddish color, and coarse-grained, passing into a conglomerate composed of pebbles of milk-white quartz, and occasionally trappean pebbles—13 feet, resting *unconformably* on the azoic rocks, consisting of.

3. Chlorite slate and novaculite, or silicious slate, variously colored, and much contorted—in places folded over.

4. A dark hornblende and feldspar rock evidently trappean in its origin.

Formations 3 and 4 are traversed by veins of quartz which in no case penetrate the overlying sandstone. The slates are also occasionally intersected by dykes of trap.

This section is exceedingly instructive, inasmuch as it enables us to draw a line of demarcation between two formations different in age and external characters. While the newer formation—the Potsdam sandstone—is but slightly if at all disturbed and little changed by metamorphism, the older, or azoic slates, are contorted and folded into numerous arches, and in several places, invaded by igneous rocks. Their structure has been changed from granular to sub-crystalline, and the whole mass is intersected by numerous planes of lamination.

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Thickness of this System. Foldings of the Strata.—All attempts to estimate the thickness of the various schistose, calcareous and quartzose beds, must prove merely approximative. They occupy a belt which in its widest expansion reaches not less than eighty miles in width, and wherever exposed, have an inclination approaching verticality. If we were to deduct the spaces occupied by the purely igneous rocks, and then measure across the basset edges of those supposed to be sedimentary, the result would give us an incredible thickness—a thickness far surpassing that of the whole series of rocks heretofore observed, from the base of the Silurian to the crowning member of the tertiary. It is highly probable that the beds are arranged in a series of flexures, and that the observer in passing over the outcropping edges, beholds numerous repetitions of the same beds; they have, however, been so repeatedly shattered by earthquakes,

so disturbed and forced asunder by igneous protrusions, and so metamorphosed by direct and transmitted heat, that it is impossible to trace their continuity except over limited areas. If we could unfold these beds, and stretch them out in a nearly horizontal position, as when first deposited, they would require a far greater space than they now occupy. The causes by which these foldings have been effected, will be discussed in a subsequent chapter.

We have thus described the range, extent and mineral peculiarities of a series of rocks, detrital in their origin, interposed between the granite and the base of the Silurian system. Throughout their whole extent, they are more or less metamorphosed, presenting a series of gradations, represented at one extreme by crystalline gneiss and compact hornblende, and at the other by bedded limestone and ripple-marked quartz. To the presence of granite and trappean rocks this transformation is, in a great degree, to be attributed. Much of the compact hornblende presents the external characters of an igneous product; but, since it is found to occupy an almost invariable relation to the granite axis—flanking their slopes—and to assume a fissile structure as it recedes from the lines of igneous outburst, we cannot but regard it as the more highly metamorphosed portions of the dark-green chlorite slates. This compact hornblende is not to be confounded with those lenticular-shaped masses observed in the slates, which, we doubt not, are trappean in their nature.

We have seen that those igneous causes which produced numerous axes of elevation, and folded the strata into a series of flexures, had ceased to operate before the deposition of the Silurian groups, since they are found to repose in a nearly horizontal position upon the upturned edges of the slates, or to occupy the sinuosities in the granite, nowhere exhibiting traces of metamorphism or derangement of the strata. We do not now allude to the renewal of those igneous causes as manifested on Keweenaw Point and Isle Royale during the Silurian epoch, producing a class of igneous products widely different from those associated with the rocks of the azoic system. In a former report (Part I.) we have described the igneous rocks of the Silurian epoch as appearing under a variety of aspects, such as crystalline greenstone, porphyry, granular trap, and a highly cellular amygdaloid, differing little from modern lava, except that the cells are filled with various zeolitic minerals.

From the local details above given, it will be seen that the igneous rocks of the azoic period, though crystalline, compact, and occasionally porphyritic in their texture, are never amygdaloidal; and hence we infer that they were produced under widely different conditions. The latter may have been con-

solidated beneath the pressure of a deep ocean, while from the former a greater part of this pressure may have been removed; or it may be that both were, in the first instance, equally vesicular, but that the latter assumed a crystalline or compact structure from long-continued exposure to heat, under immense pressure. All the phenomena would seem to indicate that the eruption of the trappean rocks of this period took place beneath an ocean of great depth; or, at least, under conditions widely different from those which prevailed during the formation of the trappean belts of Keweenaw Point and Isle Royale.

Remarks.—The investigations of geologists in different parts of the world, within the last few years, have clearly demonstrated the existance of a series of non-fossiliferous rocks below the Silurian or Cambrian systems, and there can be no doubt that they are destined to occupy a conspicuous place in the classification of the rocks both of Europe and America. At the meeting of the American Association at Cincinnati, in the spring of 1851, we made the development of this system in the northern portion of the United States and Canada the special subject of a communication. Professor Mather, after having confirmed the accuracy of our views, from personal observation, stated that he had observed the continuation of this system near the sources of the Mississippi, and on the waters of the St. Peter's. Its existence in Missouri, where it is associated, as on Lake Superior, with immense beds of magnetic, and specular iron ore, is rendered certain by the observations of Mr. Mersh, which will be found incorporated in the subsequent pages of this report. At this meeting, Dr. King, who has examined this region with much care, confirmed these views, and we regret that the proceedings have not yet been made public, that we might quote his remarks in full.

Dr. Engelman, also, on that occasion, described a series of azoic rocks, as occurring in Arkansas, between Little Rock and the Hot Springs, which present a striking analogy with those of Missouri and Lake Superior, consisting of talcose, silicious and crystalline hornblende slates, often highly inclined, with beds of dark-blue limestone. On these older rocks, rests unconformably a sandstone, probably analogous to that of Lake Superior. Within this district of non-fossiliferous, stratified rocks, occurs a beautiful syenite. The vast masses of micaceous, or sub-magnetic oxyd of iron, which occur in Missouri, find their representative in the well known "Arkansas Magnets," or, in the iron ore of Magnet Cove. It is here associated with the interesting titaniferous minerals, schorlomite, brookite, and elæolite. Dr. E. thinks he has traced this series on the northern branches of the Colorado in Texas.

In the eastern portions of the United States, there can be no doubt of the existence of this system.

We are satisfied from personal observation that it flanks the Adirondack range in New York, where it is associated with hypersthene rocks and with masses of sub-magnetic oxyd of iron, below the Potsdam sandstone.

The Messieurs Rogers describe a series of obscurely stratified rocks in Pennsylvania and Virginia, occupying the same relative position known as the gneissoid series. They undoubtedly flank the Appalachian chain on the east, throughout their entire range, and will probably be found well developed in Tennessee and North Carolina.

In Europe the existense of this series has been established beyond controversy. It has been shown by eminent geologists, especially by Murchison and de Verneuil,* that the lowest beds in Scandinavia, containing the least traces of organic life, are the exact equivalents of the Lower Silurian strata of the British Isles, and that these have been distinctly formed out of, and rest upon, slaty and other rocks which had undergone crystallization before their particles were ground up and cemented together again to compose the earliest beds in which organic life is traceable. To this most ancient system of rocks in Scandinavia, they have given the name of azoic. By this term, they do not mean dogmatically to assert that nothing organic could have been in existence during the earliest times, when those rocks were in the process of formation, but simply to express the great fact, that, as far as our present state of knowledge goes, we look in vain for any traces of organic life, and it seems probable that they were formed under such physical conditions that nothing living could have flourished during that period.

The great mass of rock in Scandinavia is made up of a crystalline, granitic gneiss, presenting an almost infinite succession of feldspathic, quartzose, micaceous and hornblendic laminæ, which are often highly contorted, though a general strike or direction may be traced over a large tract of country. These rocks are by no means to be confounded with the metamorphic Silurian strata, occurring under a similar and analogous form in the same country. These azoic rocks are often disturbed and cut through by dykes of greenstone and traversed by countless veins of granite.

It is evident from the direct comparison of the more ancient azoic with newer metamorphic Silurian, that, from lithological characters alone, no distinction could be drawn between them, and it is only where the most conclusive evidence is afforded by superposition of the latter unconformably upon the former,

* Russia and the Ural Mountains, vol. i, p. 10.

that they can be clearly recognized and defined as belonging to different ages.

De la Beche remarks that, although alterations in the mineral character of the fossiliferous rocks, from the influence of intruded igneous matter in a molten state, or arising from other modifying causes, often produce mica slates, hornblende slates, gneiss and other forms of laminated and stratified deposits, with a peculiar aspect, there appears, nevertheless, evidence in Scandinavia and the British Isles, and also in other parts of Europe, to show that, beneath all the fossiliferous rocks, there are mica and chlorite slates, quartz rocks, crystalline limestones, gneiss, hornblende, and other rocks of earlier production. These may be, indeed, merely altered, or metamorphosed detrital and chemical deposits of earlier times, and *possibly* organic remains may eventually be discovered in them; but until this shall happen, it seems desirable to keep them asunder, for the convenience of showing previous accumulations to those known as the Cambrian group. He, therefore, proposes the name of *Mona Series* for the reception of these older rocks, which are well displayed in the island of Anglesea, in connection with those of the succeeding group.*

In the admirable and detailed examinations of the Geological Survey of Wales, where the numerous intercalated beds of trappean rocks and the complicated series of faults have rendered the task of unravelling the geology one of great difficulty, the surveyors have clearly shown the existence of this azoic series below the lowest Silurian strata, which is there represented by the sandstone of Barmouth and Harlech.

Barrande, also, in his investigations of the Bohemian basin, has recognized a series of semi-crystalline slates alternating with compact argillaceous slates, below the lowest Silurian strata, in which he has failed to detect any trace of organic life; hence he has applied the name azoic to these rocks without meaning to assert positively that the series is absolutely destitute of all traces of life, but simply as indicating the great fact that, thus far, none have been discovered.

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LOWER SILURIAN SYSTEM.

We now proceed to a description of the palæozoic series of this region. Unlike the rocks which we have hitherto described, they exhibit few traces of igneous outburst, and few displacements of the strata; but, on the other hand, they repose nearly horizontally on the basset edges of the slates, or occupy the depressions in the granite.

This general remark, however, must be received with some qualification, for we find that there existed, during a part of the

* Geological Observer, pp. 31, 32.

Silurian epoch, at least two lines of volcanic foci, from which flowed numerous streams of lava. These, mingling with the detrital deposits then in progress of accumulation, formed a mass, whose united thickness far surpasses the height of the loftiest summits in this region. We refer to the trap ranges of Keweenaw Point and Isle Royale, described in a previous report.

* * * * *

The limits of this oceanic basin are but imperfectly defined. It stretched east and west, from the shores of the Atlantic to the flanks of the Rocky mountains; to the south, it extended beyond the Rio Grand, and north, to the Arctic ocean.

Within this basin, the granite axes between Lake Superior and Lake Michigan, and between Lake Superior and Hudson's Bay, rose above the waters of the Silurian sea.

A large portion of North America is embraced within this oceanic bed, constituting its fairest and most productive tracts. The uplifting force, by which this sea-bottom was converted into land, must have been gradually applied; since the strata, for the most part, repose in a nearly horizontal position, and exhibit few marks of derangement. We meet with no mountain chains, and no transverse valleys, except such as have been excavated by running water. The whole region is spread out in gently undulating plains; or, if ridges exist, they are due to accumulations of drift, or to the greater coherence between the particles in certain groups, which enabled them to resist the general denudation, which has everywhere left such incontestible evidence of its action.

From the MS. of Mr. Hall, we append some general remarks on the identity of the members of the Silurian groups, as developed in different parts of this basin.

"The observations of the past season have served to show that nearly all the more important groups of the Silurian system extend uninterruptedly from the more easterly points, where they have been investigated, through the northern peninsula of Michigan, as far west as the Mississippi, and even beyond. These observations form a connecting link between those heretofore made in New York and Canada, and those made in the southern peninsula of Michigan, Wisconsin, Iowa, Minnesota, and other portions of the West, and enable the geologist to form a correct idea of the range, extent, and fossil contents of these groups, as developed in the northern portion of the United States. We believe that these results will render some points of resemblance, heretofore obscure, clear and distinct; and remove any doubts that may have been entertained, as to the identity of certain members of the Silurian system, in their eastern and western prolongation. If these results are attained, we shall be satisfied that we have not labored in vain.

"These strata, originally deposited in a wide ocean, everywhere present evidences of organic existence. It is not to be supposed that in tracing deposits of this kind over so wide areas, the conditions of the ocean shores would have been uniform in their physical character, and we cannot, therefore, expect uniformity in the character of the deposits themselves. The very circumstances under which the sediment was thrown down and the causes which gave origin to it, are of themselves evidence, *a priori*, that absolute uniformity could not prevail over so wide an area. We are to look, in like manner, for corresponding changes in organic remains. It cannot be supposed that animals, possessing certain characters and habits of life, would continue to live for any length of time, when the physical conditions were unfavorable to their existence. When we consider, also, the extent over which these deposits have been traced, the difference in longitude alone would lead us to expect some differences in the fauna of this ancient period. We have only to compare this great linear development of the palæozoic strata with an equal extent of modern coast, to form some idea of the changes that might be expected to occur under similar circumstances. In making such a comparison, however, we shall find that the actual condition of these ancient deposits is far more uniform than that of the sea-shore, or sea-bottom, at the present day.

"By making the preliminary examination of recent shores, or of recent geological deposits, we are far better prepared to appreciate and understand the changes which will be observed in the rocks themselves. These changes, though gradual, and readily understood, when studied continuously, are, nevertheless, difficult of explanation, when seen at wide intervals, or examined at distant localities.

"The general results of these examinations, as will be seen by the local details, have shown an increasing thickness in the Potsdam sandstone, in its western extension, though, at some of the intermediate points, it is much thinner than in New York. It should be recollected, however, that these beds were deposited upon the uneven surface of preëxisting rocks, and their entire thickness may, therefore, be exposed only at some of the points where the originally unequal floor presents some of its elevations, and thus give an erroneous idea of its actual, entire thickness. The calciferous sandstone, in like manner, appears to increase as traced from the eastern limits of the district westward, and on the Mississippi, attains a thickness equal to that which it has in New York.

"From all of the evidence, it would appear that these two groups which are very intimately related to each other, have their most extreme tennity somewhere near the northern boundary of the great arch, formed by the circuits of the older formations

around the northern shores of lakes Huron and Michigan. From this point, they increase in thickness as traced to the east and to the west. On the other hand, the lower limestones—the Chazy, Birds-eye, Black river, and Trenton groups—appear to decrease gradually in thickness, as traced westwardly from the Mohawk valley, and the outlet of Lake Ontario, where they exist in great force. This fact is made very conspicuous, when their entire thickness, as developed in New York, is compared with that of the same groups on the northern side of Lakes Huron and Michigan, where they contract to within a hundred feet, and in some places even less. We have also evidence that they do not, like the preceding groups, increase in thickness, when traced into Wisconsin and across the Mississippi; for there, they hardly attain a vertical range of fifty feet. Although over the entire area the identity of character and the continuity of the beds are maintained, it is clear that the materials have continually undergone diminution, and that the formation, unless it has been supplied from other sources, will be found to die out.

"In the upper member of the Lower Silurian system, the Hudson-river group of New York, we have also a striking example of the marked change which a deposit is found to have undergone when traced over a considerable area. In the district under consideration, it is clearly recognized throughout its whole extent, from Drummond's Island to Green Bay, except where it has been denuded, and the space occupied by lakes or bays. This group, like the other members of the limestone series before alluded to, exhibits a diminution in its thickness, as traced westwardly, and disappears, a short distance beyond the limits of the district.

"If we next consider the upper groups of the Silurian system, as displayed in this district, we find that, to a great extent, the reverse is true. Leaving out of view the Medina sandstone and the arenaceous portions of the Clinton group, which are scarcely recognizable here, we find the succeeding limestones much increased in thickness, and exhibiting no diminution, but rather an augmentation, as traced to the westward.

"From these general remarks, the reader will be prepared to understand the details given under each division of the series. It should be observed, however, in the outset, that the width of surface occupied on the map, by a particular group, is not always to be regarded as an indication of its thickness; for this, in most cases, is dependent upon the amount of dip in the beds, which, in this case, nearly corresponds with the slope of the country; so that it often happens that a group, less than a hundred feet in thickness, forms a belt several miles in width.

"The bearing and inclination of these successive groups indicate that they formed the outer margin of a great geological basin,

whose greatest depression is in the lower peninsula of Michigan, where the surface is occupied by rocks of the carboniferous epoch. It is only in a northern and northwestern direction, however, that we are enabled to trace the strata in a descending order quite to the lowest members of the series, and even to the non-fossiliferous series beneath them. In other directions, we find the most elevated portions of the border exposing only members of the upper, or at most, of the middle, portions of the Silurian system."

Potsdam Sandstone.—This was the first formed deposit in the Silurian basin, and attained, in its greatest development, a thickness of about two hundred and fifty feet, if we exclude the conglomerate-bands associated with the trap of Keweenaw Point and Isle Royale.

Range and Extent.—The bed of Lake Superior, embracing an area of about 32,000 square miles, is occupied almost exclusively by this rock, if we may judge from the islands which dot its surface, and the isolated patches which occur along its shores. On the north, the granite ranges approach near the coast and confine the sandstone within narrow limits; on the south, it occupies a broader area, and has been traced continuously around the axis, which divides the waters respectively flowing into Lake Superior, Lake Michigan, and the Mississippi. While the granite ranges attain in places an elevation of 1200 feet above the lake, the sandstones, except in the vicinity of the trap, do not reach higher that 350 feet, and rest in a position nearly horizontal. Consequently, the granites and slates rise up like islands through this great waste of sandstone.

This sandstone does not exhibit, at remote points, a homogeneity of character, or uniformity in thickness; but appears to have been modified, to a great extent, by local causes. Thus, in the vicinity of the trappean rocks, which afford ample evidence of intense and long-continued volcanic agency, the beds attain the enormous thickness of 5000 feet, and often consist of conglomerates, composed of trappean pebbles, cemented by a volcanic sand. Away from these lines of disturbance, and where it abuts against the azoic rocks, the mass consists of nearly pure silicious sand, enveloping pebbles of quartz and patches of slate. Where granite forms the adjoining rock, an equally marked change is observed in the character of the pebbles.

* * * * *

The Potsdam sandstone of New York is a quartzose rock, whose particles are firmly aggregated, while the same rock, on the northern slope of Lake Michigan, is so slightly coherent, that it may be crushed in the hand. The calciferous sandstone of New York, when traced west, passes into a magnesian lime-

stone. Even in that state, according to Hall, groups which, at one extremity, are of great importance, and well characterized by fossils, cannot be identified at the other.

The great mass of the materials which form the sandstones of this region appears to have been derived from the northwest, since the beds there attain a much greater thickness, and are composed of coarser sediments. They thin out as we trace them south and east, and are charged with fewer pebbles. On the Atlantic slope, according to the Messieurs Rogers, the sandstones expand in their southeasterly prolongation, while the limestones decrease. Hence, the ancient land was to the southeast and northwest, while the intervening space which formed the ocean bed, is occupied by the palæozoic series.

Sandstones of the Northern Shore. – On the northern shore of the lake, as before remarked, the sandstone occurs in insulated patches. According to Mr. Logan, to whom we are largely indebted for our knowledge of the geology of this portion of the region, on the isthmus between Thunder and Black Bays, sandstone occurs, composed of white silicious particles, but occasionally calcareous grains are intermingled. These beds attain a thickness of at least two hundred feet, and are succeeded by beds of red and white, in alternating layers. Conglomerates, containing pebbles of coarse red jasper, held in a reddish white or greenish sand, are also interstratified. The upper portion of the beds contains more calcareous matter, and some of the conglomerates envelope patches of limestone and chert. Mr. Logan estimates their entire thickness at five hundred feet. They are succeeded by limestones of a reddish-white color and very compact, interstratified with calcareo-argillaceous shales and reddish-white sandstones,—the whole attaining a thickness of eighty feet, with an addition of fifty feet of reddish, indurated marl. Succeeding these calcareous strata, after an interval, the amount of which is uncertain, red and white sandstones occur, with conglomerate layers, which become interstratified with trap layers, and an enormous amount of a volcanic overflow crowns the formation.

* * * * *

Sandstone of the Southern Shore.—Between Keweenaw Point and Isle Royale, there is, as we have before remarked, an immense curvature of the strata, probably reaching five hundred feet below the ocean level. A narrow belt of conglomerate and sandstone lines nearly the whole southern coast, from the head of Keweenaw Point to the Montreal river, beyond which, the latter, no longer associated with coarser bands, rapidly expands in width, and continues to Fond du Lac. For the most part, it appears in low ledges, or reefs, along the lake shore, but, at a few points, it rises in vertical cliffs, which afford many scenes of great beauty.

The bedded trap and conglomerate are admirably exhibited on Keweenaw Point, and in many places in the Ontonagon district; but as we have minutely described the geographical distribution of all the trappean rocks of this era with the associated conglomerate and sandstone, in a former report, a further description is deemed unnecessary.

* * * * *

[The features of this sandstone are described as presented at various points north of the lake, at Presqu'Isle, Carp River, Grand Island,—where the variety of colors the rock presents has given rise to the name of "Pictured Rocks,"—on the Menomonee River, White Rapids, etc.]

At the White Rapids, the sandstone is again exposed, presenting very nearly the same external characters, except that it is less discolored, and reposes on the uptilted edges of the quartz. It may be seen in some of the rapids below, and reäppears, for the last time, in the river banks, forming ledges six or eight feet high, about three miles above the Big Bend, in township 35, range 29.

"* In this vicinity, Mr. Desor discovered, in some of the loose masses of this rock, other fossils than the *Lingulæ*, which are so characteristic of this group further to the east. These fossils consist of the fragments of one or more species of trilobites, resembling *Asaphus*. From the characters preserved in a single caudal extremity, one species is identical with that which occurs in the same rock on the Missisippi and St. Croix rivers.

From the Menomonee river, the Potsdam sandstone approaches within fifteen or twenty miles of the shore of Green Bay, being distinctly exposed on all the streams flowing into it. Continuing in the same direction, its easterly limit passes near the Great Bend in the Wolf river, northwest from the outlet of Lake Winnebago. From thence, meandering westerly, it follows nearly the course of Wolf river, crossing it in the neighborhood of Lake Pauwaiceen, and is thence prolonged southwesterly towards Green and Puckaway lakes. In the neighborhood of Pleasant Valley, about twelve miles west of Strong's Landing, on the Fox river, it is exposed in several low escarpments, succeeded by the calciferous sandstone, which here presents its usual characters. From this region, its southern limit stretches to the west and northwest. The country here presents a feature which continues to the Mississippi river. The hills appear to be outliers, capped by the calciferous sandstone or succeeding limestones, while the valleys and the lower part of the escarpments are composed of the Potsdam beds.

* The description of the westerly prolongation of this sandstone is from the MS. of Mr. Hall.

The rock is fine grained, of a light yellow color and very friable. Some of the superior beds, which are thin, have been wrought for grindstones. The friable character of this sandstone is one of its most prominent features, and, owing to this circumstance, the escarpments are not usually high, or abrupt, unless it has been protected by the overlying rock. In its want of cohesion, it differs, in a very marked degree, from the prevailing character of this rock, as developed in New York and Canada, where it is usually, though not always, compact. It is not, however, unlike the sandstone of the Pictured Rocks, and is less friable than that of the Mississippi and St. Croix region.

The almost uninterrupted continuity with which this rock can be traced, even from its eastern extension through Canada and along the northern shore of Lake Huron to the St. Mary's river, and thence westerly, leaves no doubt as to its true position and identity in age with the Potsdam sandstone of New York. If we were at a loss in thus tracing it continuously, we have still the evidence of the succeeding fossiliferous strata, which show, conclusively, the same relations to this sandstone as they do to its equivalent in New York. With both these evidences combined, we cannot hesitate for a moment in our conclusion regarding its age and place in the series.

From the points just noticed, where this sandstone appears in eastern Wisconsin, it can be traced uninterruptedly across the entire breadth of the state to the Mississippi and St. Croix rivers. It is true, at the last named localities, we have the evidence of fossils which are not known to occur in its easterly extension; but we have already noticed the occurrence of the Trilobite on the Menomonee, while we have the Lingula everywhere, though in far greater profusion in the St. Croix region than elsewhere. In drawing inferences as to the age of the rock, from the occurrence of these fossils, it should be remembered that it is by no means improbable that similar ones may yet be found in more easterly localities. They seem to be coëxistent with calcareous bands, or the more calcareous portions of the group, and it is to this modification that we should look for the development of the fauna of this ancient period.

From all this evidence, we regard the question of the age of this rock as settled—that the Potsdam sandstone of New York is identical with that of the Mississippi and the St. Croix. One great source of doubt and perplexity in its determination, heretofore, was the recurrence of a sandstone identical in character with the lower, but superior in position to the calciferous sandstone, or lower magnesian limestone. It is a thin mass, evidently due to a recurrence of the same causes which produced the inferior deposit. This has been well elucidated by Dr. Owen in his reports on the upper Mississippi, in which he has shown that,

near the junction of the lower sandstone with the calciferous, there are several alternations of calcareous and silicious bands, the latter having the character of the sandstones below, and the former of the calcareous deposits above. These occur in several places on the upper Mississippi river, and give the geologist an introduction to that condition of things which subsequently produced the upper sandstone, which is distributed over a large part of Wisconsin, so often mistaken for the lower member of the series; but which, in fact, is separated from it by two or three hundred feet of calcareous rocks.

This upper sandstone can be regarded in no other light than as the result of the same causes which produced the Potsdam, and were suspended during the period of the deposition of the calciferous sandstone, or lower magnesian limestone, to be renewed, for a short period, in the deposition of a mass of sandstone, varying from fifty to eighty feet in thickness, upon the surface of the calcareous deposit. This fact shows the more intimate connection between these two lower groups than has heretofore been suspected. It is, nevertheless, shown in many places within the Lake Superior district, that the true sandstone, as it is traced upward, becomes gradually calcareous, and "finally passes into well-characterized, compact, magnesian limestone."* The same is true, also, of this rock, in Canada and New York; while, however, there is rarely any evidence of increase in the silicious materials towards the termination, as we observe in the west. In some localities, there are thin but distinct bands, near the upper portion, having an oolitic structure, which, as we go westward, appear to be replaced by beds of a granular texture and of a silicious character.

Art. III.—*Analysis of Tin Pyrites;* by Dr. J. W. Mallet.†

This rather rare mineral is one of which the chemical composition has appeared somewhat doubtful, owing to the considerable discrepancy between the three or four analyses which have been made of it, and to the fact that it almost invariably occurs massive and so intimately mixed with copper pyrites and other minerals as to render it difficult to select a fair specimen for examination. The locality from which most of the specimens in cabinets have been derived is Wheal Rock, near St. Agnes, Cornwall; but the mineral has also been observed in two or three other Cornish localities, and at Zinnwald, in Bohemia. I have recently received a specimen (name unknown) from a friend in England, which he states to have been found on St. Michael's Mount,

* Part 1, p. 117.

† Communicated for this Journal.

Cornwall, and which, on examination, proves to be tin pyrites, and apparently in a purer state than any hitherto analyzed.

This specimen occurs in quartz which has obviously been taken from a vein in granite. The structure appears to be crystalline, although no distinct planes could be observed. The color is not steel-gray, as in that of the mineral from Wheal Rock, but iron-black, with slight superficial blue and red tarnish in some places. Streak black, lustre sub-metallic, fracture uneven. Hardness $=4$. Sp. gr. $=4{\cdot}522$. Heated before the blowpipe, on charcoal, sulphurous acid is given off, oxyd of tin deposited in large quantity upon the charcoal, and a black globule obtained, from which copper and tin may be reduced on the addition of soda.

A carefully conducted quantitative analysis, in which chlorine was used to decompose the mineral, gave the following results.

			Equivalents.	
Sulphur,	29·46		1·841	8·092
Tin,	26·85		·455	2·
Copper,	29·18		·920	4·044
Iron,	6·73	·240	·464	2·040
Zinc,	7·26	·224		
Gangue,	·16			
	99·64			

Thus the relative number of atoms of sulphur, tin, copper, and iron and zinc, as given in the 2nd and 3rd columns above, are almost exactly as 8 : 2 : 4 : 2 ; whence we have the formula first assigned by Kudernatsch, (Pogg. Ann., xxxix, 146.) viz., 2(Fe S+ZnS), SnS_2+2Cu_2S, SnS_2. The present analysis agrees so closely with this formula, from which the results of Kudernatsch, and even those of Rammelsberg, (Handw. d. Chem. Theils d. Mineral. 2d Suppl., 179,) sensibly differ, that it seems fairly to be considered as representing the composition of the mineral in a pure state. The analysis also possesses some interest in showing the presence of zinc in considerable quantity, therein agreeing with Rammelsberg's analysis above referred to of the mineral from Zinnwald. It is to be observed that in both cases the iron and zinc occur in very nearly atomic proportions, so that *perhaps* the formula should be written 2FeS, SnS_2+2ZnS, SnS_2+2(2 Cu_2S, SnS_2), though this does not seem very probable, since Kudernatsch found 12·44 p. ct. of iron to but 1·77 of zinc, while Johnston gives 10·113 p. c. of the latter to 4·791 of the former, as contained in tin pyrites (from the same locality as the specimen submitted to the present analysis, St. Michael's Mount). The presence of this large quantity of zinc, is however of importance, chiefly as proving that the tin must enter into the composition of the mineral as bi-sulphuret, since the other formula which has been proposed, namely, 2SnS, FeS_2+2Cu_2S, FeS_2, would

require us to admit the presence of ZnS_2, whereas no such compound is yet known either occurring in nature or formed by artificial means. It is remarkable that Fahlerz, the only other compound sulphuret in which zinc occurs in notable quantity, is the mineral whose composition (4(RS), SbS_3) seems to approach most nearly to that of tin pyrites, the latter containing two instead of four atoms of the sulphur bases to one of sulphur acid, and this electro-negative sulphuret being bi-sulphuret of tin, instead of ter-sulphuret of antimony. Indeed, some real connection between these two minerals seems to be further indicated by their occurrence in the same crystalline system, and their close resemblance to each other in hardness, specific gravity, and general physical characters.

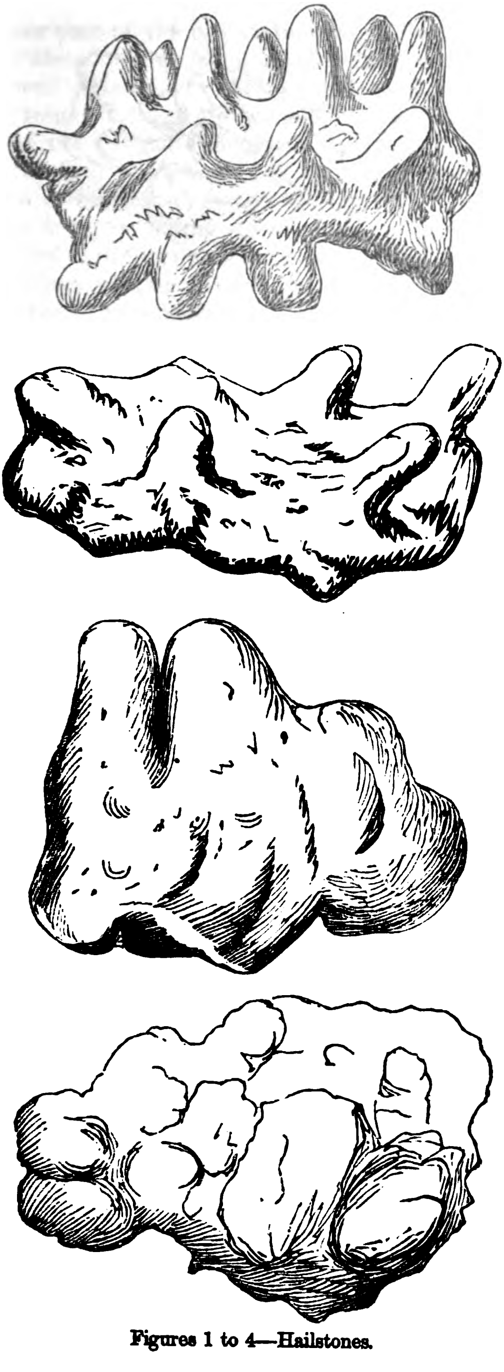
ART. IV.—*Notice of the Hail Storm which passed over New York City, on the first of July*, 1853; by ELIAS LOOMIS, Professor of Mathematics and Natural Philosophy, in the University of the City of New York.

ON the first of July, 1853, a very remarkable hail storm passed over the city of New York. The day had been uncommonly hot and sultry, the thermometer having risen to 90 degrees, and the air was believed to contain an unusual amount of vapor. A little before 5 o'clock in the afternoon, a heavy black cloud was observed to rise in the northwest, the wind at the time blowing moderately from the northeast, and subsequently from the east. As the cloud advanced and covered the northwestern sky, while it was still clear in the southeast, numerous streaks of zig-zag lightning appeared to issue from the front margin of the cloud and descend towards the earth. I noticed the approach of the storm from my lodgings in Eighth street, within a quarter of a mile of the University. About five o'clock the wind came strong from the northwest, and the rain poured down in torrents. Presently I heard a loud thump upon the roof of the opposite house; soon another thump; and presently a third and fourth. I was not long in discovering that the noise was produced by the fall of hailstones of a size such as I had never before witnessed. They were few in number—but their average size was little less than that of a hen's egg; and one or two I am persuaded were fully as large as my fist. They almost invariably broke on striking the pavement; so that I could not secure either of those large stones except in fragments; and moreover the rain was falling in torrents. I however hastened to the yard in the rear of the house, hoping to find some upon the grass which had not been broken in the fall. After the rain had nearly subsided, we

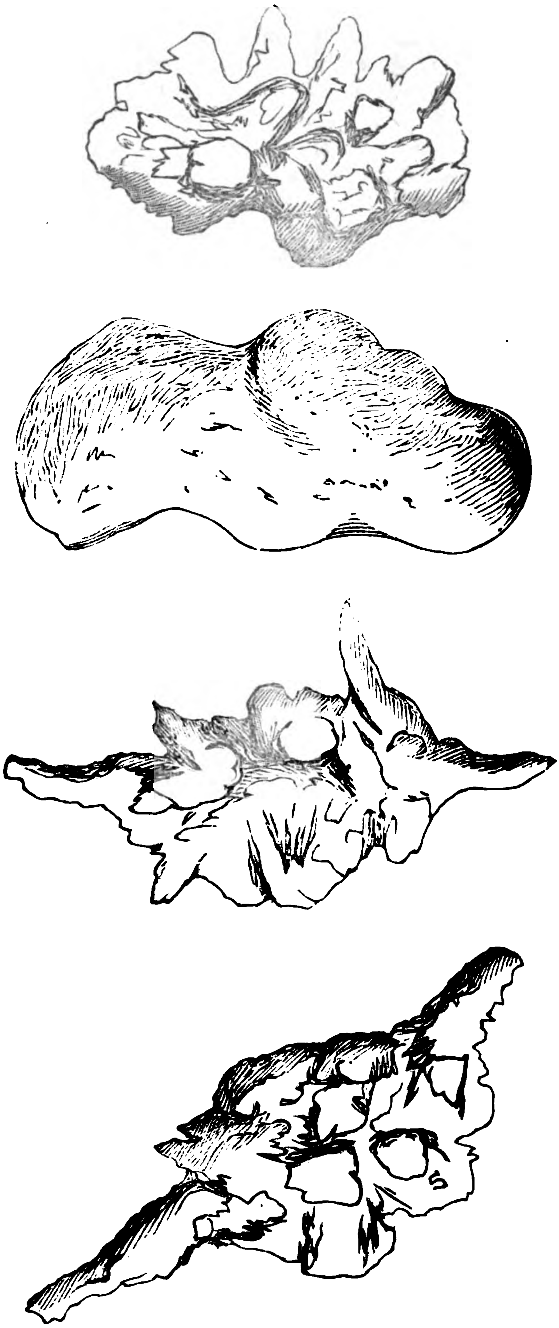
found several handfuls of hailstones of good size, though altogether inferior to those which I saw in the street. They generally consisted of very transparent and solid ice, with many air bubbles; but they were not spongy in the centre as they are sometimes found. Hailstones sometimes occur which appear to be little more than pretty compact snow-balls. In the present instance, the hail was *not* of this kind. The large stones, however, generally consisted of an irregular assemblage of angular pieces of ice, which individually did not much exceed the size of hazel nuts—but they were cemented very firmly together. Indeed there was no appearance of seams or joints between these individual portions—but the ice was equally strong throughout every part of the mass. Their structure therefore did not indicate that several small hailstones were seperately formed and were subsequently cemented together; but rather that all were formed simultaneously about a common nucleus. Several persons independently, and without concert, suggested that the conglomerated mass resembled rock-candy: and the comparison appeared to me to be a very just one. There was a decided appearance of a tendency to crystallization. This tendency was in many cases towards a pyramidal form; in others they bore a resemblance to hexagonal prisms; and in some it appeared to me the tendency was towards a cubical form—though as the angles were all much rounded by the melting of the ice, I did not find any complete cubes.

Several of the stones which we picked up in the yard measured two inches long, and one measured over two and a half inches. These had been lying several minutes in a warm drenching rain; and it is my full conviction that two or three of those which I saw in the street were three and a half inches long, by two and a half inches wide, and they did not appear to deviate much from the spheroidal figure. A friend of mine, who is by profession a painter, and who saw and handled the hailstones, at my request made a sketch of some of the most remarkble of those which we picked up. These are shown in the accompanying figures which are drawn of the natural size. It is to be understood however that these were unquestionably smaller than many which we saw fall.

The rain, accompanied by thunder and lightning, continued for six or eight minutes, when its violence somewhat abated—it returned again with renewed energy, but soon afterwards entirely ceased. Another, but more moderate shower followed half an hour later, yet without either hail or lightning. Throughout the entire storm, the wind had blown with considerable force, but not with destructive violence, in that part of the city which is southwest of the University; and in the lower part of the city there was comparatively little wind.



Figures 1 to 4—Hailstones.



Figures 5 to 8—Hailstones.

In the upper part of the city, however, in the neighborhood of the Crystal Palace, the wind blew with destructive violence. A high brick wall was blown flat to the ground—a block of four wooden buildings (not entirely completed) was prostrated—and a small wing of the Crystal Palace was blown down. The fall of hail was heavy, and considerable glass in the Crystal Palace, and the buildings in the vicinity, were broken.

During the first part of the storm, the lightning was unusually severe. Several buildings and trees in New York and Williamsburgh were struck by the electric fluid, and one or two barns were burned to the ground.

I have succeeded in tracing this storm for a distance of full twenty five miles, and for about two-thirds of this distance have followed the track personally on foot. The portion of the track which I have myself surveyed, commences about a mile and a half southwest of Paterson, N. J., from which point it proceeds in a southeast direction—passing over the village of Acquackanonck, together with the cities of N. York and Williamsburg—and from this point the storm can be traced with diminished energy to Jamaica Bay. Near Paterson, the wind is believed to have been more violent than in any other part of the above mentioned track. Where it swept through the forests, many large trees, of one to two feet in diameter, were overturned—while others were snapped off and twisted like reeds. This remark applies to a distance of about three or four miles from the commencement near Paterson. In the neighborhood of Acquackanonck, a few trees were overturned—but not a large number. East of Acquackanonck, the track soon crossed the Hackensack meadows where the ground is low and flat, and there were no trees to be overturned. I have obtained no information of the effect of the wind upon the high ridge on the west bank of the Hudson river—but the entire length of the track across New York was marked by violence, as above stated. This region was particularly exposed since it was the highest ground encountered by the storm in its passage across the island. Having crossed East river, the storm passed centrally over Williamsburgh, where it caused more damage than in any other part of its course. The steeples of two churches (the first Presbyterian and the Dutch) were blown down; the roof of a third church was partially blown off—the roofs of a large number of dwelling houses were carried away—the tin from numerous roofs was rolled up in long solid coils, and in some cases carried off—while in others, it clung to the roofs and was left after the storm in long massive windrows.

The breadth of the track near Paterson is thought not to have exceeded half a mile—perhaps was somewhat less than this—and

the destructive violence did not extend beyond these limits, until the storm reached Williamsburgh: but here the wind was almost equally violent over a space a mile and a half in breadth, while houses were unroofed over a track two miles in breadth. Beyond Williamsburgh, the wind was less destructive—the track became broader and less distinctly defined—and the general course deviated more to the east. The storm was reported as severe at Jamaica and South Jamaica. From the commencment near Paterson to Williamsburgh the track did not deviate sensibly from a straight line; and its course was from N. 40° W. to S. 40° E.

Throughout the entire track here mentioned, hail fell of unusual size. Near Paterson the stones were smallest in size, but most abundant in quantity. The destruction caused to the fruit and the crops was such as not unfrequently occurs in France, but has seldom been witnessed in this country. When I visited the spot a few days after the storm, the trees looked as if they had been pelted by myriads of heavy stones. The leaves were strewed thick upon the ground; and most of those which still clung to the branches, were riddled through and through, and dried upon the stems. The rails of the fences bore marks of large gashes where the brown weather-worn surface had been nicked off and a fresh surface exposed, as if by a volley of stones from a troop of mischievous boys. Upon the north side of the houses along the track, scarce a pane of glass was left entire, and the clapboards were covered thick with gashes an inch in diameter, where the paint was chipped off. Fields of wheat and rye, which had not been harvested, were beaten down as flat as if a heavy iron roller (such as is sometimes employed for smoothing gravelled walks) had been dragged over them; and fields of corn were totally destroyed. On some fields, I was assured that after the storm the ice lay in a solid compact mass two inches thick. Large quantities of ice still remained unmelted on the ground the next morning, and a tenant on one of the farms collected a considerable quantity and carried it into Paterson, (two miles distant,) to show to his landlord; and I was informed that in a hollow, against the side of a house, the ice accumulated to such a depth, that on the evening of July 2nd, the day after the storm, a bushel basket full of ice was shoveled up; the stones varied in size from that of a pigeon's egg, to a hen's egg. The track of the ice did not deviate much from the track of greatest violence of the wind, and followed the same general direction, but covered a somewhat greater breadth. From Paterson to the Hackensack meadows beyond Acquackanonck, the damage caused by the hail was very great, amounting to nearly an entire destruction of the crops of wheat, rye, oats and corn, as also the cherries, peaches, apples, etc., within the limits of the track.

In the city of New York, the damage done by the hail was not very great; for the stones were not numerous, although of prodigious size. The ship-yard of Mr. Thomas Collyer at the Dry Dock, was covered with singularly shaped pieces of ice,—one of which was measured and found to be 6¼ inches in circumference—another seven inches, and a third measured three inches long, and two inches thick.

In Williamsburgh the hail appears to have destroyed more glass than in New York. In many houses nearly half the glass was broken in windows which were unprotected on the north side. Over 400 panes of glass were broken from the north side of a single school house.

On the same day with the preceding storm, large hail is said to have fallen at several places in Pennsylvania. About three o'clock in the afternoon, a terrific hail-storm passed over Northumberland doing great damage. Hail-stones are said to have been picked up measuring 7¾ inches in circumference; and several thousand panes of glass were broken in that town.

At 5½ o'clock, P. M., a severe hail-storm passed about 20 miles north of Philadelphia. At Upper Dublin, the storm was very destructive. Several barns were unroofed, many fruit and forest trees were blown down—and many fields of wheat and oats so badly damaged, as scarcely to pay for harvesting. One hailstone was measured, and its greatest circumference found to be 6½ inches, and its smallest five inches,—and this was half an hour after the storm had abated. At Norristown and Doylestown the crops were much injured by the hail, and at Burlington, N. J., the wind was exceedingly violent.

It is not probable that either of these storms was the same as that which passed over New York. The hail-storm near Philadelphia, was about simultaneous with that at New York. The storm at Northumberland may have been identical with that at Upper Dublin, the distance of the places being 100 miles—the interval of time 2½ hours—and the direction nearly parallel with the track of the New York storm.

It would appear that a violent wave of great extent set in from the northwest, which rolled over both New York and Philadelphia, and within this wave were formed about simultaneously several distinct veins of hail.

Was the storm which passed over New York a whirlwind?

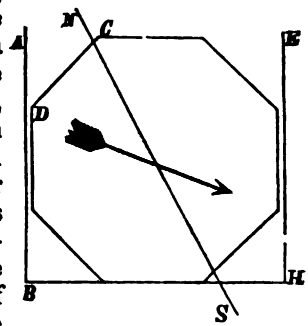
I have surveyed every part of the track of the storm where I have heard of any violent effects, especially with reference to the decision of this question. Throughout Williamsburgh, I could find no unequivocal evidence of rotation. The steeples which were prostrated, fell in a direction coinciding very nearly with that of the storm's progress, that is, towards the southeast. In the case of one of the churches whose steeple was blown down

(the first Presbyterian Church) I noticed a phenomenon which I considered worth recording. The track crossed the ridge of the church at an angle of about 45°. On the leeward side, the tin roof was started from the boards (but not broken) and puffed up forming a wrinkle about twenty feet long, two or three feet wide, and ten inches high. This appears to me to indicate the operation of a force beneath, pushing up the tin; but not being able to tear the tin open, bulged it up and left it in a ridge.

This phenomenon appears to be analogous to what often occurs in tornadoes, and I ascribe it to a rarefaction of the air on the leeward roof. A current of air, forcibly impelled over an obstacle like the roof of a building, by friction drags along with it the air lying upon the leeward side of the roof, producing a partial rarefaction, and the air beneath by its expansion, tends to lift the roof. Thus the leeward roof is often carried away, while the windward roof remains. In the present case, this upward pressure lifted the tin about ten inches, stretching but not tearing it. This force appeared to be insufficient to tear the tin from its fastenings—perhaps because from the carrying away of the steeple, and the ripping up of the adjacent edge of the tin, the air beneath found a ready escape.

In the neighborhood of the Crystal Palace, occurred a phenomenon which appeared to indicate the existence of currents blowing nearly in opposite directions. The wooden buildings, which have already been mentioned, were blown toward the southeast —but the brick wall, the line of which run from N. 28° E. to S. 28° W., fell toward *the west;* that is, in a direction nearly contrary to that of the storm's progress.

The following appears to me to be the explanation of this phenomenon. The Latting Observatory is an octagonal tower, 300 feet high and 75 in diameter at base, sloping uniformly to the top. In the annexed figure, the octagon represents the base of the tower, and the line N S represents a meridian. On the west side of the tower, was erected a brick wall A B, 25 feet high, and only three feet from the side of the tower. At the south end, it was connected with another wall B H, but at the north end it was entirely free, and had no support except an iron clamp projecting from the side of the tower. The direction of the storm's progress is indicated by the arrow.



It might have been anticipated that the wall A B would have been thrown towards the east upon the tower; whereas in fact it was thrown outwards towards the west. But we know

from the testimony of spectators, that this wall fell at the first onset of the blast, when, as we shall presently see, the wind blew nearly from the north, or perhaps a little east of north. Now a violent current from the north, driven into the triangular space A D C, would necessarily become condensed between the wall and the tower, exerting a force to push the wall outward, and the wall had little strength to resist the pressure, being weak not only from its great height, but also being unsupported at the north end. The bricks also had been recently laid, and the mortar was not yet dry. On the east side of the tower, was a similar wall E H, but only 14 feet high, which was not prostrated. Its security is probably to be ascribed to its inferior height.

The following facts at first seemed to me a little puzzling. Near Paterson, a large oak limb, a foot thick, was twisted off at the height of fifty feet, and prostrated in a direction pointing S. 20° E., the top of the limb lying towards the base of the tree. Within a short distance I found another large oak limb, torn off at a great height and thrown towards S. 40° E., with its top also turned towards the base of the tree. Not far off, I found a third limb in a similar position. Broken limbs were generally found to have been carried eastward, with the top pointing to the S. E., and the base towards the N. W. The three cases I have here specified were exceptions to the general rule, and it appears to me that they are to be explained by supposing that the branches turned a somerset in falling.

A like case occurred with the steeple of the first Presbyterian Church in Williamsburgh. The spire fell across the street—the top struck a brick house on the opposite side of the street and broke off, while the upper portion of the spire remained imbedded in the roof of the house which was crushed in by the blow. The remainder of the spire, which was now the frustum of a pyramid, fell down into the street; but it is probable that the smaller end of the frustum struck the pavement first, for the steeple turned a somerset along the street towards the east, and lay after the storm with the smaller end of the frustum turned *towards* the church.

A similar supposition will satisfactorily account for the observed position of the three limbs above mentioned.

In the woods between Acquackanonck and Paterson, I measured with a pocket compass the direction of a large number of prostrate trees. The following list shows the entire range of the bearings which I measured; *not* arranged in the order in which they were measured, but classified according to the points of the compass. They were

S. 70° W.; S. 20° W.; S. 15° W.; S. 10° W.; south; S. 10° E.; S. 20° E.; S. 25° E.; S. 30° E.; S. 35° E.; S. 40° E.; S. 45° E.; S. 50° E.; S. 60° E.; S. 70° E.; east.

These bearings were measured at various points upon a portion of the track about two miles in length; and it will be noticed that there is not a single instance of a tree which was prostrated towards any point between East, North and West. The bearings extend from east, through south, to S. 70° W., a range of 160 degrees; but I found only one instance of a bearing approaching nearly so close to the west point. With but one exception, the bearings were all confined between east and S. 20° W. The wind did not then blow from every point of the compass indifferently, at least not with sufficient force to prostrate trees, but it blew only from the northward, including northeast and northwest. Neither was the wind a simple rectilinear current. What law then did the wind observe? Was its motion merely centripetal? Did it revolve in a whirl? Or did it follow some other law?

In order to decide these questions, I attempted to apply the method which I had successfully practised in the Mayfield tornado of Feb., 1842. This method consisted in selecting groups of prostrate trees which lay upon each other, and measuring successively the bearings of the bottom tree, of the next in order, and so on to the top, and regarding these bearings as indicating the successive directions of the wind at the point of observation, as the storm passed by. At Mayfield I had no difficulty in finding groups of trees piled upon each other, frequently four or five in a group; but at Paterson I found in no case more than two trees crossing at a considerable angle, and only five instances of this description. The following are the observations in the cases referred to, the bearing first mentioned in each case being that of the bottom tree.

1. S. 50° E.	2. S. 40° E.	3. S. 20° E.	4. S. 40° E.	5. S. 70° W.
S. 20 E.	S. 25 E.	S. 40 E.	S. 10 E.	S. 60 E.

The first four cases present no angle greater than 30°; the fifth case presents an angle of 130°; that is, the two trees were turned in nearly opposite directions.

From a comparison of all the facts, I conclude that the wind blew first from the northeast, and that this current was succeeded by a north and presently a northwest wind. The following are my reasons for this conclusion.

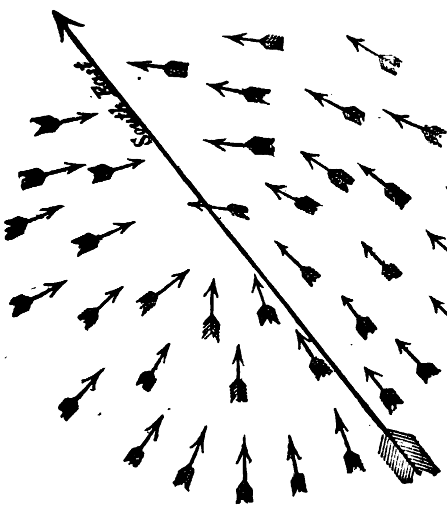
1. The fifth case of interfering trees, just mentioned, taken in connection with all the bearings observed, points to this conclusion. We find that one large tree was prostrated with its top turned towards a point S. 70° W. Upon it lay another large tree with its top turned S. 60° E. We may safely infer that these directions corresponded nearly with the direction of the wind when the trees were prostrated, and that the wind came first from a point N. 70° E., and was succeeded by a current from N. 60° W. In each of the other cases of interfering trees, the angle of crossing was so small, as to convey no very distinct information

on this question. In three cases out of the four, the first blast appears to have been a little more westerly than the final one; but all the trees were prostrated by a northwest wind.

2. A very intelligent farmer, whose house was close upon the northeast margin of the track, about four miles from Paterson, gave the following testimony. He first took refuge from the hail under a shed on the southwest side of his barn, the wind then blowing from the N. E. After a short time, the hail began to beat upon him, the wind having veered to the N. W., and he was obliged to seek a shelter on the southeast side of his barn in order to escape the hail.

3. It is known from the testimony of several individuals, that the wind at New York was easterly on the first approach of the storm.

Upon comparing these facts, it appears to me that the direction of the wind at the time of its most destructive violence may be tolerably well represented by the annexed diagram, showing a



current from the N. E., on the front of the storm; and from the N. W. in the rear, the whole having a progressive motion towards the S. E., which would give to each place in succession (unless near the southwest margin of the track) first a N. E. wind, and afterwards a N. W. wind.

I do not then find in this case that evidence of a complete rotation which I have found in some other tornadoes; but it is possible that at a little elevation above the earth's surface, the rotary motion may have been more decided.

What was the cause of the hail?

The hail was caused by a violent upward movement of the air, carrying along with it an unusual amount of vapor, which was suddenly condensed, and at so low a temperature, that it was frozen in large semi-crystalline masses.

That there was a violent upward movement of the air, I infer from the following considerations.

1. Rev. J. W. McLane of Williamsburgh was in the street near his house, and noticed the coming up of the storm. He says the cloud was very dense and black—moved rapidly forward—and under the main sheet, the clouds boiled up in a violent and angry manner, which led him to anticipate a severe blast. Other observers have testified to substantially the same facts.

2. It appears impossible that two currents, in close juxta-position, should blow from nearly opposite quarters, with sufficient violence to prostrate large trees, unless there is opportunity for the air to escape by an upward movement. This conclusion is also in perfect harmony with what we have frequent occasion to observe in small sand whirls and water spouts.

How was the cold which formed the hail produced?

According to the observations of Pouillet, in France, the temperature of hail-stones when they fall, is sometimes as low as 25° Fahr. They must then have been formed at a temperature considerably below that of melting ice—a temperature probably as low as 20° Fahr. How can so low a temperature be produced in the hottest weather of July? The temperature of the air diminishes as we ascend from the earth, and at the height of 8800 feet above New York, is estimated at 32° in summer. At the height of 12000 feet the temperature is reduced to 20°. Were the hail stones in the present case, formed at an elevation of 12000 feet? It does not appear to me that we are at liberty to make such an assumption. In the summer of 1835, several hail-storms passed over the southern part of France, where there were insulated peaks of mountains, which afforded precise means of measuring the elevation of the hail. In the storm of July 28th, 1835, no hail fell on the summit of the Puy du Dome, an elevation of 4800 feet above the sea; but a few stones fell at the height of 3700 feet, while at the foot of the mountain, the ground was covered to the depth of three inches, and some of the stones weighed eight ounces. On the 2d of August of the same year, a hail cloud enveloped the summit of the mountain, rising therefore to the height of at least 5000 feet.

It does not therefore appear to me that we are at liberty to assume that the hail of July 1st, was formed at an elevation much exceeding 5000 feet, and here the summer temperature may be estimated at 46°. This cold is of course insufficient to form ice.

It is believed that during the passage of a hail storm, the temperature of the upper air is considerably below the mean. The simple presence of clouds in the lower atmosphere would tend to produce such an effect. The atmosphere derives its heat from the earth, and is but little affected by the direct passage of the solar rays. The heat which the earth imbibes from the sun is continually thrown off by radiation ;—but when the surface of the earth is covered by a cloud, this radiant heat is intercepted, and the temperature of the lower air is thereby elevated. On a still night, the presence of clouds sometimes causes the thermometer to stand ten or fifteen degrees higher that it would otherwise. But if, by the interposition of a cloud, the lower atmosphere becomes unusually hot, the atmosphere above the cloud must receive less than its usual supply of heat, that is, it must become unusually cold.

Moreover, in the storm of July 1st, the hail was formed in a current blowing violently from the northwest, which came therefore from a higher latitude, and of course brought with it a diminished temperature. I have no data sufficiently precise for estimating the effect to be ascribed to this cause, but I think we may conclude that at the time of the storm in question, at an elevation of 5000 feet above New York, the temperature could not have differed much from 32°. We have not however yet reached the temperature necessary to the production of hail.

Another source of cold is to be found in the evaporation from the surface of the hailstones. It is well known that if we tie a piece of thin muslin upon the bulb of a thermometer, and then after dipping the bulb in water, swing it rapidly through the air, the thermometer will sink below the temperature of the air, several degrees, sometimes ten or fifteen; an effect due to evaporation. During a hail storm, the hot air from the earth's surface is carried by the upward movement to a considerable elevation, by expansion it is cooled, and a portion of its vapor is condensed. The drops thus formed at a temperature not far from 32°, are still further cooled by evaporation from their surface, (the evaporation being promoted by their rapid motion;) the remainder of the drop is congealed; and as new vapor is precipitated, it is congealed upon the former;—thus forming concentric layers round the nucleus. Since water, like nearly every other substance, in passing to the solid state, inclines to crystallization, the ball as it increases, does not generally retain the spherical form, but shoots out irregular prisms.

How does a hailstone remain suspended in the air long enough to acquire a weight of half a pound?

This difficulty is not, to my mind, a very formidable one. I conceive that hail stones are formed with great rapidity. The vapor is condensed with great suddenness and almost instantly frozen. I think very large hailstones may be formed in five

minutes. In a vacuum, a stone would fall from the height of 5000 feet in less than 20 seconds—but drops of water and hail stones fall with only a moderate velocity. From my own observations of the hail stones of July 1st, I estimated the velocity of their fall at about 40 feet per second. At the uniform rate of 40 feet per second, a stone would be more than two minutes in falling 5000 feet.

In order to obtain some reliable data for estimating the velocity of hail stones, I have computed the greatest velocity of a number of small bodies differing in size and specific gravity. Dr. Hutton determined by numerous experiments the resistance of the air to bodies moving with different velocities; and in the third volume of his Tracts, p. 218, has given a table of the air's resistance to a sphere 2 inches in diameter. His experiments also indicated that the resistance, of spheres increases in a ratio somewhat greater than the squares of the diameters. This excess for spheres of from 2 to 4 inches diameter was about $\frac{1}{30}$th part of the resistance. The second column in the following Table is taken from Hutton's Tracts, the resistances for velocities from 50 to 100 being supplied by interpolation. The resistance for a sphere 1 inch in diameter is found by taking one-fourth of the numbers in the second column, and diminishing the result by one-thirtieth part. Each succeeding column is derived from the preceding in a similar manner.

Resistance of the air to Spheres of different Diameters.

Velocity per second	Sphere 2 inches in diameter.	Sphere 1 inch in diameter.	Sphere $\frac{1}{2}$ inch in diameter.	Sphere $\frac{1}{4}$ inch in diameter.	Sphere $\frac{1}{8}$ inch in diameter.
feet.	ounces.	ounces.	ounces.	ounces.	ounces.
5	0·006	0·001	0·000	0·0001	0·0000
10	0·026	0·006	0·001	0·0004	0·0001
15	0·058	0·014	0·003	0·0008	0·0002
20	0·103	0·025	0·006	0·0015	0·0004
25	0·163	0·039	0·010	0·0023	0·0006
30	0·237	0·057	0·014	0·0033	0·0008
35	0·325	0·078	0·019	0·0046	0·0011
40	0·427	0·103	0·025	0·0060	0·0015
45	0·544	0·131	0·032	0.0077	0·0019
50	0·676	0·163	0·039	0·0095	0·0023
55	0·821	0·198	0·048	0·0116	0·0028
60	0·981	0·237	0·057	0·0138	0·0033
65	1·155	0·279	0·067	0·0163	0·0039
70	1·343	0·325	0·078	0·0190	0·0046
75	1·546	0·374	0·090	0·0218	0·0053
80	1·764	0·426	0·103	0·0249	0·0060
85	1·996	0·482	0·116	0·0282	0·0068
90	2·243	0·542	0·131	0·0317	0·0076
95	2·505	0·605	0·146	0·0354	0·0085
100	2·780	0·672	0·162	0·0392	0·0095
200	11·340	2·764	0·668	0·1615	0·0390
300	25·800	6·235	1·507	0·3641	0·0880

In a vacuum, a body falling under the influence of gravity is continually accelerated; but when a heavy body falls through the atmosphere, the resistance increases with the velocity, until the resistance becomes equal to the weight of the body. When this takes place, there can be no further increase of velocity, and the body will afterwards descend with a uniform motion. In order therefore to determine the greatest velocity which a heavy body can acquire by falling through the atmosphere, it is only necessary to compute the weight of a sphere of given diameter, and then to search in the preceding table for the velocity due to an equal resistance upon a body of the proposed diameter. The following Table exhibits the results for spheres of lead (assuming the specific gravity 11·35), of iron (specific gravity 7·78), of water, of ice (sp. gr. 0·93), and cork (sp. gr. 0·25); the diameters varying from two inches to one-eighth of an inch.

	Weight of a sphere of					Final velocity of sphere of				
	Lead.	Iron.	Water.	Ice.	Cork.	Lead.	Iron.	Water.	Ice.	Cork
Diam.	ounces.	ounces.	ounces.	ounces.	ounces.	feet.	feet.	feet.	feet.	feet.
2 in.	27·5132	18·8593	2·4241	2·2544	0·6060	310	257	94	90	47
1 "	3·4392	2·3574	0·3030	0·2818	0·0757	223	185	68	65	34
½ "	0·4299	0·2947	0·0379	0·0352	0·0095	161	134	49	47	25
¼ "	0·0537	0·0368	0·0047	0·0044	0·0012	117	97	36	35	18
⅛ "	0·0067	0·0046	0·0006	0·0006	0·0001	84	70	25	24	12

Thus it appears that a hail stone in the form of a sphere two inches in diameter, falling through a tranquil atmosphere cannot possibly acquire a velocity exceeding 90 feet per second, and spheres of a smaller size would acquire a still less velocity. Also a hail stone of irregular shape would experience more resistance than a sphere, and would acquire a smaller velocity in falling. An upward current of air moving with a velocity of 90 feet per second, or 61 miles per hour, would sustain a sphere of ice two inches in diameter; also an upward current of 30 miles per hour would sustain hail stones of half an inch in diameter, and would greatly reduce the velocity of stones of larger size. The strong upward movement which is known to exist in the neighborhood where hail is formed, is therefore quite sufficient to sustain hail stones of the largest kind as long as they can be kept within the influence of this vortex. After they have entirely escaped from the influence of the vortex, small stones would fall to the earth from an elevation of 5000 feet in about two minutes; and very large stones in one minute. I see no difficulty therefore in supposing the great mass of hail to remain in the air five minutes before reaching the earth—and that in peculiar cases, stones may remain supported for ten minutes and even a great deal longer. This period appears to me sufficient to account for the hail which fell at New York.

Why did the hail in the present case attain to such unusual size?

Because of the following circumstances which are unusually favorable to its formation. The temperature of the air before the storm was 90°, and it is my opinion that the dew-point could not have been less than 80°; in other words the atmosphere contained about as much vapor as it is ever known to contain in this latitude. This vapor was suddenly lifted to a region of great cold, and rapidly condensed and frozen. The strong upward movement helped to sustain the crystals as they increased in size, until the upward force was no longer equal to gravity—or until they escaped from the influence of the vortex. Most of the stones would fall in five minutes and be of moderate size; others might be sustained ten or fifteen minutes, and attain enormous dimensions.

How did the hail in this storm compare with the most remarkable cases on record.

There are well authenticated cases of hailstones having fallen in England and France weighing half a pound—and even more than this—but the accounts of hail stones weighing so much as one pound, do not appear to me entirely satisfactory. A mass of ice of the specific gravity 0·93, weighing eight ounces must contain nearly 15 cubic inches; or would make a cube whose edge is nearly 2·5 inches. I have selected a piece of ice which was estimated to be about the size of the largest stone which I saw fall on the 1st of July, and found it to weigh eight ounces. But these large stones of July 1st appeared to me unusually white, and may therefore be conjectured to have had a spongy nucleus—which would have reduced the weight to perhaps six ounces.

The hail therefore in the present storm was somewhat smaller than has been observed to fall in other places.

Since the preceding was written, I have received notice of several remarkable hail storms in different parts of the United States, two of which were so extraordinary that I have added an account of them to this paper.

Hail Storm experienced at Warren, N. H., Aug. 13, 1851.

My first information respecting this storm was derived from a letter from Dr. Peter L. Hoyt, dated Wentworth, Grafton Co., N. H., Aug. 3, 1853. The following is an extract from Dr. Hoyt's letter.

"Perhaps a brief notice of a hail storm which occurred in this vicinity on the 13th of August, 1851 may be of interest to you. This shower, about one o'clock, P. M., passed from the west towards the east over an extent of four or five miles around the base of Moosehillock Mountain, in the towns of Benton and Warren. The largest and most hail fell in the north east part of

the latter town, in a basin between the mountains near the source of a stream called Baker's river. I stood at the railroad depot in Wentworth, at the time of this shower, distant in an air line six or seven miles. It was the most remarkable appearing cloud I think I ever saw—so black and dense, encircling and covering the mountain, and shutting down to the earth.

"The hail was of prodigious size and in great quantities. The largest of the stones was of an irregular shape, rough and angular, suggesting the idea to some that they were made up of a number adhering together. They were however very solid and not easily broken.

"One was weighed upon the spot at the time of the shower, and weighed 20 ounces; and the person who informed me of this was of the opinion that he saw one fall and break in pieces which was still larger. They looked, he said, like vast pieces of ice that had been broken above, and were falling to the earth. A quantity was gathered in a basket and brought to Warren village, a distance of three or four miles, and there exhibited at least an hour after the shower, and in a hot and sultry afternoon. One of them there weighed 14 ounces, and measured 10 inches in circumference. Twelve of the largest taken out of the basket weighed on the counter scales in the store, seven pounds.

"About 4 o'clock, P. M., three hours after their fall, a box of them was brought to Wentworth village, where I reside, a distance of about eight miles. One of them was shown to me. Its diameter according to my best judgment was from 2 to 2½ inches. It had the appearance of being originally somewhat angular, with the angles melted off. It was perfectly solid and clear.

"So large was the quantity of hail in many places in Warren, that a cart load might have been gathered without moving from the place. Luckily the track of the storm was not through the most cultivated part of the towns, but along the borders and skirts of the forests, where the population was scattering. Crops of hay and grain were ground to the earth, poultry were killed—cattle's backs were bruised—and the roofs of many buildings were badly broken. But little glass was broken from the fact that the direction of the hail was nearly perpendicular to the earth."

Immediately upon receiving this letter, I wrote to Dr. Hoyt, stating that the facts which he had communicated respecting the size of the hail were so remarkable, that they ought to be substantiated by such evidence as would be deemed conclusive in a court of justice; that it was therefore important that he should obtain written statements from the identical persons who weighed the stones; and that it would not be derogatory to the dignity of science for these persons to make affidavit to the truth of their statements before a Justice of the Peace. I also suggested several

additional points upon which it was desirable that information should be obtained. In reply I received another letter from Dr. Hoyt accompanied by documents such as I had suggested. The following is extracted from his reply.

"As yet I have been unable to substantiate the weight of a hail-stone at 20 ounces; yet throughout the town of Warren the impression prevails that one was so weighed. The enclosed affidavit of Mr. Libby, and statement of Mr. Flanders fixes the extreme weight of two stones weighed by them at 17½ and 18 ounces; with the firm belief of Mr. Libby that had he weighed them at the time of falling, their weight would have been increased some two or three ounces.

"I have the names of some two or three other individuals who weighed several stones of a pound weight,—others were weighed weighing three fourths of a pound. One several hours after the storm weighed 14 ounces. A tin pail full containing fifteen, weighed 10 pounds—four collectively weighed three pounds, etc. Incredible as the above may appear to some, they are facts which can be proved by a multitude of evidence.

"This storm was remarkable for the amount of ice which fell as well as the size of the stones. Mr. Flanders thinks that in Benton the average depth of the hail was about four inches, and from enquiry along the track of the storm, I should judge that he is not far from right in his estimate. The extreme width of the hail was about two miles, and the length over a cultivated district perhaps about five or six miles. How far east it extended I have no means of knowing, as it entered a forest of many miles in width. The largest hail stones fell near the edge or skirts of its track; the thickest and greatest amount or depth of hail fell in the centre. Although the sun came out "boiling hot" as one man expressed it, after the shower, still the hail remained on the ground in many places until the next day. An owner of a saw mill, on the stream of water which has its source in the forests over which the shower passed, told me that the water kept swollen for two or three days, when from common showers of rain it would have fallen in twenty-four hours. This he attributed to the gradual melting of so large a quantity of ice in the woods. I think there was but little if any difference in the distribution of the large stones along the track, as the two whose weights are given by Mr. Flanders and Mr. Libby were picked up about five miles apart, and near the extremes of its track before it entered the forest. On the borders or skirts of the cloud the large stones fell scattering, and as it approached the centre it was as if the whole contents of the cloud were let down in ice. During the time of the hail, which lasted some twenty or thirty minutes, there was but little rain; after the hail it rained briskly for ten or fifteen minutes.

"*Shape of the hail.*—In Benton at the commencement of the hail, the masses were angular, having a resemblance to broken ice; while further along the track they assumed a smoother and more uniform surface, being oval or oblong. In many instances the surface is described as being notched or scolloped; and in some few as being covered with icy spikes, like icicles somewhat resembling a burr. It is the opinion of those who examined these stones the most minutely, that they were not formed by the union of several masses, but were distinct and individual in formation. They were compact and very solid; so hard that they might be thrown with great force against a house and not be broken.

"*Velocity.*—All agree that the hail fell with great velocity and force. Mr. E. W. Cleasby, a very correct and veracious man, whose statement is appended to this letter, says that hail stones very solid and weighing in the vicinity of 10 or 12 ounces, averaging one on about every two feet, fell in a piece of unmowed grass. In their passage through the grass they entangled it so as to carry it imbedded into the sward ground to the depth of some two or three inches, and after the melting of the hail, left the turf full of holes like little bird's nests. These holes remained through the season. As a test of the force necessary to effect this, he repeatedly with a pitch fork handle having a rounded head, tried to strike it into the ground to an equal depth, and was unable to do it.

"Many of the barns in this neighborhood have their roofs covered with what are styled 'long shingles'—that is with spruce shingles without previous boarding. Whenever these large stones fell upon such roofs, they broke a hole completely through; and one man having sought refuge in a barn under such circumstances, was obliged to hide under the scaffold. The marks and bruises upon the buildings caused by the hail are still to be seen. Says one person, 'they looked like little pumpkins falling.' The roar and rattle of the hail was distinctly heard at the village in Warren, a distance of four or five miles, and was likened to the noise of a heavy train of cars.

"*Wind.*—During the storm there was but little wind. The hail fell nearly perpendicularly. The general bearing of the wind as appears from my weather table on that day, was west-south-west; and the direction of the shower was in correspondence with this.

"*Heat.*—No record of the heat was kept in the neighborhood of the storm. My thermometer at 2 o'clock, P. M., indicated 76 degrees. I very well recollect that after witnessing the passage of this cloud to the north, the sun broke out very hot and scorching. Such also is the testimony of people living there.

"You ask if it was possible that the larger stones could have been formed by the cementing or freezing together of several while lying on the ground? I should think it *impossible* that such could be the case. Furthermore the general opinion among the inhabitants is that each stone was a unit in formation."

The following is a copy of the affidavit of Mr. Libby already referred to.

Warren, N. H., Aug. 24, 1853.

I live in Warren and witnessed the hail storm on the 13th of August 1851, between the hours of one and two o'clock, P. M. I weighed a number of the hail stones which fell at that time, but not until after the shower had ceased—perhaps an hour and a half or two hours after. During this time it was very hot. The largest which I weighed was 17¼ ounces in weight. The others varied in the vicinity of a pound. I am fully in the belief that had they been weighed at the time of falling, their weight would have been some two or three ounces more. Previous to weighing them I washed the dirt from them in water. They were very irregular in shape, somewhat scolloped, with ice projections from their surface. I picked these stones up from soft ploughed ground where they were imbedded more than half their size in the ground. During the time that the hail was falling there was but little rain, with little or no wind. After the hail there was a warm rain of some ten or fifteen minutes duration. JOHN LIBBY.

Sworn to before me, JESSE LITTLE, *Justice of the Peace.*

The following is the statement of Mr. Flanders already referred to.

Wentworth, Aug. 30, 1853.

I live in Benton, N. H., County of Grafton, and resided there at the time of the hail storm on the 13th of August, 1851. I weighed a number of the hail stones after the rain was over. The heaviest one weighed 18 ounces; the others ranged in the vicinity of a pound. They were very irregular in shape—some nearly square—some scolloped—some angular as if made up of several pieces. According to my best judgment there was an average depth of four inches of hail which fell at that time.

GRANVILLE E. FLANDERS.

The following is the statement of Mr. Cleasby.

Warren, N. H., Sept. 3, 1853.

This certifies that several of the hail stones which fell here on the 13th of August, 1851, were measured by members of my family. According to my best recollection the circumference of the largest was fourteen inches one way and nine the other. Their form was very generally oval. EZRA W. CLEASBY.

The preceding evidence satisfies me that hail stones fell in New Hampshire on the 13th of August, 1851, weighing *more than one pound*; and I do not know of any satisfactory evidence that hail of equal size has ever been seen in any other part of the world.

Hail Storm at Montrose, Iowa, on the Mississippi River, in Lat. 40° 30′, *about the middle of June*, 1838.

The following notice of this storm is derived from a letter received from Mr. D. W. Kilbourne, who resided at Montrose in 1838, but now lives at Keokuk, twelve miles below Montrose.

"About four o'clock in the afternoon, a very heavy black cloud rose in the northwest, the wind at the time blowing strong from that quarter. There was much thunder and lightning; at the same time it was clear in the east and southeast.

Very soon however the whole sky seemed to be covered by clouds; there was a heavy mist, and it was almost as dark as night. Rain immediately followed, and for a few moments fell in torrents. Then hail stones began to fall. At first they were small and excited no surprise in myself or family; but they continued to increase in quantity and size to such an extent as to excite not only our wonder but our fears. The hail storm continued nearly ten minutes, and during all the time small and large hail fell together. The wind was high.

As soon as the storm abated so that it was safe to go out, my family were all engaged in picking up the stones. We then selected the largest and measured their circumference. The largest one found *measured ten and one-fourth inches.* There were a large number that measured from two to ten inches in circumference. I gathered up with my own hands in one spot on the grass without moving, a half bushel measure full.

Mrs. Kilbourne placed several of the largest ones upon the top of common sized glass tumblers, and when melted they filled the tumblers so that some of them could not be moved without spilling the water.

The hailstones were irregular in their formation, and presented very much the appearance of rock-candy. The ice was solid and transparent. We did not weigh them.

The hail fell only about half a mile in width, and not more than two miles in length from west to east. No hail fell on the east side of the river. But few white families resided in the neighborhood at that time, or in the county; and I do not believe the hail stones were particularly noticed or measured except at our house."

ART. V.—*Description of a Tertiary Rainbow;* by CHARLES HARTWELL.*

ON the 28th of July, 1851, the writer observed, from the Theological Seminary, in South Windsor, Conn., what he judged to be a Tertiary Rainbow. After a heavy shower, and a little before sunset, the sun appeared, painting on the dark clouds in the east a beautiful primary bow. At the same time an appearance of decomposed light was seen in the N. W., upon a cloud of not very large dimensions, but from which rain was evidently falling. To the S. W., also, upon clouds somewhat separated, decomposed light was visible.

The appearance north of the sun was very bright, though in it were observed only the various shades of red and orange. It extended, according to my judgment, a degree or more in horizontal width, and from five to ten degrees upward. To the south the phenomenon was less brilliant, less in width, but distinctly traceable for some fifteen degrees from the horizon. Had these phenomena appeared in the east, no one would have doubted but that they constituted the two ends of a rainbow. The curvature of the colored light, and the correspondence in position, would have been sufficient proof. But as they were seen in the west, on the side with the sun, and tertiary bows are very rarely seen, it may be necessary to give the reasons which convinced me that I had really seen one. The phenomenon to the north was first observed, and filled the beholder with astonishment. What this appearance could be, so much more brilliant than ordinary views of the sun's shining on clouds, and then, too, not on the edge but near the middle while the rest appeared as clouds ordinarily do, at the same time no reason being manifest from the position of the cloud and sun and the state of the intermediate heavens why the sun should shine on that part rather than another, not a little puzzled him.

On going to another window, the phenomenon to the south was seen. From its greater length, curved form, and its position on the opposite side of the sun, the conclusion was immediately drawn that they were the two ends of a rainbow. Recalling some instructions of my former teacher, Prof. Snell, of Amherst

* *Messrs. Editors.*—The accompanying paper was prepared by Rev. Charles Hartwell, now connected with the mission in China, to be read at the meeting of the American Association in Albany, 1851. Being unable to attend himself, he sent it by a friend, who was, however, obliged to leave before the day assigned for its reading. Ill-health and pressing engagements prevented his giving further attention to it, till he was on his voyage to China; when, lighting upon the notes, he transmitted them to me, to be disposed of as I should think best. Though more than two years have elapsed since the phenomenon herein described was observed, I think the account of it ought, even now, to find a place in some public record.

Yours truly, E. S. SNELL.

Amherst College, Nov. 11th. 1853.

College, the thought flashed into my mind that this was a tertiary bow. Not recalling the dimensions of such a bow, I measured off the heavens as best I could, and judged the radius as seen to be about 40°. I have since learned that the radius by calculation is 40° 40′, so that my judgment, correct or incorrect, agrees very well with the true dimensions of the bow.

Having stated these facts to Prof. Snell, and requested a brief communication from him, he kindly furnished one, which I append to this paper.

"Amherst College, Aug. 15, 1851.

Since hearing you give an account of a phenomenon which you observed and supposed to be a tertiary rainbow, I have examined my notes upon the subject, and am well satisfied that you were not mistaken. All the circumstances forbid the supposition that it was a halo formed in prisms of ice. You estimated the distance of the arcs from the sun to be 40°; this differs but about half a degree from the radius of the tertiary bow, as determined by calculation. The arcs were seen also in masses of falling rain of such limited thickness, that the light might well be supposed to have been transmitted through the rain to the eye.

Though the appearance was not one of remarkable splendor, yet you may congratulate yourself, I think, on having witnessed a phenomenon of the most rare occurrence. The writer of the treatise on Optics, in the English Library of Useful Knowledge, speaking of the bows caused by *three or four* reflections in each drop of rain, says, 'none of these bows, however, have been seen.' Professor Forbes, of the University of Edinburgh, in his learned Report on Meteorology to the British Association, 1840, remarks, 'These, the ternary, quaternary, &c., rainbows, have been long theoretically known, though rarely, if ever, observed in nature. The ternary rainbow ought to be about 41° from the sun, but is generally stated to be too faint to be visible. Two observations by Bergmann are the only recorded ones I have met with. Kæmptz observed a ternary amidst the spray of the falls of Schauffhausen.'

Kæmptz, in his course of meteorology, after speaking of the bows of the third and fourth order, adds, 'but the intensity of these two latter is so feeble that they are rarely seen;' and puts in a note the following quotation, 'M. Babinet, when in the most favorable circumstances, on Mount d'Or, and on the Canigou, vainly endeavored to perceive them.'

It is obvious, as these quotations show, that the phenomenon which you witnessed is as rare as it is interesting. It seems to me highly probable that there is yet no public record of an instance in which an American observer has seen a rainbow on the same side of the heavens with the sun."

ART. VI.—*The Earl of Rosse's Telescopes, and their Revelations in the Sidereal Heavens;* by REV. W. SCORESBY, D.D., F.R.SS.L. & E., etc.*

IN a second lecture on these interesting subjects, recently delivered at Torquay, much and important consideration was given to the inquiry,—What has the gigantic telescope done?

The lecturer having himself had the privilege of observing on different visits, and for considerable periods, with both the instruments, was enabled to reply, he hoped in a satisfactory manner, to this inquiry. His opportunities of observing, he said, notwithstanding interruptions from clouds and disturbed atmosphere, had been somewhat numerous, and, not unfrequently, highly instructive and delightful. Of these observations he had made records of nearly 60, on the moon, planets, double stars, clusters, and nebulæ. He had been permitted also to have free access to, and examination of, all the observatory records and drawings, so that he was enabled on the best grounds, he believed, to say, that there had been no disappointment in the performance of the instruments; and that the great instrument, in its peculiar qualities of superiority, possesses a marvellous power in collecting light and penetrating into regions of previously untouched space. In what may be called the domestic regions of our planet—the objects in the solar system—all that other instruments may reveal is within its grasp or more, though by the prodigious flood of light from the brighter planets, the eye is dazzled unless a large portion is shut out.

But in its application to the distant heavens and exploration of the nebulous systems there, its peculiar powers have, with a steady atmosphere, their highest developments and noblest triumphs. In this department—that to which the instrument has been particularly directed—every known object it touches, when the air is favorable, is, as a general fact, exhibited under some new aspect. It pierces into the indefinite or diffuse nebulous forms shewn by other instruments in a general manner, and either exhibits configurations altogether unimagined, or resolves perhaps the nebulous patches of light into clusters of stars. Guided in the general researches by the works of the talented and laborious Herschels—to whom astronomy and science owe a deep debt of gratitude—time has been economized, and the interests of the results vastly enhanced. So that many objects in which the fine instruments of other observers could discern only some vague indefinite patch of light, have been brought out in striking, definite, and marvellous configurations.

Among these peculiar revelations is that of the *spiral* form—the most striking and appreciable of all—which we may venture to designate "*The Rossean Configuration.*" Its discovery was

* An abstract of a Lecture delivered at Torquay, November 15, 1852.—From the Edinburgh New Philosophical Journal for January, 1853.

at once novel and splendid; and in reference to the dynamical principles on which these vast aggregations of remote suns are whirled about within their respective systems and sustained against interferences, promises to be of the greatest importance.

One of the most splendid nebulæ of this class—the *great spiral* or *whirlpool*—has been figured in the Philosophical Transactions for 1850. It may be considered as the grand type and example of a class; for near 40 more, with spiral characteristics, have been observed, and about 20 of them carefully figured. Dr. Scoresby had the pleasure of being present at the discovery of this particular form in a nebula of the planetary denomination, in which two portions following spiral forms were detected. Its color was peculiar,—pale blue. He had the privilege, too, of being present on another interesting occasion, when the examination of the great nebula in Orion was first seen to yield decisive tokens of resolution.

In these departments of research, the examination of the configurations of nebulæ, and the resolution of nebulæ into stars, the six-feet speculum has had its grandest triumphs, and the noble artificer and observer the highest rewards of his talents and enterprise. Altogether, the quantity of work done, during a period of about seven years,—including a winter when a noble philanthropy for a starving population absorbed the keenest interests of science,—has been decidedly great, and the new knowledge acquired, concerning the handiwork of the Great Creator, amply satisfying of even sanguine anticipations.

Dr. Scoresby found, in September last, that about 700 catalogued nebulæ had been already examined, and transferred to the ledger records from the journals of the Observatory, (comprising only a selection from the general observations,) and the *new nebulæ*, or nebulous knots, discovered merely incidentally, amounted to 140 or more. The number of observations, involving separate sets of the instrument, recorded in the ledger, (exclusive of very many hundreds, possibly thousands, on the moon and planets,) amount to near 1700, involving several hundreds of determinations of position and angular measurements with the micrometer on the far distant stars. The carefully drawn configurations, eliciting *new characteristics*, exceed 90, and the rough or less-finished sketches amount to above 200. Of the 700 catalogued nebulæ already examined, it should be observed, that in full one-half or more, something new has been elicited.

In speaking of the effects of the flood of light accumulated by the six-feet speculum of the Earl of Rosse, Dr. Scoresby remarked, that this peculiarity of the instrument (connected as it is with due length of focus and admirable definition) enabled it to reach distances in space far beyond the powers of any other instrument. This was its peculiar province; and in this, as to existing instruments, there was not, nor, as he hoped to shew, could there be,

any competition. For comparing the space-penetrating power of the six-feet speculum with one of two feet (which has rarely been exceeded) we find it three to one in favor of the largest, with an accumulation of *light* in the ratio of 6^2 to 2^2, or 9 to 1. On comparing the powers of this magnificent instrument with those of a refractor of two feet aperture, the largest hitherto attempted, we have a *superiority*—making a due allowance for the loss of light by reflection from two mirrors, and assuming an equal degree of perfectness, figure, and other optical requirements in the refractor, and *no* allowance for absorption of light—in the ratio of about 4·5 to 1, as to light, and as 2·12 to 1, as to the capability of penetrating space, or detecting nebulous or sidereal objects at the extreme distance of visibility. Hence, whilst the range of telescopic vision in a refractor of two feet aperture would embrace a *sphere* in space represented by a diameter of 2; the six-feet speculum (assuming both instruments to be of equal optical perfection, magnifying equally, and allowing fifty per cent. for loss of light for two reflections in the one case, and none (?) in the other) would comprehend a sphere of about 4·24 diameter,—the outer shell of which, 1·12 in thickness, being the province of the great instrument alone. But let us reduce these proportions to *sections* of equal spaces, that we may judge more accurately of the relative powers. Now, the solid contents of different spheres, we know, are in the ratio of the cubes of their diameters. Hence the comparative spheres, penetrated by the two instruments rereferred to, should be $4{\cdot}24^3$ to 2^3; that is, as 9·5 to 1. Deducting, then, from this vast grasp of space the inner sphere, capable of being explored by other instruments, we find that, out of nearly ten sections of space reached by this telescope, there are nearly nine sections which the six feet speculum may embrace as peculiarly its own!

What its revelations yet may prove, then, we can have no idea. Several thousands of nebulæ have been catalogued: the great reflector might add to these tens of thousands more. But this, seeing how few nights in a year are favorable for the highest powers, must be the work of years of perseverance. It would be a worthy undertaking for the government of a great country, to afford the means of multiplying such gigantic instruments. Application is to be made, in this direction, for a six-feet reflector at the Cape of Good Hope, for the examination of the heavens towards the southern pole. Lord Rosse, with his usual nobleness of liberality, will yield up his laboratory, machinery, and men, to the service of government, and is willing, moreover, to give the direction and guidance of his master-mind. Will the British nation be content with a refusal?

The range opened to us by the great telescope at Birr Castle, is best, perhaps, apprehended by the now usual measurement—not of distances in miles, or millions of miles, or diameters of the

earth's orbit, but—of the progress of light in free space. The determination, within, no doubt, a small proportion of error, of the parallax of a considerable number of the fixed stars, yields, according to M. Peters, a space betwixt us and the fixed stars of the smallest magnitude, the sixth, ordinarily visible to the naked eye, of 130 years in the flight of light. This information enables us, on the principles of *sounding the heavens*, suggested by Sir W. Herschel, with the photometrical researches on the stars of Dr. Wollaston and others, to carry the estimation of distances, and that by no means on vague assumption, to the limits of space opened out by the most effective telescopes. And from the guidance thus afforded us, as to the comparative power of the six feet speculum in the penetration of space, as already elucidated, we might fairly assume the fact, that if any other telescope now in use could follow the sun if removed to the remotest visible position, or till its light would require 10,000 years to reach us, the grand instrument at Parsonstown would follow it so far, that from 20,000 to 25,000 years would be spent in the transmission of its light to the earth. But in the cases of clusters of stars, and of nebulæ exhibiting a mere speck of misty luminosity, from the combined light perhaps of hundreds of thousands of suns, the *penetration* into space, compared with the results of ordinary vision, must be enormous; so that it would not be difficult to shew the *probability* that a million of years, in flight of light, would be requisite, in regard to the most distant, to trace the enormous interval.

But after all, what is all this, vast as the attainment may seem, in the exploration of the extent of the works of the Almighty? For in this attempt to look into space, as the great reflector enables us, we see but a *mere speck*—for SPACE IS INFINITE. Could we take, therefore, not the tardy wings of the morning, with the speed of the mere spread of day, nor flee as with the leaden wings of light, which would require years to reach the nearest star, but, like unhampered *thought*, could we speed to the farthest visible nebula at a bound,—there, doubtless, we should have a continuance of revelations; and if bound after bound were taken, and new spheres of space for ten thousand repetitions explored, should we not probably find each additional sphere of telescopic vision garnished with suns and nebulous configurations rich and marvellous as our own? If these views serve to enlarge our conception of creative wonders, and of the glory and power of the Great Architect of the heavens, should they not deeply impress us in respect to the Divine condescension in regarding so graciously this little, inferior world of ours! Animated with the spirit of the Psalmist, we shall each one, surely, be disposed appropriately to join in his emphatic saying,—"When I *consider* thy heavens, the work of thy fingers, the moon and the stars which thou hast ordained; what is man, that thou art mindful of him? or the son of man, that thou visitest him?"

Art. VII.—*Researches on the Development of Viviparous Aphides;* by Waldo I. Burnett, M.D., Boston.

Every naturalist is aware of the remarkable phenomena connected with the viviparous reproduction of Aphides or plant-lice, for their singularity has led them to be recounted in works other than those of natural science, and, from the days of the earlier observers, they have been the theme of a kind of wonder-story in zoology and physiology.

I need not here go over the historical relations of this subject. The queer experiments and the amusing writings of the old Entomologists are well known. The brief history of the general conditions of the development of these insects is as follows: In the early autumn the colonies of plant-lice are composed of both male and female individuals; these pair, the males then die, and the females soon begin to deposit their eggs, after which they die also. Early in the ensuing spring, as soon as the sap begins to flow, these eggs are hatched, and the young lice immediately begin to pump up sap from the tender leaves and shoots, increase rapidly in size, and in a short time come to maturity. In this state it is found that the whole brood, without a single exception, consists solely of females, or rather and more properly, of individuals which are capable of reproducing their kind. This reproduction takes place by a viviparous generation, there being formed in the individuals in question, young lice which, when capable of entering upon individual life, escape from their progenitor and form a new and greatly increased colony. This second generation pursues the same course as the first, the individuals of which it is composed being like those of the first, sexless, or at least without any trace of the male sex throughout. These same conditions are then repeated, and so on almost indefinitely, experiments having shown that this power of reproduction under such circumstances may be exercised, according to Bonnet,* at least through nine generations, while Duvau† obtained thus, eleven generations in seven months, his experiments being curtailed at this stage, not by a failure of the reproductive power, but by the approach of winter which killed his specimens; and Kyber‡ even observed that a colony of *Aphis dianthi* which had been brought into a constantly heated room, continued to propagate for four years, in this manner, without the intervention of males, and even in this instance it remains to be proved how much longer these phenomena might have been continued.

The singularity of these results led to much incredulity as to their authenticity, and on this account the experiments were often

* *Bonnet,* Traité d'Insectologie, ou observations sur les Pucerons, 1745.
† *Duvau,* Mém. du Mus. d'Hist. Nat., xiii, p. 126.
‡ *Kyber,* *Germar's* Magaz. d. Entomol., 1812.

and carefully repeated; and there can now be no doubt that the virgin *Aphis* reproduces her kind—a phenomenon which may be continued almost indefinitely, ending finally in the appearance of individuals of distinct male and female sex, which lay the foundation of new colonies in the manner just described.*

The question arises, what interpretation is to be put upon these almost anomalous phenomena? Many explanations have been offered by various naturalists and physiologists, but most of them have been as unsatisfactory as they have been forced, and were admissible only by the acceptance in physiology of quite new features.

As the criticism I intend to offer upon some of these opinions, will be the better understood after the detail of my own researches, I will reserve their future notice until the concluding part of this paper.

My observations were made upon one of the largest species of *Aphis* with which I am acquainted, the *Aphis Caryæ* of *Harris.*† While in Georgia, this last spring, it was my good fortune that myriads of these destroyers appeared on a hickory which grew near the house in which I lived. The number of broods on this tree did not exceed three, for with the third series their numbers were so great that their source of subsistence failed and they gradually disappeared from starvation. The individuals of each brood were, throughout, of the producing kind, no males having been found upon the closest search; they were all, moreover, winged; and those few which were seen without these appendages appeared to have lost them by accident. I mention this fact especially, since it has been supposed by naturalists that the females were always wingless, and therefore that the winged individuals, or the males, appeared only in the autumn.‡

The first brood, upon their appearance from their winter hiding-places, were of mature size, and I found in them the developing germs of the second brood quite far advanced. On this account it was the embryology of the third series or brood alone, that I was able to trace in these observations.

* For details of experiments by which *Bonnet's* original results were verified, see *Réaumur*, Mémoires, iii, Mém. 9 and 11, and vi, Mém. 13. Also,
Degeer, Mémoires, iii, ch. 2, 3.
Curtis, Trans. Linn. Soc., vi. Philos. Trans. 1771.
Sauvages, Journ. de Physique, i.
Dutrochet, Mémoires, ii, p. 442. See also the more modern writers, and especially *Kirby* and *Spence*, Introduction to Entomology, iv. p. 161.

† *Harris*, A treatise on some of the Insects of New England which are injurious to Vegetation. 2nd ed. 1852. p. 208. As *Dr. Harris* says, it is probably *Lachnus* of Illiger, (*Cinara* of Curtis.)

‡ See *Westwood*, An introduction to the modern Classification of Insects, &c. London, 1839. ii, p. 438—but especially
Owen, Parthenogenesis, &c., p. 23, note, and p. 59, note, where he says, "Many of the virgin viviparous Aphides acquire wings, but never perfect the generative organs!"

A few days after the appearance of these insects, the individuals of second brood (B), still within their parents (A), had reached two-thirds of their mature size. At this time the arches of the segments of the embryo had begun to close on the back, and the various external appendages of the insect to appear prominently; the alimentary canal had been more or less completely formed, although distinct abdominal organs of any kind belonging to the digestive system were not very prominent. At this period, and while the individuals of generation B, are not only in the abdomen of their parent A, but are also enclosed, each, in its primitive egg-like capsule,—at this time, I repeat, appear the first traces of the germs of the third brood (C).

These first traces consist of small egg-like bodies arranged, two, three, or four in a row, and attached in the abdomen at the locality where the ovaries are situated in the oviparous forms of these animals.

These egg-like bodies consisted either of single nucleated cells, of $\frac{1}{3000}$ of an inch in diameter, or, a small number of such cells enclosed in a simple sac. These are the germs of the third generation; they increase with the development of the embryo in which they have been formed, and this increase of size takes place not by a segmentation of the primitive cells, but by the endogenous formation of new cells. After this increase has gone on for a certain time, these egg-like bodies appear like little oval bags of cells—all these component cells being of the same size and shape, there being no cell which is larger and more prominent than the rest, and which could be comparable to a germinative vesicle. While these germs are thus constituted, the formation of new ones is continually taking place. This occurs by a kind of constriction-process of the first germs, one of their ends being pinched off, as it were, and in this way what was a single sac, is changed into two which are attached in a moniliform manner. The new germ thus formed, may consist of even a single cell only as I have often seen, but it (the germ) soon attains a more uniform size by the endogenous formation of new cells within the sac by which it is enclosed. In this way the germs are multiplied to a considerable number, the nutritive material for their growth being apparently a fatty liquid with which they are bathed, contained in the abdomen, and which is thence derived from the abdomen of the first parent.

When these germs have reached the size of $\frac{1}{300}$ of an inch in diameter, there appears on each, near one end, a yellowish, vitellus-looking mass or spot, which is composed of large, yellowish cells, which in size and general aspect, are different from those constituting the germ proper. This yellow mass increases *pari passu* with the germ, and at last lies like a cloud over and concealing one of its poles. I would also insist on the point that it

does not extend itself gradually over the whole germ-mass, and is therefore quite unlike a true germinative vesicle or a proligerous disc. When the egg-like germs have attained the size of $\frac{1}{135}$ of an inch, there distinctly appears the sketching or marking out of the future animal. This sketching consists at first of delicately-marked retreatings of the cells here and there, but which soon become more prominent from furrows, and at last the whole form of the embryo stands boldly out. As the whole idea and form of the insect is thus moulded out of a mass of cells, it is evident that the separate parts which then appear, such as the arches of the segments, the extremities and the oval apparatus, consist at first of only rows of simple cells. This point is here beautifully prominent, and nowhere have I observed finer illustrations of the cell-constitution of developing forms.

The development thus proceeding, each part of the dermo-skeleton becomes more and more distinct, and the increase of size of the whole is attained by the constant development of new cells. During this time, the yellow vitellus-looking mass, situated at one of the poles of the embryo, has not changed its place; it has increased somewhat in size, but otherwise appears the same. When the development has proceeded somewhat farther, and the embryo is pretty well formed, the arches of the segments, which have hitherto remained gapingly open, appear to close together on the back, thereby enclosing this vitellus-looking mass within the abdominal cavity.

It is this same vitelloid mass thus enclosed that furnishes the nutritive material for the development of new germs which would be those of the fourth brood or D; this development of germs here commences with the closing up of the abdominal cavity, and the same processes which we have just described are again repeated.

The details of the development subsequent to this point, are like those of the development of ordinary insects or of the Articulata in general; and although this ovoid germ has at no time the structural peculiarities of a true ovum—such as a real vitellus, a germinative vesicle and germinative dot; yet, if we allow a little latitude in our comparison and regard the vitellus-looking mass as the *mucous*, and the germ-mass proper as the *serous* fold of the germinating tissue, as in true eggs—if, I repeat, we can admit this comparison of parts, then the analogy of development between these germs and true eggs of insects, may be traced in considerable detail.

This comparison I have been inclined to admit at least in part, from the striking resemblance of these developing forms at certain stages, with the embryological forms of spiders as they have been figured by Herold* and as I have myself traced them.

* *Herold*, De Generatione Aranearum in ovo. Marbourg, 1824.

When, in spiders, the serous fold of the germinating tissue has extended so as to cover two-thirds of the developing form, leaving the vitelline mass on the dorsal surface near one of the poles, the whole embryo quite resembles that of a developing Aphis just before the arches of the segments close up on the back.

With this view of the relative parts of the germ, the following would be the details of the development of the different systems, and in the noticing of which I shall follow Kölliker.*

1. The germinating tissue consists of two parts; a serous and mucous fold.

2. The abdominal plates arise from the serous fold, sprout out towards the vitelloid mass, pass over it and unite on the dorsal surface of the future animal; on the opposite side are formed plates which do not unite, but are formed into the hind legs.

3. The wings are the lateral limbs.

4. The first traces of the abdominal column appear in the chain of abdominal muscles, situated between the nerves and the intestinal canal.

5. The nervous system in all its parts arises from the serous fold, as well also as the organs of sense.

6. The mucous fold or the vitellus-looking mass, serves no purpose in the formation until the closing in of the visceral plates.

7. Thus enclosed in the abdominal cavity, it is not transformed directly into the intestinal canal, but simply furnishes the material from which the component cells of said canal and its hepatic deverticula are formed. It also furnishes the material from which the new germs are formed, as already shown.

8. The heart is formed on the dorsal aspect between the mucous and serous folds. In this way the details of development closely correspond with those of the embryology of the other Articulata which I have studied; and the subject is all the more interesting as the germ-masses, from which such development occurs, in no way and at no time structurally resemble true eggs.

When the embryo is ready to burst from its developing capsule and make its escape from the abdomen of its parent, it is about $\frac{1}{16}$th of an inch in length, or more than eight times the size of the germ at the time when the first traces of development were seen. From this it is evident that, even admitting that these germ-masses are true eggs, the conditions of development are quite different from those of the truly viviparous animals; such as for instance in *Musca*, *Anthomyia*, *Sarcophaga*, *Tachina*, *Dexia*, ***Miltogramma***, and others among Dipterous insects;† or in the vivipa-

* *Kölliker*, Observationes de prima Insectorum genesi adjecta articulatorum evolutionis cum Vertebratorum comparatione. Diss. Inaug. Scr. Alb. Kölliker. Turin, 1812.

A work replete with facts and interesting suggestions.

† See *Siebold* in *Frorieps* neue Notiz. iii, p. 337, and in *Wiegmann's* Arch. 1838, i, p. 197.—also his Observat. quaed. Entom., &c., p. 18.

rous reptiles,—for in all these cases of ordinary viviparity, the egg is simply hatched in the body instead of out of it. The egg moreover, is formed exactly in the same way as though it was to be deposited, and its vitellus contains all the nutritive material required for the development of the egg until the coming forth of the new individual. The abdomen of the mother serves only as a proper indus or incubatory pouch for its full development. This is true of all the the ovo-viviparous animals whatsoever.* With the viviparous Aphides, on the contrary, the developing germ derives its nutritive material from the fatty liquid in which it is bathed, and which fills the abdomen of the parent.† The conditions of development here therefore are more like those in Mammalia, and the whole animal may, in one sense, be regarded as an individualized uterus filled with germs, for the digestive canal, with its appendages, seems to serve only as a kind of laboratory for the conversion of the succulent fluids which the animal extracts from the tree on which it lives, into this fatty liquid from which the increase and development of the germs take place.

When the young animal has reached its full development as an embryo, it bursts from its encasement and appears to escape from the abdomen of its parent through a small opening *(Porus genitalis)* situated just above the anus. In the species under consideration it generally remains clinging on the back of the parent until its external parts are dry and it is able to begin life for itself. Each parent here produces from eight to twelve individuals, and if this rapid increase is continued undisturbed, through seven to nine broods, we cannot wonder at the countless numbers which appear from so few original individuals.‡

Such are the details of the embryological development of the so-called viviparous Aphides, as far as I have enjoyed opportunities for their study. We will now refer for a moment to the special points which have here been made out. In the first place, it is evident that *the germs which develop these forms are not true eggs.* They have none of the structural characteristics of eggs, such as a vitellus, a germinative vesicle and dot; on the other

* It is true that in the Scorpionidae, the eggs are developed in the ovary, but there is no reason to suppose that the conditions are here different from those of the viviparous Diptera.

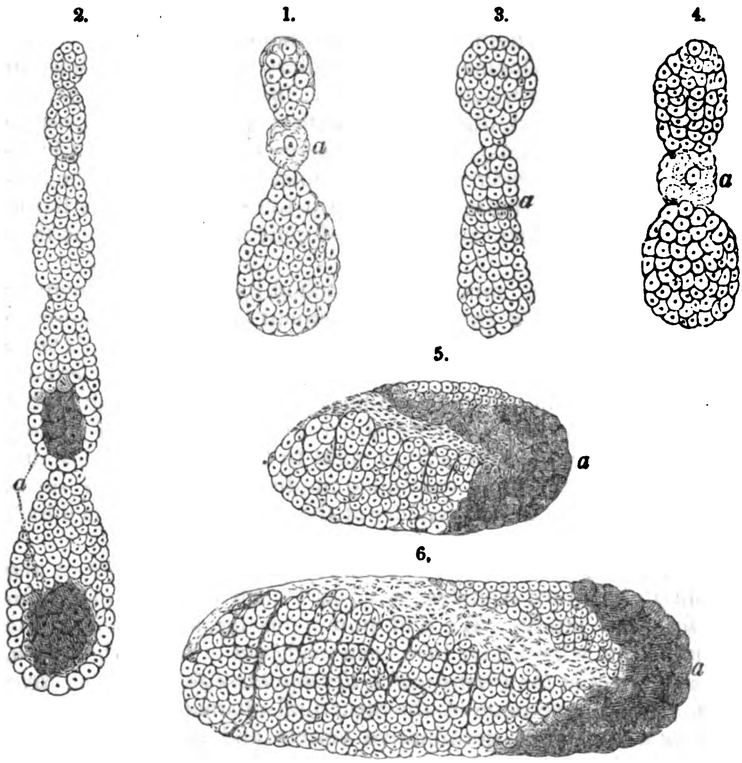
In *Oribates*, also, the eggs are developed in a kind of uterus situated directly above the ovipositor, but this appears to be only an incubatory pouch.

† This fatty matter forms beautiful crystals of margarine, and the crystallization may well be seen to take place. The forms exactly resemble those given by *Robin* and *Verdeil*, Traité de Chim. Anat et Physiol. Paris, 1853. Pl. XXXVIII, fig. 2 *h*.

‡ *Réaumur* has shown that in these animals the rate of increase is so great that in five generations or broods only one Aphis may be the progenitor of five billion, nine hundred and four million, nine hundred thousand (5,904,900,000) descendants; we may well ask what would be the number of descendants where the broods were extended to eleven!!

See *Kirby* and *Spence*, Introduction to Entomology, i, p. 175.

hand, they are, at first, simple collections, in oval masses, of nucleated cells. Then again, they receive no special fecundating power from the male, as is the necessary preliminary condition of all true eggs; and, furthermore, the appearance of the new individual is not preceded by the phenomena of segmentation, as also is the case with all true eggs. Therefore their primitive formation, their development, and the preparatory changes they undergo for the evolution of the new individual, are all different from those of real ova.*



EXPLANATION OF FIGURES.

Figs. 1, 2, 3 and 4, represent the egg-like buds in different stages of development—and as they are successively formed sprouting off from the internal surface of the parent.

Fig. 1, *a*, shows a bud consisting of a single cell and situated between two larger. The same is true of

Fig. 4, *a*, where, however, new cells are about to be developed around the central one.

Fig. 3, *a*, shows the formation of a new bud by a constriction process.

Fig. 2, *a*, shows first appearance of the vitellus-looking mass near one extremity of bud.

Fig. 5, shows the appearances of the marking out of the embryo from the germ-mass.

Fig. 6. Further development and different parts appearing. *a*, represents vitelloid mass, in both figures.

* *Milne Edwards* thinks he has found true *ova* and ovaries in the viviparous forms of these animals. (Quoted by *Dr. Carpenter* in Brit. and For. Med. Chir. Rev., 1849, iv, p. 443.) I think he must have been deceived, as I was at first, by the general appearances which, unless carefully examined, closely resemble those of true oviparous individuals.

Another point is, *these viviparous individuals have no proper ovaries and oviducts.* Distinct organs of this kind I have never been able to make out. The germs are situated in moniliform rows, like the successive joints of confervoid plants, and are not enclosed in a special tube. These rows of germs commence, each, by a single germ-mass which sprouts from the inner surface of the animal, and which increases in length and in the number of its component parts from the successive formation of new germs by a constriction-process as already mentioned. Moreover, these rows of germs which, at one period, closely resemble in general form the ovaries of some true insects, are not continuous with any uterine or other female organ, and therefore do not at all communicate directly with the external world. On the other hand, they are simply attached to the inner surface of the animal, and their component germs are detached into the abdominal cavity as fast as they are developed, and then escape outwards through a *Porus genitalis*, exactly as is the case with the eggs of fishes.* Here, then, comes the important question, What interpretation shall be put upon these reproductive parts—these moniliform rows of germs? Ignoring all existing special theories relating to reproduction, the observing physiologist would be left no alternative but to regard them as *buds*, true gemmæ, which sprout from the inner surface of the Aphis, exactly like the buds from the external skin of a Polyp.†

Before proceeding to a discussion of the relations of this important conclusion to which we have just arrived, it may be well to refer to the views of others upon the exact signification of these singular reproductive phenomena.

Those old entomologists, such as Bonnet, Réaumur, Degeer, &c., who were the first to observe, besides verifying beyond all doubt, these peculiar phenomena, all believed that each brood constitutes a separate generation, and that the reproduction takes place by true ova, as in the common generative act of other insects. This wide deviation from the ordinary course of nature as it seemed to them, they attempted to explain and reconcile by various theories. Thus, Réaumur‡ affirmed that these viviparous individuals were androgynous; and, in later times, Leon Dufour,§ who knew too well the anatomical structures of insects to believe with Réaumur that they could be hermaphrodites, referred these phenomena to spontaneous or equivocal generation.

* These observations of mine on the special anatomy of the reproductive parts of viviparous Aphides, agree with those of *Siebold*, who studied the subject with much care several years since. See *Froriep's* neue Not. xii, p. 308. *Siebold*, however, regarded them as true ovaries and oviducts, but without any of the usual appendages which are found in the true oviparous Aphides.

† I would insist upon this wide and important distinction between buds and ova. The structure and conditions of all ova are the same, and there is no passage between them and buds. But this point will be enlarged upon hereafter.

‡ *Réaumur*, loc. cit. Mém's.

§ *Léon Dufour*, Recherches Anat. et Physiolog. sur les Hémiptères, Paris, 1833.

Morrem,* who made somewhat extended researches on the anatomy of *Aphis persicæ*, and especially of its generative organs, advanced the novel theory, that these broods were developed in the body of the virgin parent, by a previously organized tissue becoming individualized and assuming an independent life, exactly, as he believed, to be the case with Entozoa. To each and all of these views, it scarcely need be said that they would be wholly inadmissible according to the present established doctrines of physiological science, even had we no directly controverting observations.

But, there are other explanations or views which deserve more attention. The first of these is that advanced by Kirby and Spence.† According to them "One conjunction of the sexes suffices for the impregnation of all the females that in a succession of generations spring from that union." In support of the reasonableness of this hypothesis, they quote several instances which they regard as of analogous character; thus, they say in regard to the hive-bee, that "a single intercourse with the male fertilizes all the eggs that are laid for the space of two years."

In this connection should be mentioned the similar hypothesis advanced for a like purpose by Jourdan.‡ According to him many Lepidoptera lay fertile eggs when completely isolated from the males: such are, *Euprepsia casta*, *Episema cæruleocephala*, *Gastropacha potatoria*, *G. quercifolia*, and *G. pini*, *Sphinx ligustri*, *Smerinthus populi* and *Bombyx querci.*

But, all these cases have really no strict analogy with that of the Aphides in question; for there is not, as with these last, a succession of innately fertile individuals, but only females which are capable of producing several broods from a single coitus, or after having been long removed from the males which may even then be dead.§ Late researches upon the minute anatomy of the generative organs of insects have furnished results by which these phenomena, seemingly strange at first, can be explained. All these insects which are thus capable of laying fecundated eggs again and again after the first impregnation, have a *Receptaculum*

* *Morrem*, Anat. de l'Aphis persicæ, in the Ann. d. Sc. Nat., v. 1836, p. 90.

† *Kirby* and *Spence*, Introduction to Entomology, iv. p. 161.

‡ *Jourdan*, Manuel de Physiologie, par J. Müller, Trad. de l'Allemand. etc., par A. J. L. Jourdan. Deux. éd. rev. et annots., par E. Littré, ii, p. 599, note.

§ *Siebold* has made observations upon allied phenomena occurring in the Psychidae, which are of no little interest. He has shown that in the genera *Psyche* and *Fumea*, the alleged reproduction, *sine lusina*, is unfounded—these insects having well formed internal genital organs, and the male being adapted to impregnate the female while the latter is in her case. But in the genus *Talæporia*, *Siebold* has shown that there is propagation *sine concubitu*, exactly as occurs with the Aphides. See Ueber die Fortpflanzung der Psyche: Ein Beitrag zur Naturgeschichte der Schmetterlinge, in *Siebold & Kölliker's* Zeitsch. i, 1849. p. 93; but, for his last researches on *Talæporia*, see his Bericht üb. d. entomol. Arbeiten d. schles. Gesellsch im J. 1850—or its English transl. in the Trans. of the Ent. Soc. N. S., i, p. 234.

seminis connecting with the oviduct, in which the semen is deposited during coition and where it may be preserved without losing its vitalizing power, for several months.* Thus, by this provision, the males, having copulated with the females in the autumn, may immediately die, while these last, hibernating, produce in the spring, fertile ova; and in the instance of the *Bombus americana*, such a coition suffices for all the three broods which are produced the ensuing summer.

Another explanation of these curious phenomena, and which has attracted some attention, as well from its singularity as from the eminence of its propounder, is that of Owen, advanced in his Hunterian Lectures in 1843.†

He affirms that the larval Aphides are productive in virtue of the successive continuation from brood to brood of a portion of the primitively fertilized germ, and which material product or leaven is not exhausted until nine to eleven generations. I will quote his own language: "In the Aphides the corresponding vitelline cells retain their share of the fecundating principle (which was diffused through the parent egg by the alternating, fissiparous, liquefactive, and assimilative processes) in so potent a degree, that a certain growth and nutritive vigor in the insect, suffice to set on foot in the ovarian, nucleated cells, a repetition of the fissiparous and assimilative process, by which they transform themselves in their turn into productive insects; and the fecundating force is not exhausted by such successive subdivision until a 7th, 9th, or 11th generation." This same doctrine, the successive inheritance of a portion of the primary germ-mass from brood to brood, and by means of which the fertile germs are continued, this doctrine, I say, is repeated in full in this author's work on Parthenogenesis, and I will here quote one sentence, not only in illustration of this, but to show how different his own observations on the development of these animals, are from mine, just described. He says, "One sees such portion of the germ-mass taken into the semi-transparent body of the embryo Aphis, like the remnant of the yolk in the chick. I at first thought it was about to be enclosed in the alimentary canal but it was not so. As the embryo grows, it assumes the portion of the ovarium, and

* For many details on this subject of the *Receptaculum seminis*, see *Siebold, Müller's* Arch., 1837, p. 392; also in *Wiegmann's* Arch., 1839, i, p. 107, (*Vespa*), and in *Germars* Zeitsch., ii. 1840. p. 442, (*Culex*). See also *Stein*, Vergleich. Anat. &c. 1847. p. 96, 112.

I cannot but believe that the anomalous reproductive conditions of the Cynipidæ will, at last, have a solution equally as satisfactory. See *Hartig, Germars* Zeitsch., ii, p. 178, and iv, p. 395. See also *Siebold* and *Stannius* Comparative Anatomy, transl., i, § 348, notes 1 and 4.

† Lectures on the Comparative Anatomy and Physiology of the Invertebrate Animals, &c. London, 1843, p. 238. This explanation is lately insisted upon (strange to relate) in his recent work "On Parthenogenesis, or the successive production of procreating individuals from a single Ovum." London, 1849.

becomes divided into oval masses and enclosed by the filamentary extremities of the eight oviducts. Individual development is checked and arrested at the apterous larval condition. It is plain, therefore, that the essential condition of the development of another embryo in this larva is the retention of part of the progeny of the primary impregnated germ-cell."—p. 70.

This view of Owen, so ingeniously advanced, and which he has made subservient for the chief support of his new doctrine of Parthenogensis, is indeed plausible and seems at first satisfactory: but, as I hope to show, it will not bear analysis.

In the first place, it is evident that Owen does not recognize any physiological difference between a *bud* and an *ovum;* this is clear from what he remarks in the first quotation, but in his work on Parthenogenesis he has said so in as many words. "The growth by cell-multiplication producing a bud, instead of being altogether distinct from the growth by cell-multiplication in an egg, is essentially the same kind of growth or developmental process.—p. 45.

Here is a fundamental error which, if not removed, will obscure all our views of the physiology of reproduction. I have already insisted upon the necessity of this broad distinction between these two forms, a necessity based not only upon differences of anatomical constitution, but also upon physiological signification. An *Ovum* is the exclusive product of an individual of the female sex, and is always formed in a special organ called the ovary. It is the particular potential representative of the female, and has its ulterior development only from its conjunction with a corresponding element of the opposite or male sex; and zoology presents no instance where there is development from eggs, unless these conditions of the two sexes are fully carried out.

A *Bud*, on the other hand, is simply an offshoot from the form on which it rests, a portion of the animal capable of individual development. It sustains, therefore, no relations to sex, and, in truth, is widely separated in its ulterior signification from that cycle of processes conceived in a true oviparous reproduction.

All physiologists who have carefully studied embryological and developmental processes, must feel the correctness and importance of this distinction which lies in realities and not in words.

It is true that a bud and an ovum are composed each of the same elements,—simple nucleated cells; but in one, these cells are simply in a mass, while in the other, they have, throughout the animal kingdom, high or low, a definite and invariable arrangement. Then again as to the constitution of each and both being, on the whole, of nucleated cells, it may be said, that it could hardly be conceived to be otherwise, for nucleated cells are the elementary components of all functional organized forms; and it may be added, moreover, that he knows little of the high-

est physiology who has not learned that widely different teleological significations may be concealed beneath isomorphic animal forms.

I have thus dwelt rather lengthily upon this point because I think it is a vital one in our subject, and the possession of clear ideas thereon will be found singularly conducive to our correct appreciation of this whole class of anomalous phenomena under discussion. But we will revert to the subject of Owen's hypothesis.

As to the chief point in this hypothesis, the continuation of the primary germ-mass as a leaven, from brood to brood, it requires but little thought to perceive that it is physically impossible. I would first allude to Owen's statement, quoted above, that a portion of the germ-mass is taken into the abdomen of the embryo Aphis, and as he thinks, assumes, without any change, the position of the ovarium. By this he refers, undoubtedly, to the vitellus-looking mass I have described in my observations, and according to which, also, it appeared to serve only as the nutritive material out of which the digestive organs and the germs are formed. Moreover, I feel quite sure that the germ-cells are new cells formed in the abdomen, and not those derived from the parent.

But the point I wish to enforce, is, that even admitting that individuals B may contain an *actual residue* of individuals A, it is clearly evident that this succession must stop with brood B; for these residual germ-cells which compose B in its earliest condition are lost in the developmental processes, and the germs of individuals C, which are found in B, are, each, primarily, nucleated cells formed *de novo*, as I have observed and above described. With these observed conditions of development, it is impossible for the individuals of the successive broods to inherit the original spermatic force in the continuation of the original cells.

The hypothesis of Owen, therefore, plausible and ingenious as it may seem, does not appear to me to accord either with observed facts, or with the soundest physiology of the reproductive processes. I may here remark also, that his doctrine of Parthenogensis, based as it is upon the conditions of the hypothesis in question, cannot, as such, be sustained, for the same reasons, and all its phenomena would appear to find their solution either in Steenstrup's doctrine of "Alternation of Generations," so-called, or in the conditions of true gemmiparity—admitting, provisionally, that Steenstrup's doctrine, and gemmiparity, include really different physiological conditions.

But the most important explanation advanced, and the last which I shall notice, is that offered by Steenstrup* in his doctrine

* On the Alternation of Generations, or the Propagation and development of Animals through Alternate Generations, a peculiar form of fostering the young in the lower classes of Animals. Transl. by the Ray Society, London. 1845—passim.

of the "Alternation of Generations," and of which it forms a chief support. The details of this peculiar doctrine of Steenstrup I need not here furnish; they are well known to all physiological anatomists. Its features, however, may be expressed in a formula-like manner. Individuals A, produce true fecundated eggs, from which are hatched individuals B, which are unlike their parents in all zoological respects, but in which are developed spontaneously and without any reference to sex, germs which ultimately become individuals like A, and so the cycle of development is completed. These intermediate individuals, B, Steenstrup has termed nurses (*Amnen*), and he regards them as distinct animals subservient for a special end; he therefore considers that B constitutes a real generation.

Instances of such phenomena are found in the lower orders of the animal kingdom—Polyps, Acalephs and Worms; and late research has shown that they are more or less common throughout the whole of the Invertebrata.

The difference between alternation of generation and metamorphosis is too marked to require illustration; in the latter there is the same individual throughout, and the developmental processes, although concealed beneath different exteriors, are regular and normal; with the former, however, this chain of development is broken by one form being developed in another, this intermediate form serving as a stepping-stone for a higher and ulterior development. Another important point in this alternate reproduction, is, that in each new change some real progress is made—the nursing-form being manifestly inferior to the individual to which it gives rise.

Steenstrup regards the Aphides as furnishing the most perfect examples known of nursing individuals, and, on the whole, as constituting typical illustrations of this doctrine he has advanced.*

But if this doctrine implies conditions other than those which belong to true gemmiparity, it does not appear to me that it has any support in the phenomena in question of the Aphides. And although I am inclined to believe, as I shall soon show, that all these phenomena, essentially, may be of the same nature, yet there can be no doubt that the manifestations are here somewhat peculiar. With the Aphides there is no real morphological progress made in each brood, for the viviparous individuals are, zoologically, as perfect in every way, as those which are oviparous, except in their want of true sexual generative organs. I have shown that, in the one species here described, they had well developed wings like the true sexual individuals. Moreover, each brood, from the first to the last, inclusive, is merely a repetition of the same. But these conditions are external and economical, and, instead of offering these prominent examples as evidence against the validi-

* See, *Steenstrup*, loc. cit., p, 112.

ty of Steenstrup's doctrine, I would rather present them as broadly indicating that, after all, this doctrine in question involves no conditions excepting those belonging to a modified form of gemmiparity. All the instances of Polyps, Acalephs, Worms, Insects, &c., all would then be classed in the same category, and the variations in manifestation would belong rather to the economical relations of the animal, than to any intrinsic difference of physiological process. Thus the Distoma-nurses instead of being developed to a condition resembling at all their parent, remain persistent on a low form, and not only is their whole zoological character undeveloped, but they also experience morphological changes from the developmental process which immediately go on within them. All this is in perfect keeping with their economy, as animals, for the low order of their conditions of life does not necessitate a higher and more truly zoological form of these nurses from which are to be developed the true animals; were it otherwise, I cannot but believe that both the nurses and the grand-nurse of Distoma, would quite resemble the original animals. In the case of the Aphides, the economical conditions are different, and finely illustrate this point.

The Aphis-nurse, in virtue of its very typical structure as an insect, must live under higher conditions, and so its development, zoologically, proceeds to a corresponding point; this point is where it, as an insect and as an Aphis, can furnish the nutritive material for the development of its endogenous germs.

Herein, then, would appear to consist the prominent morphological differences observed in this category of phenomena, and I need not labor further to show that they are irrelevant of the primary essential conditions of these curious processes.

Such appears to me to be the highest, both physiological and zoological, interpretation that can be advanced for these phenomena which Steenstrup has so ingeniously collected and collated; and to advance the view that these intermediate individuals or nurses, are not intrinsically and zoologically the same as their parents, but furnish examples of how dissimilar animals may arise from a common stock—to put forth this view, I say, is to advocate a doctrine in physiology, as mischievous as it is deeply erroneous. I think, therefore, that the doctrine of Steenstrup may prove to be unfounded as far as it would involve, intrinsically, new phenomena in the processes of reproduction; and, as I have said on a preceding page, all its conditions may find their illustration and solution in the various phases of gemmiparity.*

* This statement is made perhaps more strongly and exclusively than the present state of our knowledge would warrant, but I throw it out much in a suggestive way. There is no subject in Physiology more interesting and comprehensive than that of *Gemmation*; the important question now is, does it, as an individual process, embrace all the categories of phenomena treated by *Lovén*, *Steenstrup*, &c., these phenomena varying extrinsically, according to economical conditions, or do they (the phenomena) imply something beyond and dissimilar from gemmation?

If in this discussion of some of the highest relations of physiology, we have not wandered too far from our subject proper which we have thereby sought to illustrate indirectly, we will revert to the thread of its discourse for a few concluding remarks.

The final question now is, what is the legitimate interpretation to be put upon the reproductive phenomena of the Aphides we have described? My answer to this has been anticipated in the foregoing remarks. I regard the whole as constituting only a rather anomalous form of gemmiparity. As already shown, the viviparous Aphides are sexless; they are not females, for they have no proper female organs, no ovaries and oviducts. These viviparous individuals, therefore, are simply gemmiparous, and the budding is here internal instead of external as in the Polyps and Acalephs; it, moreover, takes on some of the morphological peculiarities of oviparity, but all these dissimilar conditions are economical and extrinsic, and do not touch the intrinsic nature of the processes concerned therein.

Viewed in this way, the different broods of Aphides cannot be said to constitute as many true generations any more than the different branches of a tree can be said to constitute as many trees; on the other hand, the whole suite from the first to the last constitute but a single true generation. I would insist upon this point as illustrative of the distinction to be drawn between *sexual* and *gemmiparous* reproduction. Morphologically, they have, it is true, many points of close resemblance; but there is a grand physiological difference, the true perception of which is deeply connected with our highest appreciation of individual animal life.* A true generation must be regarded as resulting only from the conjugation of two opposite sexes—from a sexual process in which the potential representations of two individuals are united for the elimination of one germ. This germ-power may be extended by gemmation or by fission, but it can be formed only by the act of generation, and its play of extension and prolongation by *budding*, or by division, must always be within a certain cycle, and this cycle is recommenced by the new act of the conjugation again of the sexes.

In this way, the dignity of the ovum as the primordium of all true individuality is maintained; and the axiom of Harvey, *omne vivum ex ovo*, stands as golden in physiology. The buds may put on the dress and the forms of the ovum, but these resemblances are extrinsic and in fact only an inheritance from their great predecessor.

* In this view as well as in several others herein discussed, I am pleased to say that I have the support of so learned a physiologist as Dr. Carpenter. See his Review "On the development and Metamorphoses of Zoophytes" in the Brit. and Foreign Med. Chir. Rev., 1848, i. p. 183; and "On Reproduction and Repair" in Ibid. 1849, ii, p. 419.

These phenomena thus interpretated, furnish us an excellent key to many others which have long been regarded as anomalous, in the history of development.

I refer here to the so-called hibernating eggs (*Wintereier*) which are found in many Invertebrates. These I have not seen, but they have been carefully described by several very trustworthy observers. These so-called eggs consist of oval masses or cells invested with a capsule, but in which no germinative vesicle and dot have ever been seen. Structurally, therefore, they do not resemble eggs, and it is from their form and ulterior development only that they have received this name. Moreover, they sustain none of the usual relations of eggs to the sexual organs, and, as far as I am aware, no one has witnessed their development in the ovaries. These bodies have been observed in *Hydatina** and *Notommata*† among the Infusoria; in *Lacinularia*‡ among the Rotatoria; and in *Daphnia*§ among the Crustacea. In all these instances they hatch without the aid of the male, the existence of which sex was once doubted from its infrequent appearance.

Now I regard these hibernating eggs as merely egg-like *buds* exactly corresponding to the germs of the viviparous Aphides. In other words, there are in the animals I have just mentioned, certain individuals which reproduce by buds which are developed under rather anomalous conditions; and I will add in conclusion, that I suspect that this gemmiparous mode of reproduction will be found to be far from uncommon among most of the Invertebrata, when our researches into the history of their development shall have been more widely extended.‖

* *Ehrenberg*, "Die Infusionsthierchen," p. 413.

† *Dalrymple*, Philos. Trans. 1849. p. 340.

‡ *Huxley*, Quarterly Jour. Micr. Sc. 1852. i, p. 13.

§ *Müller*, Entomostraca, p. 84. Tab. XI, fig. 9–11, Tab. XII. fig. 5. Also, *Ramdohr*, Beiträge zur Naturgesch. einiger deutschen Monokulus-Arten, 1805. p. 28; *Strauss*, Mém. sur les Daphnia, in the Mem. du Mus. d'Hist. Nat., v, p. 413. Pl. XXIX; *Jurine*, Histoire des Monocles, 1820. p. 120. Pl. XI, fig. 1–4. Jurine calls these aggregated eggs "La maladie de la selle."

There is, moreover, reason to believe that these anomalous reproductive conditions occur in nearly all the Entomostraca:—see *Siebold* and *Stannius's* Comparative Anat. My transl. vol. i, my note under § 292, note 4.

§ Notice may here be given of some curious observations, which *Filipi* (Ann. Nat. Hist., ix, 1852. p. 461) has furnished on the development of the Pteromalidæ. A *Pteromalus* lives in the ova of *Rhynchites betuleti ;* in each of these ova, there is seen, soon after its deposit, a minute infusorial animal, with a tail by which it moves briskly about among the vitelline cells. It soon ceases to move, however, and in its interior appears a vesicle which increases and changes into a larva which is that of *Pteromalus ;* this larva becomes a pupa, and, after eight or ten days, changes to the perfect insect which escapes from the ovum.

If these observations are verified, we have here a case exactly like that of the Aphides, excepting that like the *Distoma*, the intermediate budding form is very low, and takes on none of the zoological peculiarities of the parent. But these statements need corroboration, for they do not agree with the history of other species of *Pteromalus* whose development is well known. See also, the wonderful gemmiparous phenomena related by Siebold of *Gyrodactylus; Siebold* and *Kolliker's* Zeitsch. f. wiss. Zool., i, 1849. p. 347.

P. S.—I regret that I should not have seen until now, when this paper is concluded, the important writings of Leydig on the subject under discussion. In his article "Einige Bemerkungen über die Entwickelung der Blattläuse" in *Siebold* and *Kolliker's* Zeitsch. f. wiss. Zool. ii, 1850. p. 62, he speaks of his former observations in the Isis, 1848, iii, p. 184. These I have not seen, neither also a work to which he refers, of J. Victor Carus, (Zu näheren Kenntniss des Generationswecshels, Leipzig, 1849.) Leydig in his criticism of Carus's views, expresses the opinion that the development of the viviparous Aphides is, histologically, like that of the Articulata in general. According to him, also, the germ-bodies undergo processes corresponding to those of impregnated eggs. These statements of Leydig, who is an excellent observer, have induced me recently to repeat my observations; but this afforded the same results as before, viz.: that the germ-bodies out of which are developed the viviparous Aphides, have no true histological identity with eggs.

Art. VIII.—*Mineralogical Contributions;* by James D. Dana.

1. *Brooke and Miller's Mineralogy.*

The reviewer of this work in volume xv, page 41, mentions but briefly its importance in a crystallographic point of view. It is in this department eminently an original work, the result of special researches on the crystallization of very many of the species, with the measurement and calculation of their angles. It must therefore be long a standard work. Mr. Brooke has been indefatigable in his crystallographic studies, and early became prominent in English mineralogy by his Introduction to Crystallography, published in 1823; Professor Miller is no less distinguished for the precision and accuracy of his labors in physical science. The number of angles of each species given is very large, and there is a mathematical exactness and system in the selection of those which are mentioned, according with the methods of calculation. The circles filled in with dots, accompanying the descriptions of the species, afford much assistance in apprehending the positions of planes. The eye when looking down upon a crystal from above, an axis being in the line of vision, sees the planes arranged symmetrically, around the axis: and on describing a circle about the axis as its centre, if dots corresponding each in position to the normal of a plane, be placed in the circle, an ideal view, or rather the mathematical essence of the crystal, will be presented. Such are the circles referred to. The great deficiency in the work is the want of proper figures to aid in applying these ideal representations so as to give an actual shape to the mind correspond-

ing with the habits of the species. Moreover, another method of tabulating the planes may accomplish the same result more simply, we think, and one in which the planes shall be indicated by their true expressions, instead of arbitrary letters. The method alluded to, has already been presented in this Journal, in the writer's paper on Sphene and Euclase. It contrasts with the method in Brooke and Miller's work mainly as Mercator's projection of a sphere contrasts with the spherical projection.

The method of calculation adopted, is based for the most part on spherical trigonometry. It is less general in its formulas and less elegant, we think, than the system from analytical geometry, but sometimes affords more concise equations. The hexagonal system is rather at variance with itself in the method used. The planes are referred to *three* axes,—the three lines that connect the centres of opposite faces in a rhombohedron. The plan is not objectionable as regards the rhombohedral section of the hexagonal system. But in the hexagonal section of this system, in which the symmetrical parts are by sixes instead of threes, the planes of a simple dihexagonal pyramid require two sets of symbols in place of one: that is, where two identical planes occur about the same angle of a hexagonal prism, as in beryl, these must have different mathematical expressions. This leads the mathematician to no error; but tends to perplex a simple subject for the student.

But these are minor points, and leave the work still, the most accurate, thorough, and original work on the crystallization of minerals that has been published.

2. *Von Kobell's "Mineral-Namen."**

Prof. von Kobell, introduces the subject of his work by many judicious and pithy remarks on the multitude of synonyms in mineralogy, and the varieties of structure, derivation and formation, exhibited among the names of species. He next classifies the names according to their derivation, and explains their etymology. He presents, *First*, a catalogue of those names that are of mythological origin. *Second*, leaving the gods and mythology, he passes to those derived from the names of cultivators of the science, collectors of specimens, patrons, statesmen, and what not, among whom, about two hundred and thirty are hereby commemorated,—not immortalized, for amid the fatality to which mineral species are subject and the custom of change in authors, very many of the names are already in the rubbish heaps of the science. *Third*, comes a list of the names derived from localities; *fourth*, those alluding to the structure of the species; *fifth*, those based

* Die Mineral-Namen und die mineralogische Nomenklatur, von Franz von Kobell. 182 pp. 8vo; München, 1853. Briefly mentioned in this Journal, page 304, vol. xvi.

on color; *sixth*, on other physical characters; *seventh*, on chemical composition and reactions; *eighth*, names derived from other peculiarities, uses, arbitrary or fanciful allusions; *ninth*, those of unknown or doubtful origin. After speaking of the importance of a universal nomenclature for different countries, Prof. von Kobell lays down several rules for nomenclature in mineralogy, requiring that names derived from names of persons or localities should be written according to their original orthography, and not altered for each different language; that the Greek language should be used, rarely the Latin, for the derivation of other names; that the earliest name of a species should be retained, only when correctly formed in accordance with these principles: and he gives a list of some names that have been more or less recently proposed as substitutes for earlier objectionable names, the general adoption of which he observes would tend to make mineralogical language the same the world over. The greater part of these names are already accepted in the science.

It may be doubted whether, by carrying out with full strictness, his laws, we may not in some cases, create more confusion than we avoid, especially in the case of species well known in the arts. For example in substituting, as is proposed, *Liparite* (Glocker) for *Fluor* or *Fluor spar* (Fluss or Flusspath, of the Germans), we are giving a new word to science, without special significance in itself, and making confusion between the terms of the arts and science. As mineralogy is but a semi-science, and its nomenclature but a convenient means of designation without a proper scientific basis, we should hesitate before adopting new names in cases like the above. Print it *Liparite* and still the mineral will be called *fluor*.

Blende or *Zincblende* is another case of this kind. We cannot consider Glocker's *Sphalerite* a needed substitute for the old name. *Hematite* is an unfortunate substitute for specular iron, as it is restricting to narrow limits an old name of wider signification; and in this country, it is the most common designation of the species limonite. It is however coming into common use in Europe and Great Britain. *Arsenite* for arsenous acid, and *Chromite* for chromic iron, are objectionable names, as the termination is a chemical one for a section of salts. *Galenite* is not an improvement on Galena, as the word is as appropriate without the "*ite.*"

Horn silver (Hornsilber of the German), although two words and obnoxious to the criticism of not being Greek, is significant and contains fewer syllables and letters than Kerargyrite,—or Cerargyrite as the word should be written with us. For *Silver glance*, early called *Argyrose* by Beudant, Haidinger's name *Argentite* is adopted by von Kobell. *Clay* is dignified with the name of *Argillite*. *Azurite* of Beudant (*Blue Malachite*, *Kup-*

ferlasur, of the Germans, *Chessylite* of Brooke and Miller) is written Lasurite, contrary to a canon laid down by the author, requiring a Greek etymology; while *mispickel* is thrown aside for *arsenopyrite* of Glocker, as it wants this honorable origin. —The mineral called *Fahlerz* by the Germans, has given great trouble to English mineralogists, partly on the ground that a name like Gray Copper, consisting of two words, is objectionable, and partly because of the desirableness of a name common to both countries: sometimes the German name has been used, although one of the least significant of names, meaning simply Gray-ore; and sometimes Fahl-ore is employed, as if preferable to the translated expression. Haidinger's name, *Tetrahedrite* is adopted for it by von Kobell to the rejection of Beudant's *Panabase*, which is long prior in date, but less appropriate and badly compounded.—The mineral named *Lölingite* by Haidinger, and so accepted by von Kobell, was named *Mohsine* by Chapman, in 1843, and *Leucopyrite* by Shepard in 1837, and this last name has, therefore, the best title.

The name given by the writer to the so-called *Common* or *Oblique mica*, on the ground that the old name was bad, is overlooked by von Kobell. The word *Muscovite* was intended as no indignity to the Czar or his subjects, and commemorates the old name Muscovy glass, as well as the mineralogical fact that Russia has long been famous for affording gigantic plates of this species. Von Kobell adopts the name *phengite* for the mineral, Breithaupt's *generic* name for the biaxial micas.—The name *Calamine* is adopted for *Electric Calamine*, and *Smithsonite* for Carbonate of Zinc, as was long ago proposed by Beudant. Brooke and Miller have unfortunately reversed these names.

Although some objections have been suggested, the names and principles of von Kobell will command special attention. A system of names once agreed upon, would in part stop off the crowd of synonyms that are constantly coming in upon the science:—only in part, however, as long as there exists more ambition to attach a name to a stone than carefully to determine and accurately describe a species. Add to Prof. von Kobell's principles, one more,—truth and not self as the end of every investigation,—and the remedy would be nearly complete.

3. *The "Krystallo-Chemische Mineralsystem of Gustav Rose."*

Prof. Rose has here presented a modification of the Berzelian system of classification of minerals, in which the modifying principle is derived principally from Crystallography. The first part of the work, after an introductory chapter, contains a view of the distribution of species; according to their composition, as mentioned in volume xv, of this journal, page 430.

The application of crystallography, which is made in the latter part of the volume, consists in arranging the species of the several subdivisions in columns, according to the system of crystallization to which the species belongs. The divisions are thus broken up into natural groups, to a considerable extent, and a very interesting exhibition of the relations of the species is afforded.

If any objection can be made to this arrangement, it is one that science is not at present able wholly to overcome. There are various indications among the results of recent researches, that *peroxyd* and *protoxyd* compounds cannot be necessarily separated, and also that *hydrous* and *anhydrous* species may belong to one and the same group,—whether we adopt the views of Scheerer or not. Even in the method of Prof. Rose some exceptions are allowed, such as the placing of spodumene near augite, (rightly as we think,) although it belongs, in fact, to a following subdivision if the constituents of the species be considered. The exceptions are prophetic of a higher and wider principle than has yet been clearly brought out.

This volume contains extended notes, and original views on many mineral species. Among them it is observed that Sillimanite and Kyanite are widely different in specific gravity, and are probably distinct species; that ryacolite is probably a bad species based on an analysis by the author (G. Rose) of a specimen of feldspar mixed with some Nepheline; that Lepolite of Orrijervi, Amphodelite of Lojo and Tunaberg, and Walmstedt's Scapolite of Tunaberg are nothing but anorthite, and Linseite, Rosite and Rosellan are altered anorthite; besides many other facts and opinions of interest. These various observations through the volume, the classification, and the chemical formulas of species, by one of the ablest mineralogists of the age, render the work of great value and authority in the science.

4. *Crystallization of Haydenite of Cleveland.*

Haydenite from Jones's Falls, Maryland, was described by Levy, as monoclinic in its crystallization, and he gave for the angle of the rhombic prism 98° 22′, and for the inclination of the basal plane on the sides, 96° 5′. On account of the close resemblance of the crystals in form, to those of Chabazite, the species has long been suspected to be nothing but that species; but the difference in form and the angles, as well as in the analyses, has seemed to favor its being distinct. In some recent measurements, the writer obtained the angle 97°–98°, sustaining the ap-

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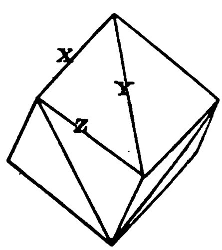

parent difference. The crystals are coated with Green earth, and only a part of the surfaces examined in this measurement were uncovered. On investigating further these and other crystals, the writer finds that the crystals are in fact scalenohedral, although nearly identical with the rhombohedron (R), the obtuse terminal edge (edge Y) differing but little from the inclined diagonal of R. Its faces are smooth and polished. While the positions of the planes show that the form is actually rhombohedral and not monoclinic, they also explain how different angles might be obtained in different directions, especially as the alternate pyramidal faces of the scalenohedron are often very unequal. This scalenohedron, since it is formed from a bevelment of the terminal edges of R, has the general sign $\frac{2}{3n-1}R^n$. The angle Y, (or that at the more obtuse or longer pyramidal edge,) by measurement is 175°–176°; X, (or that at the acuter or shorter edge,) is 97°–98°. These angles give approximately $n = \frac{10}{9}$, and lead to the sign $\frac{6}{7}R^{\frac{10}{9}}$; but calculation from this expression and the fundamental form of Chabazite makes the angle X = 100° 30′, which is too large; for Y it gives 176° 8′, and for Z 84° 46′. The measurements are however only approximative. The same scalenohedron, apparently, occurs in the Chabazite crystals of Nova Scotia, but the planes in those crystals are so made up of striæ that the angles are hardly measurable; such striæ have been considered an oscillation between the planes R and $-\frac{1}{2}R$. There can be little doubt that the species is crystallographically and physically chabazite. The analyses are adverse to this conclusion. But Prof. Silliman states that he has reason to doubt his results; and in those by Delesse, part of the iron was probably protoxyd, as obtained by Silliman. Small crystals of Heulandite (Beaumontite) cover the specimens, and are often implanted on the Chabazite. The Heulandite and green earth are probably of cotemporaneous formation, and subsequent to the Chabazite in origin. Some of the Chabazite crystals consists wholly of Green earth.

5. *Crystallization of Brucite* ($\dot{M}g\,\dot{H}$).

I am indebted to Mr. George J. Brush for a specimen of Brucite affording some minute crystals. They constituted a druse upon the foliated brucite in a narrow fissure in the serpentine. The crystals prove to be rhombohedral, as in the figure. The cleavage is basal, and the basal plane *O* is pearly. The other faces are brilliant vitreous in lustre. The crystals are so minute that the inclinations could be measured with the reflective goniometer only by means of reflected

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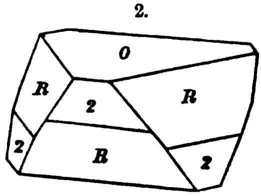

light. They thus afforded the angles $O:R=119°$ (to $119°\ 55'$) $O:2R=105°\ 30'$. These angles correspond to $R:R=82°\ 15'$, and axis $a=1{\cdot}527$.

6. *Crystallization of Hydromagnesite, and its Homœomorphism with Wollastonite.*

Messrs. Smith and Brush have recently shown that the distinctly crystallized Lancasterite is Hydromagnesite, while the pearly foliated mineral called by this name, is Brucite. The crystallizations from Texas, Pennsylvania, (Low's, and also Wood's mine,) are either massive fibrous, and radiated, looking much like Thomsonite, or flattened or acicular crystals, more or less grouped. At Hoboken the same mineral is found in very fine acicular crystals clustered together and having the aspect of natrolite. The lustre is vitreous. A distinct cleavage has not yet been observed. The Texas crystals occasionally have regular terminations, as may be seen with a magnifier of some power. They belong to the monoclinic system. With the reflective goniometer as above, I found for the angles $ii:2\grave{2}=112$—$113\tfrac{1}{2}°$; ii (back) : $-2\grave{2}=103°$; $2\grave{2}:-2\grave{2}$ (adjacent in same pyramid) $=143\tfrac{1}{2}$ $-145°$; opposite orthodiagonal edges of the pyramid (or y on y) over the summit $=94°$; same edge on orthodiagonal edge of prism (y on t) $133°$. The last two angles were measured with Smith's goniometer attached to a compound microscope.

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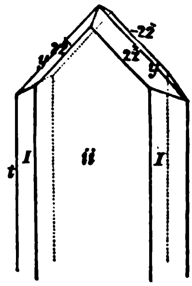

These measurements give for the inclination of the vertical axis, 82—83°; for the angle of the rhombic prism $i2$ (or $\infty P2$) corresponding to the octahedron $2\grave{2}$, $51°\ 26'$ to $51°\ 32'$; for the vertical axis, clinodiagonal and orthodiagonal of the octahedron $2a:2b:c=1:2{\cdot}2:1{\cdot}07$, nearly.

The prism I (or ∞P), hence would have the angle $87°\ 52'$–$88°$; and taking this as the fundamental prism, then the pyramid will be formed of the planes $2\grave{2}$ and $-2\grave{2}$, ($2P'2$ and $-2P'2$), as represented in the figure. We shall also have $2\grave{i}:2\grave{i}$ ($2P'\infty:2P'\infty$) $=94°$, $O:2\grave{i}$ ($0P:2P'\infty$) $=137°$, $ii:2\grave{i}$ ($\infty P'\infty:2P'\infty$) $=133°$.

The angle of the prism (I on I) is nearly the angle of Wollastonite, (this Jour. [2], xv, 449), and moreover $O:2\grave{i}$ in Wollastonite is $137°\ 48'$, or but $48'$ different from that of hydromagnesite.

Wollastonite and hydromagnesite are hence obviously homœomorphous. To this homœomorphous group belong, as elsewhere shown,

Borax, . .	$\dot{Na}\,\dddot{B}^2\,\dot{H}^{10}$	Pyroxene, ...	$\dot{R}^3\,\dddot{Si}^2$
Glauber salt, .	$\dot{Na}\,\dddot{S}\,\dot{H}^{10}$	Acmite, . . .	$\dot{Na}\,\dddot{Si}+\ddot{\overline{Fe}}\,\dddot{Si}^2$
Hornblende, .	$\dot{R}^4\,\dddot{Si}^3$	Spodumene,	$\dot{R}^3\,\dddot{Si}^2+4\ddot{\overline{R}}\,\dddot{Si}^2$, or $(\dot{R}^3,\ddot{\overline{R}})\,\dddot{Si}^2$

and probably Laumonite, $\dot{Ca}^3\,\dddot{Si}^2+4\ddot{\overline{Al}}\,\dddot{Si}^2+18\dot{H}$, which is, a Spodumene with $18\dot{H}$.

Chapman adds Crednerite; but we have seen no measurements of the angles of that species that confirm this reference.

Hydromagnesite is formed from the alteration of Brucite, and the change is one now in progress.

7. *Homœomorphism of Gypsum and Heulandite.*

The similar pearly cleavage of Gypsum and Heulandite, two species monoclinic in crystallization, suggests the idea of an approximate isomorphism: and this is sustained by a consideration of their crystals, although not obvious in the ordinary mode of viewing their forms.

Crystals of gypsum sometimes have a hexagonal outline with a bevelment of each of the six sides, by a pair of planes, either of which pair might be taken as planes of the fundamental prism. The interfacial angles of these pairs of planes or prisms are 138° 28′, 143° 42′ and 111° 42′ (B. and M.) The first of the prisms here mentioned is also a cleavage prism, cleavage taking place, though with some difficulty, parallel to its faces. There is some reason, therefore for considering this the fundamental prism, although usually taken as planes of the fundamental octahedron. There is a second imperfect cleavage parallel to a plane truncating the edge of the prism 111° 42′ (M: M of authors), a fact that evinces the prominent importance of the plane.

If we make the former cleavage directions, the lateral faces of the fundamental prism (I), the latter its basal plane (O), the planes hitherto observed (see Brooke and Miller) will be as shown in the annexed table, in which O is the base, and the columns contain the different vertical zones of planes.

O					
$1i(d)$	—	$1(l)$	—	—	—
—	—	$2(v)$	—	—	$2i(m)$
$3i(e)$	$33(w)$	$3(u)$	—	$3\grave{3}(y)$	—
—	—	—	—	—	$4i(h)$
					$6i(k)$
$ii(t)$		$I(n)$	$\grave{i}2(x)$	$\grave{i}3(s)$	$ii(b)$

The occurring octahedral planes are situated upon the *acute* basal edges and angles of the fundamental prism. The angle $I:I=138°$ $28'$, $2i:2i$ (clinodiagonal planes $2P'\infty:2P'\infty$) $=111°\ 42'$; and the faces meeting at 143° 42′, are planes of the fundamental octahedron (P or 1). The axes,

$$a:b:c=0{\cdot}9:1:2{\cdot}4135,$$

and the angle C (the inclination of the vertical axis) $=66°\ 14'$.

Now in Heulandite, $I:I=136°\ 4'$; $1:1=146°\ 56'$; and the axes,

$$a:b:c=1{\cdot}05:1:2{\cdot}475. \quad C=86°\ 26'.$$

The dimensions and the angles here mentioned (excepting C) are therefore very similar in these two species, and the crystals are much alike in habit. There is a wide difference between the two in the inclination of the vertical axis which makes large discrepances in some of the angles; for example $2i:2i$ in gyp-

sum is 111° 42′, in Heulandite, 98° 40′. When the axes are equal in two cases, the inclining of the vertical axis in one, diminishes the distance of its apex from the plane of the lateral axes, and consequently, the clinodiagonal prisms, and the octahedral forms on the obtuse basal edges and angles of the fundamental prism, become more and more flattened. This is the main source of the differences in angle between Gypsum and Heulandite. It shows us that in the oblique systems, we have to look to the relations of the axes, rather than to all the angles, in tracing out the resemblance of form among species.

Another inference from this case of isomorphism is that although some cases show that cleavage is a subordinate character, we may err in comparing crystals of different species, if we do not give due importance to the cleavage directions.

8. *Homœomorphism of Brookite and Columbite.*

Through the kindness of Mr. George J. Brush, I have recently received a specimen of Brookite from the Ellenville lead mine, Ulster Co., N. Y., occurring implanted on a group of quartz crystals. The mine is situated in the Shawangunk grit, a Silurian rock. The crystal has a striking resemblance in habit to that of the Columbite of Middletown, Ct., even to minor points in the arrangement of the planes. The form suggested at once the homœomorphism of the species, and this is closely sustained by the angles.

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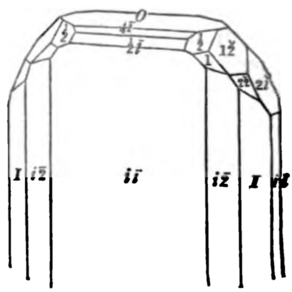


	Brookite.	Columbite.
Angle of prism $I(\infty P)$	99° 50′–100° 30′	100° 40′
$O : 2\breve{i}(0P : 2\breve{P}\infty)$	117° 54′	119° 40′
$a : b : c$	0·94438 : 1 : 0·84158	0·8675 : 1 : 0·8291.

The isomorphism is as close as between Celestine and Barytes.

It is of interest to compare the Columbite series (which includes Euxenite, Samarskite, Wolfram) with the Barytes series. The axes are as follows:

	Columbite.	Heavy Spar.
$a : b : c$	0·8778 : 1 : 0·8292	1·3127 : 1 : 0·81413.

Now it is apparent that *a* in Heavy Spar is $1\frac{1}{2}$ times *a* of Columbite. The relation is consequently a simple one—and so simple that the species may be considered as belonging to one and the same homœomorphous series. The plane $\bar{P}\infty$ of Heavy Spar and $\frac{3}{2}\bar{P}\infty$ of Columbite would be nearly identical in their inclinations on the base of the prism.

Such remarkable cases of homœomorphism almost show that similarity of form may exist irrespective of the elements concerned, or of any numerical ratio in the atoms of the elements

present. It is obvious that crystallization must follow, or go hand in hand with composition, but not lead the way in a classification of inorganic substances.

Hermann has endeavored to meet some of the difficulties produced by homœomorphism, by supposing two or more primary compounds as the bases of a class of species, the occurrence of which in different proportions shall form all the species of an homœomorphous group. This has an appearance of propriety as regards some groups of Silicates. But what symmetry in the constitution of the two can be made out in this way when Heavy Spar and Graphic Tellurium are compared; or Sulphur and Scorodite?—or Arragonite, Bournonite and Nitre?—or Chrysolite and Epsom salt?—or Pyroxene, Glauber salt and Hydromagnesite?—or Brookite and Columbite?—Such facts evince that homœomorphism is depedent on something beyond mere arrangement of similar atoms or of assumed parts of a species. The great principle of equality of atomic volume appears to be at the basis of it, and it would seem to matter not what the elements are; if only the resultant has a certain relation as regards atomic volume to the atomic volume of another compound, there is isomorphism.

Still there is often in related groups, a numerical relation in the elementary constitution which affords an explanation of the atomic volume relation, without looking to other considerations: and this numerical relation may be extended to the whole, when the subject is better understood. Such resemblances in homologous species, as between carbonates of protoxyds, etc., are of the most obvious kind, and have long been recognised.

The Gerhardtian view, that protoxyds and peroxyds may replace one another, taking three parts of protoxyds to one of peroxyd,—that is parts equal in oxygen—also explains in a numerical way many seeming anomalies, as in the case of the varieties of epidote, etc. Gerhardt applies it to the oxyds themselves, and seems to show that peroxyds and protoxyds crystallize alike; for in Martite, Specular iron ($\dddot{\bar{F}}e$) occurs in monometric forms, corresponding to the form of Periclase ($\dot{M}g$), and of Magnetite ($\dot{F}e\,\dddot{\bar{F}}e$); and Laurent has recognised that the monometric and rhombohedral metals (the latter isomorphous with $\dddot{\bar{A}}l$ and $\dddot{\bar{F}}e$) are two corresponding series, like the two forms of a dimorphous substance.

In this way, *Axinite* and *Danburite* have a like relation and both are triclinic. In axinite the ratio between the oxygen of the bases and boracic acid, and that of the silica is 1 : 1; and the same is true for Danburite as the recent analyses of Smith and Brush show.* The formulas may be written, for axinite $(\dot{R}^3, \dddot{\bar{R}}, \dddot{B},)\,\dddot{S}i$; for Danburite $(\dot{C}a^3, \dddot{B},)\,\dddot{S}i$. The propriety of reckoning the boracic acid with the bases is shown by the fact that in Tourmaline the ratio thus obtained is the only one that is constant for all

* This Journal, [2], xvi, 365.

varieties of the species, a fact admitted by Rammelsberg, but more particularly remarked upon in explanation of the isomorphism by Naumann. The formula of Tourmaline on this ground would be $(\dot{R}^{3}, \bar{R}, \bar{B})^{4}\, \dddot{Si}^{3}$.

But when we meet with such a case as that of the feldspars, where the only constant ratio is that of the oxygen of the protoxyds and peroxyds, the oxygen of the silica varying, the law for the replacement of protoxyds by peroxyds seems to have no application.

9. *Anhydrite.*

Hausmann, in his paper on the system of crystallization of Anhydrite, (Karstenite), and its homœomorphism with the Barytes series,* arrives at the following comparisons:—

		$a:b:c$	∞	$1\text{-}\bar{\infty}$	$1\text{-}\breve{\infty}$	
Thenardite	$\dot{Na}\,\dddot{S}$	0·7494 : 1 : 0·5918	118° 46′	106° 18′	76° 34′	(108° 26′)
Heavy Spar	$\dot{Ba}\,\dddot{S}$	0·7659 : 1 : 0·6234	116° 22′	105° 6′	78° 18′	(101° 42′)
Anhydrite	$\dot{Ca}\,\dddot{S}$	0·7636 : 1 : 0·6531	113° 42′	105° 16′	81° 6′	(98° 54′)

A closer approximation of Anhydrite is obtained by making the prism *m*, the vertical prism $\infty\text{-}\breve{\tfrac{3}{2}}$, and *s* the brachydome $\tfrac{4}{3}\text{-}\breve{\infty}$. Then the axes and the above angles become,

Anhydrite, 0·73486 : 1 : 0·59398 118° 35′ 107° 22′ 77° 4′ (102° 56′).

Giving the crystal the position usually adopted for heavy spar, in which the above ∞, $1\text{-}\bar{\infty}$, and $1\text{-}\breve{\infty}$ correspond respectively to $1\text{-}\bar{\infty}$, $1\text{-}\breve{\infty}$ and ∞, the axes are $a:b:c = 1{\cdot}368 : 1 : 0{\cdot}8083$, while those of Heavy Spar in the same position are 1·3127 : 1 : 0·81413. The prisms *m* and *s* in this view, are $\tfrac{3}{4}\text{-}\breve{\infty}$ and $\tfrac{4}{3}\text{-}\bar{\infty}$, and the octahedral planes are $m\text{-}\breve{n}$, $2m\text{-}2\breve{n}$, $3m\text{-}3\breve{n}$, with $m=\tfrac{3}{4}$ and $\breve{n}=\tfrac{4}{3}$.

The cleavage in this case is brachydiagonal and basal, with the macrodiagonal less perfect. The divergence in crystallization of the sulphate of lime from the others of the series, is a parallel fact with that of the bisilicate of lime ($\dot{Ca}^{3}\,\dddot{Si}^{2}$, Wollastonite) from pyroxene.

10. *Valentinite or White Antimony and Senarmontite.*

Senarmontite is described as crystalling in the monometric system and Valentinite in the trimetric, the chemical species SbO^3 being therefore dimorphous. But it is a fact worthy of remark, that the prism $1\breve{\imath}$ ($\breve{P}\,\infty$) of Valentinite is identical in angle with the angle of a regular octahedron, 109° 28′. The vertical axis and macrodiagonal in Valentinite have the ratio that subsists between the axis and intermediate diagonal of an octahedron, or $\surd 2 : 1$, and the vertical axis (a) equals nearly the sum of the two lateral ($b+c$).

* Poggendorff's Annalen, lxxxiii, 572.

ART. IX.—*Reviews and Records in Anatomy and Physiology;* by WALDO I. BURNETT.

1. *Traitè sur le venin de la Vipère*, par FONTANA. Florence, 1782. p. 229.
2. *Handbuch der Entwickelungsgeschichte des Menschen*, von G. VALENTIN. Berlin, 1835. p. 268.
3. *Mikroskopische Untersuchungen ueber die Uebereinstimmung in der Struktur und dem Wachsthum der Thiere und Pflanzen*, von THOS. SCHWANN. Berlin, 1839. p. 165.
4. *On the Minute Structure and Movements of Voluntary Muscle.* By WM. BOWMAN, in the Philos. Transact., London, 1840, pt. I, p. 457, —also, by the same, Articles, *Muscle, and Muscular Action*, in Todd's Cyclopædia of Anatomy and Physiology. 1842.
5. *On Fibre.* By MARTIN BARRY, M.D., &c., in the Philos. Transact. London, 1842, pt. I, p. 89,—also, by the same, *Neue Untersuchungen über die schraubenförmige Beschaffenheit der Elementarfasern der Muskeln, nebst Beobachtungen über die muskulöse Natur der Flimmerhäschen*, in Müller's Arch., 1850, p. 529;—and, *On Animal and Vegetable Fibre, as originally composed of twin Spiral filaments, in which every other structure has its origin;* in the Edinb. New Phil. Jour., Oct., 1853, p. 317.
6. *Observations on the Minute Structure and mode of Contraction of Voluntary Muscular Fibre;* by W. M. DOBIE, F.B.S.E., in the Annals and Mag. of Nat. Hist., iii, 1849, p. 109.
7. *Recherches sur la Formation des Muscles dans les Animaux Vertebrès, et sur la structure de la fibre musculaire en général dans les diverses classes d'animaux;* par M. le Docteur LEBERT, in the Ann. des Sci. Nat., xi, 1849, p. 349.
8. *Mikroskopische Anatomie, &c.;* von Dr. A. KÖLLIKER. Leipzig, 1850, Bd. II, erste Hälfte, p. 199.

AT this late period of histological research it may seem indeed a superfluous task to pass in review a subject apparently so well understood as that of the minute and ultimate structure of Muscular Tissue. But the truth is, that on this, as with most other subjects in microscopy, the advent of new observers, or the reappearance of old ones, in the field of research, bring with them the prestige of hitherto undiscovered facts and new truths, and so the old land-marks and positions laid down by earlier but by no means less able, faithful, and accurate observers, seem likely to be disturbed.

We do not propose here to enter upon any formal discussion of the historical relations of the doctrines advanced hitherto upon this subject. We desire to pass in review what we understand to be the leading features of the histology of this tissue, and

therein to seek the more or less definitely expressed *formula* of its structure—all of which will put us in a somewhat favorable position to regard critically some doctrines which are as remarkable as they are new.

But first of all we will briefly refer to some of the more prominent researches which, from time to time have served as true finger-posts as each succeeding investigator and explorer has passed along the road.

Although there can be but little doubt that some of those excellent old naturalists and observers of the last two hundred years, caught, with their rude magnifying powers, not erroneous glimpses of the complex intimate structure of muscle, yet any definite ideas of its real composition as explanatory of its mode of action, cannot be said to have been entertained until the days of the cell-doctrines of Schwann and Schleiden. It is true that since the time of Leeuwenhoek, or even, perhaps, before, it has been known that voluntary muscle could be split up into fine threads; but this was the limit of their real knowledge, for, if we except Fontana, none of the observers appear to have had distinct ideas of the nature of these threads.

Valentin, from studies upon the development of this tissue, had perceived clearly the general character of its composition, and Schwann, a few years after, applied more or less completely and successfully his cell-doctrine to its elementary formation and constitution. These undoubtedly were very important steps; but the contribution which marks an era in the histological history of this tissue is that of Mr. Bowman, which appeared in the Philosophical Transactions of 1840. Rare are the examples in the whole domain of minute anatomy, where so much real progress has been made by a single set of researches, as in this case. In more than one particular, Mr. Bowman exhausted the subject, and it is perfectly correct to say that in the leading and essential features of the minute anatomy of voluntary muscle, the numerous microscopical observers have added but little if any thing during the thirteen years that have since elapsed. Bowman's results are so well known and even familiar to all anatomists that it is almost out of taste to repeat them; but I will state them in a brief form: A series of discs succeeding each other in a row and at regular intervals; a row of discs thus formed constitutes the *primitive fibrilla.* Numbers of such fibrillæ are bound together with an exact coaptation of their discs and intervening spaces—constituting the striated muscular fibre. This fibre thus composed of a bundle of fibrillæ is encased in a special sheath, the *sarcolemma.* And, finally, a greater or less number of such encased fibres, bound together, constitute a *fasciculus*, and these fasciculi make up the gross muscle. We have then muscle: fasciculus, fibre, fibrilla, disc. Fibres may be split lengthwise, forming fibrillæ, and cross-

wise, forming large discs—the cleavage taking place, from the exact coaptation of the discs, through the light spaces.

Nothing can be more clear than the structure of a tissue thus wrought out, and with such data the student who has once caught its formula by an observation through the microscope on a good specimen, will never have his *idea* of striated muscle effaced,—for this in general is the elementary composition of voluntary muscular fibre wherever found, as a striated structure, among all animals.

Leaving for a future page a criticism of some of the points of Bowman's doctrine, we will continue our subject by some reference to the *development* of this tissue. Undoubtedly the most important because the most comprehensive researches that have been made in this direction are those of Kölliker and Lebert. The two leading features in the primary formation of muscle, are, first, that its origin is cellular, and second, that the fibre and not the fibrilla is the primary part evolved—the fibrilla being therefore a secondary or resultant formation.

The fibre is a more or less direct result of a fusion of a row of cells together:—this is the foundation; and the secondary changes which supervene thereon vary in extent and character according to the more or less complexity of the form of tissue ultimately evolved. The details of this genesis we need not here describe; all we wish to indicate is the original cell-constitution of muscular tissue in every locality where it is found; it might also be added that its departure from this original cell-condition and the metamorphosis of these cells into more or less complicated forms, holds a nearly corresponding ratio to the grade either of the animal in the scale of life, or of the function the particular tissue in question is to perform.

But these remarks, with the exception of the last general statement, refer particularly to the striated form of muscular fibre, to which the observations of Bowman and Lebert relate almost exclusively. But the other variety, the so-called smooth, non-striated fibres, is not the less interesting to the physiologist,—although they are, it is true, connected with the functions of the organic or non-voluntary life. Upon this subject, at least as connected with the higher animals, no single observer has thrown so much light as Kölliker. This excellent observer showed that the elements of the smooth or non-striated muscles, do not consist, as had hitherto been supposed, of long broad bands dotted with many nuclei, but are composed of comparatively short, isolated fibres, each of which contains a nucleus. The cell origin and constitution of these fibres (Faserzellen) are too apparent to be questioned. But these cells have experienced but few changes, and have undergone none of those elaborate alterations which supervene when similar cells form the striated variety of this

tissue. In one sense, then, the smooth, non-striated muscular fibres are only a kind of infra-formation of those which are striated—a condition of things in which the developmental changes persist on a low type. That this is a true version of these phenomena would seem to be indicated by the fact that in some instances these two forms of this tissue seem to run into each other, and in some insects the muscles of organic life of the alimentary canal, contain both striated and non-striated fibres.

From smooth, non-striated muscle we can proceed downward at least one step farther, where this tissue is cellular in its adult condition, a state of things where the developmental changes have been arrested, as it were, at the very outset. Muscular tissue of this simple kind is found in the Polypi and Acalephæ, especially, and contraction of muscles thus composed, occurs by a lateral widening of the cells, the whole muscle increasing in breadth proportionate to its diminution in length.

With these exordial remarks, in which we have desired rather to notice the general relations of the subject, than to be concise and explicit on special points—we will now turn and take up somewhat in detail some particular relations of this tissue, basing our remarks upon a set of recent investigations we have made under quite favorable circumstances. In this way we shall have occasion to refer to the other writings we have placed at the head of this article.

We will commence by an examination of the highest form or the striated muscle. Striated muscle does not occur lower in the animal scale than the Articulata;* at least, in the other divisions of the Invertebrata, viz., the Cephalopoda, the Cephalophora, the Acephala, the Annelides, the Turbellaria, the Helminthes, the Echinodermata, the Acalephæ,† and the Polypi, our own observations agree with those of others who have specially examined the subject, that we have never been able to perceive anything of the

* *Busk* (Transact. Microsc. Soc. London, ii.) has described and figured a striated form of muscular tissue as occurring in *Anguinaria spatulata* and *Notamia bursaria*. We have been unable, however, after considerable search upon many Bryozoa, among which were several species of *Alcyonella*, to detect anything like real striped tissue of this kind. But it should be mentioned that the descriptions of *Allman* (Rep. Brit. Assoc. for the Advancement of Sci., 1850, p. 318), who has recently studied this subject, would leave but little doubt as to the existence of the striated muscular tissue in the species of Bryozoa he examined, unless perchance this observer was deceived by a plication of the muscular tissue which occurs in these animals. See, in this connection, Comparative Anatomy, by Siebold and Stannius, Translat. &c., by Burnett, vol. i, § 29. note 1.

† In the Proceedings of the Edinburgh Physiological Society for July 23, 1853 (See Edinb. Month. Journal Med. Sc., Sept., 1853, p. 279), Dr. Cobbold is mentioned as having demonstrated striated muscular fibres in the umbrella of *Medusa aurita;* but this point needs further research, since Agassiz says, in speaking of the structure of the muscles of Medusæ; "With all the power of the best Oberhäuser Microscope, I have been unable to discover the slightest indication of striæ upon the muscular cells." See Contributions to the Natural History of the Acalephæ of North America, in the Memoirs Amer. Acad. Arts and Sc., 1850, N. S., iv, pt. II, p. 239.

kind. But in the three classes of the Articulata,—the Crustacea, the Arachnoidæ, and the Insecta, and in all the classes of the Vertebrata, it forms by far the larger portion of the muscular system. Wherever found, it is invariably the same as to the formula of its development and constitution, and whatever may be said as to the interpretation to be put upon this or that appearance, there can be no doubt but that it is always capable of being resolved into the same primary and secondary elementary forms.

The best specimens of muscular tissue, for the careful, detailed, and successful study of its elementary constitution are found in the Articulata, or, to speak more definitely from our own experience, in the Entomostraca of the Crustacea, or in the Neuroptera and Diptera of the Insecta. Our best and most satisfactory observations have been made upon the alary muscles of the common Musquito *(Culex pipiens)*, and which we could particularly recommend to those who would follow this line of inquiry.

A muscular fibre is, as we all know, a bundle of fibrillæ, but the question is, What is the histological nature of a fibrilla?

Dr. Martin Barry's view, that it has a spiral structure—being composed of two threads twisted together in opposite directions—is well known to histologists. This view, in common with the one of a like structure of the elementary parts of the organic world generally, was advanced in the first of his writings we have above named on our list. We know of no paper of its pretension which has met with such universal disfavor among investigators. But the misfortune of the case is, that this view should have again been produced in a resuscitated form, and pushed to its ultimatum even with new statements, as has recently been done in the last two writings we have cited of this observer. The paper published in Müller's Archiv appears under the prestige of being wrought out in many of its most important points with a most excellent *Pflossel* microscope, and as a whole prepared under the eye of the venerable Purkinje.

In truth, it would appear that Mr. Bowman's excellent researches to which we have already alluded, ought to have so far settled any disputation on these points, as to render any critical examination of these anomalous doctrines wholly uncalled for. But in our late review of this whole subject, we have not been unmindful of this theory, and particularly so since excellent opportunities have occurred showing what we believe to be the true version of not unfrequent, singular, spiral-like appearances observed in this tissue.

In the first place, we will say, that with the advantage of the use of some of Mr. Spencer's best and most powerful lenses, employed with the most favorable illumination, we have given several protracted sittings to the investigation of the nature of Dr. Barry's alleged phenomena, with his figures before us, and using

a great number of very excellently prepared specimens of this tissue taken from various animals.*

But with all these conditions we have failed to detect any appearance in muscular fibrilla indicating its spiral structure as advanced by Dr. Barry. This has been the result of all our examinations notwithstanding single fibrillæ in both a relaxed and a contracted state, have been subjected to an amplyfying power of more than 1000 diameters.

On the other hand we have not unfrequently observed appearances of the striæ of the fibrillæ, which, at first, and especially when a power of not more than 300 diameters is used, look remarkably like a spiral formation; but, by doubling the amplification the illusion is dispelled, and the curious appearance is shown to be referable to a peculiar arrangement or a kind of dislocation of the disc-like elements of fibrillæ. By a little rough treatment of separated fibrillæ in water, these appearances may often be produced to almost any extent. It is not uncommon to see the spiral appearance run one way half the fibrilla, and then change and run the opposite the remaining half. But the effect is most delusive when the disc-like elements of the fibrilla are not only turned awry laterally in a regular manner, but are also slightly tilted up. These same changes taking place in a bundle of fibrillæ or a fibre, and occurring with a nice coaptation throughout, give to the whole fibre a very spiral-like aspect.† Other appearances than these, indicating a complicated spiral-composition of this tissue, we have never observed, and from what we have observed both of the development and of the mature structure of muscular fibre in all its forms, we can give no credence to the doctrines of Dr. Barry, and we say this without the least distrust of our instruments, specimens, and opportunities.

The view of Kölliker upon the composition of the muscular fibrilla, is, in one sense, the very antithesis of the one we have just noticed, for he thinks that it has no structure at all, but is a homogeneous formation. He says: "The fibrillæ are composed of a substance which, although but little, is yet very perfectly elastic, and is therefore capable, from mechanical influence, of a very considerable elongation and subsequent shortening. In the extended condition, they are smooth and thin; but, when contracted, they are

* The specimens of muscular tissue we have used in these examinations, were prepared for us by Dr. Durkee, of Boston. Dr. D. has a happy ingenuity in such things, and the specimens in question, for beauty of exhibition of the ultimate structure, as well as for the completeness of preservation, exceed by far anything we have before seen.

† *Aubert* in his account of the structure of the thoracic muscles of insects has also alluded to this point, and one of his figures very well represents the spiral-like appearance of the fibrillæ seen under a low power. See Ueber die eigenthumliche Structur der Thoraxmuskeln der Insekten, in Siebold and Kölliker's Zeitsch. f. wissensch. Zool., iv, p. 388. Taf. xv, fig. II, *b*, and IV, *b*.

thicker, and have varicosities at regular intervals. . . . My opinion is, that the transverse striæ hold no essential, internal relation to the contraction of the fibres, but are simply an expression of this last and of the organic elasticity of the fibrillæ."—(Mikroskop. Anat., loc. cit. p. 263, 264.)

This view which makes the fibrilla a homogeneous formation, and according to which the striæ and disc-like structure, as usually seen, are only resultant or secondary conditions, demands a special examination, considering the high authority of its source.* In the first place, we have not been successful in verifying the chief datum on which this opinion is founded; we refer to the complete disappearance of the striæ and the consequent perfectly smooth aspect of the fibrilla. If a small bit of muscle be taken fresh, from the thorax of a Musquito for instance, and be treated in water at the same time being severely handled by pressure, extension, &c., and then put under the microscope, the field will be seen filled with fibrillæ which are pale, and at first, appear perfectly smooth. The striæ seem to have entirely disappeared, and cannot be seen, we are satisfied, with an amplification of even 500 diameters, but using double this power, or 1000 diameters, together with the advantage of favorable light, the striated appearance is distinctly observed, although irregular and disturbed from the mal-treatment received. We have prepared and repeatedly examined many specimens of this tissue thus harshly handled, and in every instance where the fibrillæ preserved their usual fibrilla-conformation, we have not failed clearly to perceive the ordinary although perhaps distorted striated appearance.

Judging from our own experience, therefore, we should be disinclined to accept the opinion that fibrillæ may be made to appear free from striæ,—on the other hand, we should attribute the alleged smoth aspect to the fact that probably sufficiently high and excellent microscopic powers were not used in the examinations.

After this discussion of different and dissimilar opinions, we may revert to our primary leading question: What is the histological composition of the muscular fibrilla? We shall be greatly aided in satisfactorily answering this question by referring to these formations as occurring under their simplest and most naked forms. If the muscles of some of the lower Crustacea, of *Argulus* for instance, or what is more convenient, of the thorax of the common house fly (*Musca harpyia*), be taken and examined under the microscope with the usual care, the fibrillæ will be seen very distinct, and separated from each other, at the same

* *Hassall* advocates the same opinion based upon the allegation that the striæ may be made entirely to disappear by the action of acetic acid, (see The Micros. Anat. of the Human body, &c., vol. i, p. 341); but this has not been the result of our own experience.

time very fragile, for, with light pressure, they immediately break up into discs or small regularly-shaped particles. This fracture is very uniform, and the resultant particles are of such a complete character in themselves, that they may be studied as distinct objects exactly as blood-discs or other organic particles. Observed with a power of 1000 or 1200 diameters these separated atoms present a regularity of form, shape, and size, fully indicating that they are special formations, in contradistinction to the view that they are the results of a systematic although accidental fracture of a homogeneous cylinder or band.* From these examinations and investigations, as well also as from others of a collateral character, we advance the opinion of the true histological existence of the discs or regular particles composing the fibrillæ, and that the transverse striæ are due to the presence of these particles and the appearances resulting from the action on them of the light.

While therefore we would support Bowman in his doctrine of the real existence of the sarcous elements, yet contrary to this observer, we shall equally believe in the existence *per se* of the fibrillæ, instead of regarding them as purely accidental cleavage formations. The phenomena observed in the muscles of many of the Articulata and especially those to which we have already referred, seem to admit of no other construction. Again, we have seen no evidence that, as Carpenter and Sharpey have advanced, the particles or small disc-like bodies composing this fibrilla, are each a "minute cell."† This is a point that has been particularly looked to, with the use of the high and excellent lenses we have the good fortune to possess. In every case these disc-like particles have showed nothing indicating a cell-constitution, but have appeared as homogeneous, elastic bodies, quite like many other organic particles we have not unfrequently met with in histological investigations.

Thus far the subject has appeared clear: but we wish now to notice some points and appearances observed, the signification of which seems a little obscure, or at least not satisfactorily determined.

If the best examples of muscular tissue in the form of fibrillæ are examined,—for instance, specimens from the thorax of the musquito,—there may often be observed, besides the spiral-like arrange-

* Leydig has described and figured this separation into discs of the muscular fibrillæ, as occurring in *Branchipus*. His observations in this respect upon these Entomostraca correspond closely with our own upon many of the Insecta. See, Ueber Artemia salina und Branchipus stagnalis, in Siebold and Kölliker's Zeitsch. f. wissensch. Zool., iii, p. 280, Taf. VIII, fig. 10, 11. This same observer also speaks of some muscles (those of the alimentary canal) of *Artemia*, being composed of spindle-shaped instead of disc-like elements, and so arranged, points and bases alternating, as to form a symmetrical fibrilla; see loc. cit. Taf. viii, fig. 6.

† See *Carpenter*, Princip. of Human Physiol., Amer. Ed., 1853, p. 305, and *Sharpey*, Quains Elements of Anat., 5th ed. ii, p. 168.

ment of the discs, anomalous appearances in the fibrilla which, although regular, are undoubtedly due to some abnormal changes occurring in the elastic substance of which the fibrilla or even its discs are composed. Several of these curious aspects have been described and figured by Mr. Dobie in his paper above cited. Thus, in some cases, instead of the appearance of a regular succession of square blocks which appear white, while the line of their apposition appears dark,—presenting, on the whole, a kind of tape-worm appearance,—there is observed another dark line bisecting the light space. There are then seen, broad dark line (the line intervening between the discs), light space, narrow dark line (line bisecting the disc itself), light space, and, broad dark line again. In other cases, the broad dark line is as wide as the disc itself, and then there appears a regular succession of light and dark spaces of equal width—the light spaces being bisected by a dark line, and the dark ones by a light line. But this beautiful series of light and shade may be completely reversed by a change of focus. In some rare instances, one of which we have now under observation, these light and dark spaces are quadrisected instead of bisected, there being two light or dark narrow lines instead of one across each space.

These singular appearances, of which there are numerous other varieties, might perhaps be accounted for on the supposition that, from rough handling, changes have occurred in the constitution of the component substance of the fibrillæ, which produce the refraction of the light here observed. But we will add that in all the examinations we have made of this interesting point, we have seen nothing in these varying aspects leading us to doubt the true disc-composition of the fibrillæ.

Another point to which we would here refer as at least of some suggestive interest, is the question whether the fibrillæ are invested with a sheath or not. Until quite recently we have observed nothing indicating that such sheath exists; but in some lately-prepared specimens, not only the disposition of the discs composing the fibrillæ, to each other, but also a kind of crenulated or denticulated aspect of the edges of the fibrillæ, sometimes observed;—such phenomena might be well explained on the supposition of the presence of a sheath, but, at the same time, they by no means demand this supposition. In some instances they have a striking appearance, but it is possible that they may be the remains of that gelatinous substance which binds the fibrillæ together in a fibre.*

* Dobie speaks of a homogeneous, delicate membrane, which is sometimes observed stretched between two fibrillæ when they are separated at a greater or less acute angle from each other, resembling the web between the toes of a palmipede bird. It may be the same substance that we have observed. See loc. cit., p. 114, Pl. VII, fig. 8, *a*, *b*.

As to the minute structure of the *non-striated muscular tissue*, we have but little to say. We have found it uniform and the same wherever occurring, even when it has the function of voluntary tissue, as is the case in the Mollusca and Radiata.

This last mentioned fact shows that the voluntary movements in animals are not necessarily performed through the agency of the striped fibre,—however much the constitution of this last would seem to indicate a greater nicety and coaptation of action, as we perceive is really the case. In no case have we observed it capable of being separated into fibrillæ, like the striated variety, as has been stated by Lebert and Robin to be the case in some of the Cephalophora*; on the other hand, in many species of this class, which have been examined we have found this tissue to be composed of fibres which admit of no further division except into their constituent cells.

Less studied, but at the same time more interesting, histologically, is that form of this tissue, which persists permanently on the cell-type, the cells still preserving their characteristics as such, instead of being lost in their contribution to a distinct form of tissue. This form of tissue composes the mass of muscle of the Radiata proper, and is perhaps best observed in the Acalephæ, and Polypi. Agassiz has given it a good description, as occurring in the former of these classes,† and in the latter we have frequently observed it, while making other investigations. A very fine example of this cell-muscle may be observed in the pedicle of the medusa-form of *Tubularia:* here, a double row of cells is observed;—these last, when in a state of relaxation are round or ovoid, but when contracted they wear a flattened, disc-like form, increasing the width of the pedicle in proportion to the contraction which has taken place. Nothing can be more beautiful to the physiologist than the observation of such phenomena, for nature is here observed with just that amount of drapery which would hide yet adorn her nakedness.

Before closing these remarks on the histological composition of muscle, we wish to refer to another point, in the form of an inquiry: Does not muscular tissue occur under even a lower form than that of being composed of cells? This query is advanced, because in some instances of the locomotive, contractile, if not muscular tissue of the lower animals, we have not succeeded in making out any cell-structure. This has been the case with the otherwise muscular, and highly contractile parts of some of the lower Helminthes, where the tissue in question seemed resolvable into granules alone, allocated under distinct forms by a delicate

* See, Kurze Notiz über allgemeine vergleichende Anatomie niederer Thiere, in Müller's Arch., 1846, p. 120.

† See Contributions to the Natural History of the Acalephæ of North America, in the Memoirs Amer. Acad. Arts and Sci., 1850, N. S., iv, P. II, p. 289.

fibrillated membrane. Again, in a long fusiform muscle of one of the tentacles of an *Alcyonella*, we have observed a like want of any cell-constituents—the muscle being apparently composed of a delicate granular, punctiform substance. On contraction, the muscle changed from fusiform to a fan-shape, its punctiform aspect becoming darker and more condensed. Further research upon these intricate points is greatly needed, but it should be remembered that there is, histologically, no reason why a granular stroma or substance should not possess contractility, as well as the elaborated formations from cells. It is true that its action may be more gross and less adaptive, but it may well possess an inherent elasticity which may be more or less under the control of the nervous system. As examples of this line of suggestion, may be cited the relation of cilia and other formations, which certainly have nothing muscular in their structure, although they possess a wonderful capability of motion, which is sometimes under the influence of the nervous system.

It is in this way that studies upon the ulterior constituents of organized forms, teach us that function in nature lies deeper than the material part in which it is manifested, or, in other words, that function cannot be regarded as the result of this or that material form, but rather that this last exists as such in virtue of the former being perfectly performed.*

In regard to the nature and character of muscular contraction—the concluding point in this discussion of our subject, it may be said that nothing is really known. We can observe the physical phenomena that attend the act of contraction and of relaxation in this tissue, but here is the limit of our successful inquiry, for the internal relation of this process is hidden from our perception, and is as much an enigma as the real essence, the remote cause, of everything else connected with the organic world about us.

The apparent phenomena in question are well-known and easily observed. A fibrilla contracts, and its increase in lateral bulk is proportionate exactly to the decrease in length that has taken place;—no space, therefore, has been gained. In the striated form of this tissue, this shortening is attended with an approximation of the constituent elements of the fibrilla—the discs; the striæ, therefore, appear in closer succession. In the two other forms of this tissue, the unstriated and the cellular, nothing is observed but a simple lateral increase of the fibres, which is precisely correlative with the amount of contraction. This much, therefore, may be said about the matter: Muscle, in virtue of its very organization as a peculiar tissue, possesses a particular organic

* See, for an inquiry into the nature of the contractile tissues of the lower animals, *Ecker*, Zur Lehre von Bau und Leben der contractilen Substanz der niedersten Thiere, in *Siebold* and *Kölliker's* Zeitsch. f. wissensch. Zool., i, p. 218. The views here advanced support those we have suggestively thrown out in the above remarks, as based upon the quite limited observations we have made on this subject.

property—*contractility*—a capacity by which its substance may be shortened or lengthened from the action of stimuli. The chief of these stimuli, is, of course, nervous force; but this last is very far from being the exclusive or necessary one, for, as we have often observed under the microscope, detached and wholly isolated fibrillæ may not unfrequently be seen to contract and relax from the stimulus of water and other liquids. We would, therefore, support the doctrine of Haller, in that muscle does not positively require for its action, the influence of a nervous system; but at the same, time we would not wish to be understood as advancing the doctrine of the true spontaneity of action with this tissue; on the other hand we would say that this action must always be the result of some stimulus, from within or without, which excites a manifestation of a peculiar organic capability.

Postscript.—Since the above was written and mostly printed, we have received the November No. of the Philosophical Magazine, which contains two recent communications by Dr. Martin Barry relating to our subject, "A Main cause of discordant Views on the Structure of the Muscular fibril," and "Further Remarks on the Muscularity of Cilia." These papers contain nothing but a reiteration of Dr. Barry's former doctrines under, what he would perhaps think, better auspices. But we have failed to perceive anything new, or indeed anything but a restatement of his previously advanced opinions; neither do we think that Dr. Barry has very satisfactorily stated the "main cause of discordant views" on the subject, although we do not think it doubtful wherein this cause really lies.

Record of Anatomy and Physiology, Dec. 1, 1853; by W. I. Burnett.

SPECIAL WORKS.

Friedreich, (*D. N.*) Beiträge zur Lehre von den Geschwülsten innerhalb der Schadelhoble. 8vo.

Klencke, (*Prof. H.*) Mikroskopische Bilder Naturansichten aus dem kleinsten Raume. In Briefen an Gebildete. Mit 430 Holzschn. 8vo.

Müller, (*J.*) Ueber den allgemeinen Plan in der Entwickelung der Echinodermen. Mit 8 Tafeln. 4to.

Bock, (*C. E.*) Lehrbuch d. pathol. Anatomie u. Diagnostik. Bd. II, Abth. 2. 8vo.

Heusinger, (*D. C. F.*) Recherches de Pathologie compareé. 2 vol. 4to.

Engel. Die Entwickelung rohrige u. blasiger Gebilde im thierischen Organismus. Mit 2 Tafeln. 8vo.

Canstatts, (*C.*) Specielle Pathologie u. Therapie vom klin. Standpunkte aus bearbeitet. 3te Aufl. Von Dr. E. Henoch. In 2 Bdn. od. 8-10 Lief. 1 Lief. 8vo.

Kehrer, (*Dr. F.*) Das Blut in seinem Krankhaften Verhältnissen: Ein Beitrag zur Pathogenie. 8vo.

Carus, (*C. G.*) Tabulæ Anatomiam Comparativam illustrantes quas exhibuit. Carolus Gustavus Carus junctus cum Eduardo D'Altone. Pars VIII. 4to.

The Transactions of the American Medical Association. Vol. vi, pp. 832.—Besides the details of the business of the Association, the reports of the various committees, and Essays on particular medical subjects, this volume contains two

prize Essays—one on The Surgical Treatment of certain fibrous Tumors of the Uterus; by Washington L. Atlee, M.D., of Philadelphia—the other on The Cell: its Physiology, Pathology, and Philosophy; as deduced from original investigations, to which is added its History and Criticism; by Waldo I. Burnett, M.D. of Boston.

As for the other or first mentioned of the above works, our space does not allow us to furnish, at this time, a special or even a general description of their contents.

II. PERIODICAL LITERATURE.

THE BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW. Oct., 1853.

The present No. of this well known and high-toned Journal is one of more than usual interest in a physiological point of view. It opens with an able and very agreeably written article by T. H. Huxley, on the Cell-Theory—also other able contributions on subjects correlative to practical medicine, and particularly the well-digested Annals of Micrology by Dr. Lyons—a labor of great value, since few, comparatively, can have access to the numerous foreign works and periodicals, from which Dr. Lyons so successfully culls. We hope to have the opportunity soon to discuss some points in Mr. Huxley's article in relation to cell-life and organization.

MÜLLER'S ARCHIV FÜR ANATOMIE, PHYSIOLOGIE UND WISSENSCHAFTLICHE MEDICIN. 1853. Heft. 3, 4. August, September.

Max Schultze. Ueber Chætonotus und Ichthydium (Ehrb.), und eine neue verwandte Gattung Turbanella.
A. Krohn. Ueber die Larve von Spatangus purpureus.
August Müller. Beobachtungen zur vergleichenden Anatomie der Wirbelsaüle.
A. Krohn. Ueber die Entwickelung der Seesterne und Holothurien.
Ed. Grube. Ueber den Bau von Peripatus Edwardsii.
A. Krohn. Ueber die Larve von Echinus brevispinosus.
H. Meyer. Das aufrechte Gehen. (Zweiter Beitrag zur Mechanik des Menschen Knochengerüstes).
Adolph Fick and *Paul du Bois-Reymond.* Ueber die unempfindliche Stelle der Netzhaut im menschlichen Auge.
L. Fick. Beitrag zur Temperaturtopographie des Organismus.
A. Krohn. Ueber die Brut des Cladonema radiatum und deren Entwickelung zum Stauridium.
W. Peters. Ueber des Kiemengerüst der Labyrinthfische.
W. Berlin. Ueber einem Wurm aus der Gruppe Anguillulæ, Enophies quadridentolus.

ANNALES DES SCIENCES NATURELLES, xix, 1853. Nos. 4, 5.

Duvernoy. Mémoire sur les Oryctéropes du Nil blanc ou d'Abyssinie, et du Sénégal, suivi de nouvelles recherches sur la composition microscopique de leurs Dents. p. 193.
Lacaze Duthiers. Recherches sur l'armure génital femelle des Insectes lépidoptères. (Suite.) p. 203.
—— De l'armure genitale des Aphaniptères. p. 213.
—— De l'armure génital des Insectes en général. p. 215.
Bischoff. Recherches sur la production de l'Urée. p. 238.
Camille Dareste. Analyse des Observations de M. Müller sur le développement des Echinodermes. p. 244, 257.
Claude Bernard. Recherches sur une nouvelle fonction du Fois, considéré comme organe producteur de matière sucrée chez l'homme et les animaux. p. 282.

COMPTES RENDUS, xxxvii, from August 8, to October 3, 1853.

A. Lavocat et *N. Joly.* Études anatomiques et tératologiques sur une mule fissipède aux pieds anterieurs. p. 337.
Philipeux et *Vulpiau.* Mémoire sur la structure de l'encéphale des Poissons cartilagineux et sur l'origine des nerfs craniens chez ces Poissons. p. 341.
Lacaze-Duthiers et *Riche.* Recherches sur l'alimentation des Insectes gallicoles. (Report on, by Quatrefages). p. 394.
A. Kölliker et *H. Müller.* Note sur la structure de la rétine humaine. p. 488.

Virchow. Déconverte d'une substance qui donne lieu aux mêmes réactions chimiques que la cellulose végétale, dans le corps humain. p. 492.
Gegenbaur. Recherches sur le mode de reproduction et sur le développement dans divers groupes de Zoophytes et de Mollusques. p. 493.

The Annals and Magazine of Natural History, vol. xii, October, November, 1853.

Thos. Williams. On the Mechanism of Aquatic Respiration and on the Structure of the Organs of Breathing in Invertebrate Animals. p. 243.
Clark. On the Branchial Currents in the Bivalves.
J. E. Gray. On the Teeth of the Pneumobranchiate Mollusca.
Thos. Williams. On the Mechanism of Aquatic Respiration, and on the Structure of the Organs of Breathing in Invertebrate Animals.

The Quarterly Journal of Microscopical Science, Nos. 4, 5. July, October, 1853.

[Transact. Microsc. Soc., London.]

Williamson. On the minute Structure of a species of Faujasina.
Gregory. Notes of a Diatomaceous Earth found in the Isle of Mull.
Wheatstone. On the Binocular Microscope and on Stereoscopic pictures of Microscopic Objects.
Wenham. On the application of Binocular Vision to the Microscope.
Shadbolt. A short description of some new forms of Diatomaceæ from Port Natal.
Legg. Observations on the Examination of Sponge Sand, with remarks on collecting, mounting and viewing Foraminifera as microscopic objects.
Rainey, (*Geo.*) A method of employing artificial light for the illumination of transparent objects, by which it is so deprived of glare and color as to be equal in its illuminating power to the best day-light.

[Original Communications.]

Brightwell. On the genus Triceratium, with descriptions and figures of the species.
Salter. On certain appearances occurring in dentine, dependent on its mode of calcification.
Lister. Observations on the Muscular Tissue of the Skin.
T. H. Huxley. On the Structure and relation of the Corpuscula tactus, and of the Pacinian bodies.
G. Rainey. Some observations on the illumination of transparent objects.
Herapath. Paper on the discovery of Quinine and Quinidine in the Urine of Patients under medical treatment with the Salts of these mixed alkaloids.
Riddell (of New Orleans, U. S.) On the Binocular Microscope.
Gregory. Additional observations on the Diatomaceous deposit of Mull.

Here follow short but interesting translations from the continental Journals, and Reviews.

Philosophical Transactions, 1853, Pts. 1, 2.

Hanfield Jones. Further Inquiries as to the Structure, Development, and Function of the Liver.
T. H. Huxley. On the Morphology of the Cephalous Mollusca, as illustrated by the Anatomy of certain Heteropoda and Pteropoda collected during the Voyage of H. M. S. "Rattlesnake," in 1846–50.
J. Tomes. Observations on the Structure and Development of Bone.
Newport. On the impregnation of the Ovum in the Amphibia, (Second Series, revised.) And on the direct Agency of the Spermatozoon.

Art. X.—*Biography of Berzelius;* by Prof. H. Rose, of Berlin.

(Concluded from vol. xvi, p. 313.)

The next subject to which Berzelius turned his attention belongs to organic chemistry. It was a comparative investigation of tartaric and racemic acids. He first corrected his former analysis of tartaric acid, in which he had given an atom more of hydrogen than Prout and Hermann, and adopted the results of these chemists. But he then found that the crystallized tartaric acid had precisely the same composition as the effloresced racemic acid, and that both acids had the same capacity of saturation,—facts which, especially at that time, were in the highest degree remarkable. This was one of the first clearly demonstrated examples that bodies of different characters may have the same composition. Berzelius had, sometime before, observed a somewhat similar fact in reference to the oxyds of tin, and Faraday, a short time afterwards, in reference to the compounds of carbon and hydrogen. Clarke had also discovered the remarkable modification of phosphoric acid, which he called pyrophosphoric acid. On this occasion Berzelius combined together, in an interesting manner, what was known of these bodies, to which he gave the name Isomeric. This term has been universally adopted, now that the number of such bodies has been so greatly increased.

From this time Berzelius frequently occupied himself with subjects which are certainly of the greatest interest to every thinking chemist, and indeed for every scientific man, since they are calculated to unfold to us somewhat more fully the nature of matter. He made known his views on this subject repeatedly, both in his "Jahresberichte," and in the several editions of his "Lehrbuch." Finally, he assumed two essentially distinct kinds of isomerism, and, in the strictest sense of the word, called those bodies only isomeric in which the elementary atoms may be regarded as grouped in different ways, forming compound bodies. These isomeric bodies may again be of two different kinds. They consist either of compounds which, with equal atomic weights, present different characters, or of compounds in which, though they possess different characters, the relative proportion of the constituents is the same, but in which the atomic weights are not equal, but twice, thrice, etc., as great as that of each other. Such bodies Berzelius termed, for the sake of antithesis, Polymeric compounds.

The other kind of isomerism Berzelius called Allotropism. It refers solely to elementary bodies, which, owing to causes not yet sufficiently understood, assume a different character from that which is usual to them, and, as it appears, retain this difference in

many combinations, where it may be the cause of differences in the character of these compounds. When isomeric conditions are observed in compound bodies, which consist of only two elements, combined in very simple proportions, this isomerism is, according to Berzelius, to be regarded less as owing to the different arrangement of the elementary atoms than to the allotropic condition of one or both of these elements; nevertheless, instances may occur in which both causes are simultaneously at work.

It is possible that Berzelius may sometimes have gone too far in his assumption of allotropic conditions, for there are some grounds for believing that an apparent allotropism may result merely from a different state of division. Thus, a few years before the discovery of the first example of isomerism, Magnus observed the interesting fact, that when the oxyds of iron, nickel, and cobalt, are reduced by means of hydrogen to the lowest possible temperature, the metals obtained ignite spontaneously, and oxydize when exposed to the atmosphere. This pyrophoric character evidently results from the finer subdivision of these metals, and it is destroyed when a higher temperature is employed in their reduction, which causes the particles to cohere together. The differences in platinum, according as it is reduced from its salts by the humid process, or obtained by igniting the ammonio-chlorid, and likewise the unequal combustibility of silicium, and its variable solubility in hydrofluoric acid, may probably be explained in the same way. Nevertheless, Berzelius was inclined to ascribe all these differences to allotropic conditions.

Shortly after the appearance of the paper in which Berzelius treated of bodies which, with the same composition, have dissimilar characters, Dumas went so far as to put forward the bold question, Whether many elementary bodies were not allotropic conditions of one substance, especially such as have the same, or very near the same, atomic weight, as nickel and cobalt, platinum and iridium, &c.? Berzelius favored this hypothesis, and regarded it as befitting, that new ideas should be followed up in all directions, even when it is not possible at the same time to adhere strictly to that which is, for the moment, to be regarded as probable; for truth sometimes appears to be inconsistent at the first glance, and in any case this was a way to arrive more rapidly at the results which might follow from a new idea. Certainly, upon the other hand, it cannot be denied that the question respecting a relation similar to isomerism between elements which have analogous but still distinctly different chemical characters, belongs to a domain, where perhaps our conjectures will never admit of being put to the proof.

The next paper by Berzelius was upon Vanadium. Sefström had found a new metal in the bar-iron of Taberg, which he called by this name. He had, however, restricted his investigation

to the preparation of the oxyd, or rather the acid of this metal, from the finer slags of the Taberg iron, and the determination of its distinguishing characters. He then transferred his stock of vanadic acid to Berzelius, in order that he might investigate the characters and history of the new metal. This investigation is a very extended one, and through it we have become acquainted with the new body in all its relations; whilst, as these are manifold and interesting, and as the acid has but little resemblance to other acids, it was difficult to assign to it its true position among them. In this respect the paper of Berzelius on vanadium may almost be compared with that upon selenium; for both have this peculiarity in common, that by them we have become so thoroughly acquainted with new and hitherto entirely unknown bodies, although in both instances but very minute quantities of rare material could be employed, that subsequent investigations have added but little to our knowledge, and nothing essential. Vanadium was afterwards found at several places, although always in very small quantities. Wöhler directed especial attention to the fact, that the acid of the new metal was contained in the lead ores of Zimapan, in Mexico, in which, as early as 1801, Del Rio discovered a new metal, and called it Erythronium; but misled by the authority of Collet-Descotils, who declared it to be chromium (with which Vanadium has certainly some similarity,) he afterwards admitted that his discovery was an error.

His next researches, which were upon Tellurium, were of a similar nature. Berzelius had already instituted experiments with very minute quantities of this metal, in so many respects interesting, but he was compelled to discontinue them for want of material. When Wöhler sent him a considerable quantity of this rare metal, which he had prepared from the telluric bismuth of Schemnitz, he again commenced the investigation. He first shewed how this metal can be prepared in its purest state. He then prepared all the compounds of tellurous acid (peroxyd,) as well as telluric acid, discovered by him, with bases, and indeed the different isomeric modifications which these acids form. These researches were likewise so complete, that they fully developed the history of this remarkable metal in all its relations.

The last great investigation by Berzelius, is that upon meteoric stones. He undertook this with the intention of studying these bodies, (as my brother and Nordenskjöld had already done,) as species of rocks, and, by this means, to determine what individual minerals they contained. The immediate inducement was a meteoric stone sent to him by Reichenbach, which had fallen a year previously in Moravia. But besides this, he examined three other earthy meteoric stones, and two masses of metallic iron. Berzelius inferred from his analysis that meteoric stones consist

entirely of such minerals as are found upon the earth, and that they certainly do not contain any elementary constituent which is not met with in terrestrial bodies. It was only in the meteoric stone of Alais that he found carbon in an unknown state of combination: this stone, when placed in water, disintegrated and fell to powder, which had a mixed smell of clay and hay. This shewed that if, as Berzelius considered, meteoric stones originated from other cosmical bodies, in their native state they could be converted into clayey mixtures, like the rocks on our own globe. He then raised the question as to whether this carbonaceous earth from the surface of another cosmical body contained organic remains, and consequently, whether there were upon its surface organised bodies, more or less resembling those on our earth? It is easy to conceive the interest with which he attempted to solve this question. This solution was not affirmative, but the results of his experiments did not justify a negative inference. Water and alkalies did not extract anything organic from the meteoric mass; on dry distillation, however, carbonic acid, water, and a blackish-grey sublimate were obtained, but no empyreumatic oil and no hydrocarbon; the carbonaceous matter was, therefore, not of the same nature as the humus on the earth's surface. The sublimate heated in oxygen, gave no carbonic acid or water; and changed into a white insoluble substance, whose nature could not be determined on account of the minute quantity. But to have pronounced it to be an elementary body, not originally belonging to our earth, would have been unwarranted.

This was the last extensive research made by Berzelius. His health, which, never strong, had already often necessitated the interruption of his labors, became, with increasing age, more delicate, and no longer admitted of his remaining continuously in the laboratory. He suffered, as is not unfrequent with intellectual men, especially from nervous headaches, which could not be mitigated by the most moderate living. He now begun to complain of a failing of the senses, especially his sight, and also of the weakness of his memory.

But his scientific activity did not on this account cease. He interested himself to the last for every branch of chemistry, and took the most active share in all the achievements of this science. Indeed, now that he was no longer occupied by important practical labors, he concentrated his activity more especially upon undertakings of a literary character, and with a zeal and industry which deserve the greater acknowledgment, since his bodily sufferings increased every year.

Among the products of the literary activity of Berzelius, I will here only make especial mention of the different editions of his "Lehrbuch der Chemie," and his "Jahresberichte ueber die Fortschritte der physikalischen Wissenschaften." His other works,

the lectures upon Animal Chemistry, and his work on the Blowpipe have already been spoken of.

The "Lehrbuch der Chemie" first appeared in Swedish. It was translated into German first by Blumhof, then by Blöde and Palmstedt, and the later editions were translated by Wöhler and Wiggers. It was also translated into other languages, but did not pass through so many editions in any, as in the German, for besides the translations of Blumhof and Blöde, five editions have appeared. The last but one, the fourth, consisted, on completion, of ten parts. The fifth and last was commenced by Berzelius in 1842, but was not completed, only five volumes having appeared, certainly very large, each one containing nearly sixty sheets. The inorganic chemistry alone was completed. Of the organic part contained in the last two volumes, the most important—the animal chemistry—is wanting.

In this work Berzelius has treated very fully of all the facts appertaining to the science, with remarkable clearness, perspicuity, and apt illustration. At the same time, every subject is criticised in such an impartial and just manner as can be displayed only by one who stands as high in science as he did. The arrangement which he selected is indeed not a strictly systematic one, which, in a science so imperfect as chemistry, can certainly only be called convenient. But especially in the inorganic part, there is still a certain well-founded succession, such that it is very easy to become familiar with the work. In the organic part the facts are not arranged according to a strict scientific principle, and a classification adapted for inorganic compounds could not possibly be carried out with organic bodies. For although Berzelius had always declared himself strongly in favor of the application to organic chemistry of what we know of the modes of combination of the elements in inorganic nature as the clue by which alone we could arrive at a knowledge of organic bodies, still he was compelled to admit, that we were far from having advanced so far as to be able to treat of all organic bodies as radicals, oxyds, chlorids, &c., as in inorganic chemistry. Most of the assumed organic radicals, often of a complicated nature, are of a hypothetical nature; they gain a character somewhat certain only when some compounds of the radical with other simple radicals can be produced, and the oxygen in them replaced by chlorine, sulphur, &c. In addititіon to this, chemists are of very different opinions as to how the composition of organic bodies is to be represented, even when they agree in a fundamental principle. Moreover, as is natural, the different arrangements vary, according as more new facts are discovered. For the present, therefore, it is at least more advantageous to treat of organic bodies in an elementary work in such a way as Berzelius has done, namely, in groups containing those bodies which have the greatest general

similarity in chemical characters. It has frequently been seen, that works in which a theoretical principle has been strictly followed throughout, do not so well fulfill their principal object.

In the organic part of this work, Berzelius has declared himself against the so-called substitution theory, and the law of types. He assumes, on the contray, that conjugate compounds exist in organic bodies, in which, for instance, acids are united with compound radicals, or with their oxyds, chlorids, &c., in such a way that the acid is not saturated, but is still capable of combining with bases without separation of the associated substance,—the conjunct,—which enters with the acid as a constitutent of the salt. When an acid has entered into such a conjugate combination, it has generally acquired such altered characters, that neither the acid nor its salts are similar to the free acid and its salts. When hydrogen is replaced in an organic substance by chlorine, or another halogen, this generally takes place in the conjunct and not in the acid, and the former does not on this account cease to play its former part, of modifying the character of the salts into which it enters, with its acids, more or less, and according as its composition is altered by substitution.

It has been asserted that the replacement of hydrogen by chlorine, in organic compounds, was not to be explained at all in accordance with the electro-chemical views of Berzelius, and that consequently these views were incorrect. But when such a substitution takes place, it is, as already mentioned, generally only in the compound radical,—that is, the conjunct, and a new radical is thus formed, in which chlorine may perhaps occupy the place of hydrogen, but cannot play the same part as it did. Substitution of elements may therefore be very satisfactorily explained, according to the principles of Berzelius; and if his theory be impartially compared with the others which have been put forward in such number in organic chemistry, the inference will be, that in the present state of the science it is in a position to explain the facts more satisfactorily than any other.

On looking carefully through the various editions of this work, it is impossible not to regard it with admiration. It is not only the clear and comprehensive description, which attracts,—the sound, impartial criticism, which compels men of opposite opinion to appreciate justly,—or the great minuteness which has not left unnoticed a single fact, however trifling, if it was of any influence—but it is also the enormous industry which must create astonishment. A scientific man who had done nothing more than publish this excellent work, in so many editions, each of which was so completely revised that but little of the previous edition was retained, could not be refused by us our grateful acknowledgments of his great industry: and yet this constitutes but a fraction of the achievements of Berzelius.

It is touching to call to mind the words with which he concluded the preface to the last German edition, which he could not quite complete; it is dated November, 1842. He says, "I cannot overlook that, even if the Almighty should grant me life and power to complete the edition of which the first part is now published, this will be the last. For this reason, I considered it necessary to revise it so thoroughly, that I could express the final views which have appeared to me as the most probable during the long space of time in which I was so fortunate as to be able to follow with uninterrupted attention the development of the science, from the first growth of the antiphlogistic chemistry up to the present time—fortunate if, among the many views which a future extended experience will alter or correct, at least some few may prove to have been rightly conceived. With the profoundest conviction of the uncertainty of our theoretical views as well as of their indispensability, I have endeavored, in presenting them to the reader, not to inspire him with any more firm conviction of their accuracy than they appear to me to merit, and I have therefore always directed his attention to the uncertainty in the selection of modes of explanation. It is a great obstacle to the progress of science to attempt to cause conviction of the truth of that which is uncertain. What is believed is not submitted to any further examination; and the history of science shews that a deeply-rooted belief in theoretical conceptions has often withstood the most palpable proofs of their inaccuracy. Many of the defenders of Phlogiston required a regular development of the doctrines of oxydation in order to be convinced of its truth, and many distinguished men died believing in Phlogiston."

An undertaking by no means less gigantic than his "Lehrbuch" was the publication of the "Jahresberichte," which appeared regularly from the year 1820 until the death of Berzelius. The last completed volume comprises the discoveries of the year 1846. Berzelius therefore published twenty-seven volumes.

After Berzelius had been elected, as successor of the botanist, Olaf Swartz, to the office of perpetual secretary of the Academy of Sciences, besides making other important changes which he considered necessary in the statutes of the Academy, he succeeded in carrying into effect the arrangement that annual reports on the progress made in the various physical sciences should be written by members of the Academy, especially the different curators of the Natural History collections of the Academy, and that these reports should be presented at the annual public meeting held upon the 31st of March, the anniversary, and extracts read from them, after which they should appear in print. Members of the Academy undertook to write such annual reports in the departments of Botany, Zoology, and Astronomy, Mathematics, and Technology. Berzelius himself undertook the reports on Physics,

Inorganic Chemistry, Mineralogy, Vegetable and Animal Chemistry, and Geology.

It was only a man like him, who as it were surveyed at one glance the whole range of chemistry, and himself worked so much in all its branches, that could have adequately executed such an undertaking. These reports will long remain an example of the way in which such productions ought to be carried out. They were very comprehensive in those departments with which Berzelius was most intimately acquainted,—inorganic chemistry, chemical mineralogy, and vegetable and animal chemistry; less so in the other parts, which contained only the most important discoveries in those sciences with which Berzelius had not especially occupied himself, or which he had not pursued during the latter half of his scientific career, such as physics and geology. The reports were generally objectively written. If the views of the author of the original paper corresponded more or less with those of Berzelius, he gave an abstract, proportionate in extent to the importance of the subject, but always most admirable. If, on the contrary, their views differed from his, he allowed himself to express his opinion upon them, and observed a noble and impartial criticism, which rarely became at all violent. In this respect, it is certainly to be regretted that precisely his last "Jahrbericht" closes with an energetic attack upon another celebrated chemist. But Berzelius never mixed up personalities with his judgments; and if sometimes one could not agree with them, still they were always of such a nature, that although they occasionally gave pain to those upon whom they were passed, they could never excite any bitterness.

For the science itself these reports were of the greatest value. Berzelius, on several occasions, drew from the investigations of others important conclusions, which had entirely escaped the notice of their authors; and as frequently did he direct attention to new experiments which should be made in order to strengthen the results already obtained, or upon which to found new arguments. In this manner he exercised a very beneficial influence. He was also led to make experiments himself by these reports; and he then gave their results, when they contradicted, improved, or extended those of others, in the reports.

These reports were especially long when it was necessary to refute opinions and views which Berzelius considered as detrimental to the progress of science. Thus, the reports of the discoveries of the year 1838 and 1839 contain very detailed arguments against the hypothesis that all organic acids are hydrogen acids, and against the substitution theory. These arguments have always a rare clearness and simplicity.

The objection has often been made to this report, that it was sometimes very complete, and in some instances too extended;

sometimes, on the contrary, especially in the physical part, scanty and imperfect. This is certainly true: but it was very natural that Berzelius should have a partiality for the treatment of those subjects in which he especially interested himself and of which he was most master; but as he was almost equally at home in all parts of chemistry, this objection cannot be made to the strictly chemical parts of the reports. With regard to the physical part of the reports, Berzelius had only undertaken it because no other member of the Academy would or could do so. It was only in the years 1838 and 1839 that the report was written by Von Wrede. As Berzelius had only occupied himself with those parts of physics which were intimately connected with chemistry, it is almost only these parts which are touched upon in his reports.

In the same way, there was no other reporter to be found for the geological part; but as Berzelius had never occupied himself specially with geology, and only in so far as it was connected with chemistry, he treated only of the chemical part of that science in his reports, and otherwise noticed only the geological researches referring to Sweden. In the latter volumes reports upon geology are altogether omitted.

I have thus attempted to furnish a sketch of the comprehensive scientific activity of Berzelius. It is probably seldom that science is so greatly advanced through the labors of one man, and there is scarcely any chemist who has furnished such admirable and sound contributions.

This representation of his scientific merits would, however, give only a feeble idea of the whole greatness of the man, were we to judge from it alone. It is rare that so perfect a correspondence of mind and character is found in any man. That which so irresistibly attached those who had the happiness to have any long intercourse with Berzelius, was not merely the lofty genius visible in all his researches; it was not merely the clearness, the astonishing copiousness of ideas, the untiring care, and the great industry: the general impression which he made was that of the highest perfection. It was—and every one who knew him intimately will agree with me—it was at the same time those characters which placed him so high as a man; it was the consideration for others, the noble friendship which he evinced towards all whom he considered worthy of it, the lofty disinterestedness, the extreme conscientiousness, the perfect and just recognition of the merits of others; in short, it was all those traits together which spring from a worthy and honorable character. These were the sentiments which inspired all those who for a longer or shorter time came into contact with him, and especially his pupils—of whom our Academy contains more than all the rest of Germany—with the most pious respect for his memory.

Berzelius travelled the path of Science together with other distinguished men, who likewise advanced chemistry with giant steps. This was a time such as no other science has yet known, for no other has grown up from its childhood to a certain maturity in so incredibly short a space of time.

Berzelius was born almost in the same year as H. Davy and Gay-Lussac. However similar were the labors of these three men in science, they were in other respects very different.

Davy's brilliant discoveries, especially that of the metallic nature of the alkalies, gave chemistry an extraordinary impulse, and caused great enthusiasm in its pursuit. He achieved great things by his discoveries, the further following out of which, however, he left to others. He died in the prime of life; but in a certain degree his intellectual blossom was already past. Born poor, he had attained to great honors and great riches, which were perhaps obstacles to his being subsequently as active for science as formerly. It is, moreover, in the highest degree to be regretted that, in the latter years of his life, his very extraordinary talents were entirely enstranged from that science for which he might have achieved so much.

Gay-Lussac commenced his scientific career with the discovery of an important law in physics, but he afterwards applied himself wholly to chemistry, and advanced it as much by accurate investigations as brilliant discoveries. To him is owing, among other important facts, the law, so important for the doctrine of definite proportions, that gases unite in simple relations of volume,—a discovery, however, of which he did not at first make the many applications that were possible. But the most brilliant researches of Gay-Lussac are indisputably,—besides those published in common with Thénard on physico-chemical subjects, —the two sets of researches upon cyanogen and iodine. Even independently of the extremely importance influence which these researches exercised upon the whole range of chemistry, they may be regarded as models of investigation, both as regards the total results, the strict consistency of the reasoning, and the admirable description. As often as they are read, even at the present day, they will be regarded with astonishment.

But when, soon after the appearance of his paper upon cyanogen, Gay-Lussac undertook, in conjunction with Arago, the editorship of the "Annales de Chimie et de Physique," his scientific activity became gradually less. The first volumes of this Journal certainly contain several small papers and remarks which call to mind the author of those on Iodine and Cyanogen; but after a few years he ceased to write almost altogether; and it is perhaps more to be sincerely regretted than in the case of Davy, that Gay-Lussac, who died but a short time since, and after Berzelius, should already in the vigor of life have renounced his active scientific career, which seemed to promise so much.

It was not so with Berzelius. He also, after years of poverty, gradually attained, if not to great wealth at least to external honors, without having sought them in the least. But these could not estrange him from science; on the contrary, he took advantage of every higher position for its benefit. Science was always the sole object of his endeavours, and he never employed them for a purpose foreign to it. So completely was his whole life dedicated to science, that, even under the sufferings resulting from a painful disease during his latter years, his whole thoughts remained bent upon it alone.

Such men present in their inspired labors, as it were, the type of the true man of science; and who does not feel himself happy to meet them in life?

Art. XI.—*Correspondence of M. Jerome Nicklès, dated Paris, October* 30, 1853.

Obituary.—François Arago.—Death has recently made grievous inroads into the ranks of French science. We have seen the fall, successively, of Laurent, Auguste de St. Hilaire, the Botanist, Adrien de Jussieu, the last male descendant of the brilliant dynasty of the Jussieus, who died in July last, President of the Academy of Sciences, and member of the Botanical Section. A loss, still more recent, has increased this list of the dead—a loss irreparable, for it is that of a man, who was at the same time an illustrious philosopher, a champion of popular progress, and a distinguished citizen.

François Arago was born on the 26th of February, 1786, at Estagel, a small village of 3000 inhabitants, situated near Perpignan (Eastern Pyrenees). His father was Treasurer of Perpignan. With a moderate patrimony, and a numerous family, he could not give his children a liberal education; but Madam Arago was able to supply it, and devoted herself to their instruction; and she afterwards had the richest recompense which a mother can look for: her sons were all men of distinction. Besides François, who immortalized himself by his discoveries, we see Jacques and Etienne, who are distinguished in literature; Jean and Joseph, who were brave officers in the Mexican service; and last, Victor Arago, the youngest, now commandant of the Artillery.

The appearance of François Arago on the arena of Science was most opportune. His father had destined him to the law; but the young man had other tastes. He met, one day, an officer of engineering drawing a plan on the ramparts of the city, and enquired of him how he could obtain the right of wearing so fine a uniform. "Become a scholar of the École Polytechnique," was the reply. From that time the career of the young man was determined. Having no instructors, he gave himself to books, and in 1803, at the age of 17 years, he entered this National School.

At the end of a year he had left behind him all his fellow students, and was detached by Monge to the Observatory of Paris, where he commenced his researches in Physics and Astronomy.

In 1806, he left for Spain, where he continued under the direction of M. Biot, the measure of the meridian of France, which had been interrupted by the death of Mecham. On the demand of the National Convention which established the Decimal system, Delambre and Mechain undertook the measurement of an arc of the meridian between Dunkerque and Barcelona. It was this measurement that MM. Biot and Arago continued to the Balearic Islands. This journey in Spain was full of dramatic incidents to Arago. Encamped on the summits of the elevated peaks of Catalonia, our observer had to contend in turn with the wind, the cold, and hunger, and also with brigands, the chief of whom ended by becoming the Protector of our young savants.

A year after their departure for Spain, MM. Biot and Arago had nearly completed the measurements as regards Spain. The former then returned to Paris, and Arago went on to Majorca to continue his operations. But the war was on the point of breaking out between France and Spain; and the night signals, the instruments, and the movements of the young Frenchmen who remained at work about the summit of Galatzo, rendered him an object of suspicion to the Majorcans, and Arago was arrested and thrown into the citadel of Belver. He managed to escape, and embarked with his papers and instruments for Algiers. The French Consul made him reëmbark for Marseilles, but at the moment of entering the Gulf of Lyons, the vessel was captured by a Spanish corsair and conducted to Rosas; Arago and his companions were at first imprisoned and then thrown intn the Pontoons of Palamos. At last, through the reclamation of the Dey of Algiers, to whom the vessel belonged, Arago and his associates were returned to Algiers. But a revolution had there taken place, and the Dey had just been decapitated; the new Dey was unwilling to let Arago go away, whom he supposed to be possessed of treasures, and under the direction of the Danish Consul, and Arago was consequently thrown into slavery. Finally, after a series of vicissitudes of various kinds, he succeeded again in quitting Algiers, and on the 2nd of July, 1809, he entered the lazaretto of Marseilles, with all his instruments which he had succeeded in preserving.

France had believed him dead. The first letter which arrived for him at the Lazaretto was one from Humboldt, who knew him only from his misfortunes, and from that time a friendship commenced between these two great men which continued to the end. On the 17th of the September following, Arago entered the Institute: he was then 23 years old. Already he had made with Biot an extensive work on the determination of the coëfficient for tables of Astronomical refraction; he had measured the refraction of different gases, a research that before had not been attempted; he had determined the relation between the weight of air and that of mercury, and found a specific value for the coëfficient in the formula for calculating the heights of mountains from barometric observations; he had also made an important investigation on the velocity of light, and numerous observations towards the verification of the laws of libration; and finally he completed the triangulation prolonging the meridian of France to the island of Formentosa.

In 1812, the Bureau des Longitudes charged him with the delivery of a course of lectures on Astronomy at the Observatory, which was

continued until 1847, presenting in them the most arduous details of the science. On the 7th of June, 1830, he was named perpetual Secretary of the Academy of Sciences, replacing Fourier. From this moment a new life actuated the Academy, and it was under the impulse received from Arago that this illustrious society attained in a great degree to that distinguished standing and authority now accorded to it by the scientific world.

The revolution of 1830 broke out, and Arago entered political life. Named a member of the Chamber of Deputies, he took his seat among the republicans; and being a great orator, he was not slow to acquire influence in the parliamentary debates. It was on his Report, that a national recompense was voted to Daguerre, the inventor of Photography, and to Vicat, the inventor of hydraulic cements. He voted the printing of the works of La Place and those of Fermat; he defended the railroads against the coalition of the "maitres de porte"; he protected electric telegraphs against the adverse intentions of the administration represented in the Chamber of Deputies by M. Pouillet, the physicist; in a word, in all circumstances, Arago was at the head of Progress.

The revolution of 1848 brought Arago into the Provisional Government. He had just completed his eulogy of Bailey, the Astronomer, the friend of Franklin, who took an active part in the revolution of 1789, of which he was a victim. Reasoning by analogy, Arago looked for a like fate. This fear was happily exaggerated. Times had changed as well as circumstances, and the only analogy between the two men, Bailey and Arago is in that both were astronomers and both perpetual Secretaries of the Academy of Sciences.

After the coup d'etat of December 2, 1851, Arago refused to take the oath of allegiance, which was required of him in his capacity as director of the Observatory, and thus made manifest once more that politics ought to be kept aloof from Science.

A life of so much labor had worn down his health. Although attacked with diabetes, he still contemplated putting the last touch to his unfinished works. Bright's malady set in and aggravated his situation, which was complicated with dropsy of the abdomen, attended with effusions, and swelling of the extremities. All announced his approaching end: yet his mind was not for a moment obscured. Shortly before his death, although blind, he superintended in some difficult researches; he asked M. Babinet to prepare for him a table of more accurately determined numbers for the lengths of undulations, that he might bring to completion a memoir on interferences; and he finished the editing of his Physical researches on the Planets, &c. &c. He died in the midst of these arduous occupations, on the 2nd of October, at the age of 67½ years, a few minutes after having shaken the hand of M. Biot.

We have mentioned some of the works which Arago accomplished in his younger days. These works were completely eclipsed by the discoveries to which his name has since become attached, which embrace the following principles:

1. The discovery of chromatic and rotatory polarization.
2. That of Electro-magnets.

3. That of the magnetism which is developed when bodies are revolved near a magnet.

Arago was an Encyclopedic genius. Science, Literature, Political and Social economy, his vast intelligence embraced all with equal ability. His powerful faculty of assimilation, popularization, and of application of principles, placed him everywhere in the first rank. Whether Orator or Professor, he shone with brilliancy both in political and scientific assemblages. He was distinguished for the perspicuity and elegance of his style, and occupies an eminent place among the prose writers of France.

In the midst of so much grandeur, Arago led a most modest life. He considered as lazy whoever did not work fourteen hours a day; and such days were for him days of repose. Although so absorbed with his occupations, he still found time to appear in the society of Paris as one of its most spirited conversationists.

While devoted to continued labor, he completely forgot his own interests, and had only what was barely necessary for the support of his family. He left two children, one Emanuel Arago, an eloquent orator of the bar of Paris and of Republican assemblies, the other Alfred Arago, a distinguished painter. If he has not bequeathed to them a fortune, he has left an immortal name: he has created by his genius a renown more illustrious than all the renown ever gained by arms—which for a long time enjoyed the privilege of giving fame, but now yields the right to the peaceful conquests of science.

Academy of Sciences.—For some weeks past, there has been little of interest brought before the Academy of Sciences. The visits of several foreign savants, MM. Richard Owen, Magnus, Rammelsberg, Kölliker, etc., have afforded some little variety. But the new scientific communications are few at the present time. I therefore leave this subject to my next letter, when I shall also be able to state who is Perpetual Secretary in place of Arago.

Views on the origin of terrestrial magnetism.—The earliest view of terrestrial magnetism supposed the existence of a magnet at the earth's centre. As this does not accord with the observations on declination, inclination and intensity, Tobias Meyer gave this fictitious magnet an eccentric position, placing it one-seventh part of the earth's radius from the centre. Hansteen imagined that there were two such magnets, different in position and intensity. Ampère set aside these unsatisfactory hypotheses by the view, derived from his discovery, that the earth itself is an electro-magnet, magnetised by an electric current, circulating about it from east to west, perpendicularly to the plane of the magnetic meridian; and that the same currents give direction to the magnetic meridian, and magnetise the ores of iron; the currents, being thermo-electric currents, excited by the action of the sun's heat successively on the different parts of the earth's surface as it revolves towards the east.

A long time before the discovery of electro-magnetism, Biot was occupied with this subject, and regarded the terrestrial magnetism as the principal resultant of all the magnetic particles disseminated in the earth. M. Gauss adopts this view, as an interpretation of the fact,

without explaining it. An observation which I made some years since along with one of my brothers* has directed my attention to this subject. It related to the fall of a cylindrical meteor whose position was sensibly in the plane of the magnetic meridian. Many luminous meteors have been observed in this same position or near it, if I may judge from some of those described in the catalogue of Borguslawsky.†

The special position of the meteor observed by my brother and myself was not fortuitous; it was determined by the magnetic action of the earth, an action which may be powerful in its influence on meteorites consisting essentially of the magnetic metals, iron and nickel. In our view, the terrestrial magnet, the earth, decomposed by influence the normal fluid of the meteoric mass, and so gave the meteor thus polarized the direction of a compass-needle.

In generalising from this fact, and recalling the experiment of Arago on the magnetism developed when a magnet acts upon a turning disc, we ask whether the magnetic polarity of our planet may not be due to a like cause. Considering it, as proved, that the sun is polarized magnetically like the earth,‡ the sun will then be the inductor magnet, the agent which decomposes the magnetic fluid of the terrestrial globe; it will be to the earth, what the earth was to the meteor. This explanation does not resolve the difficulty, as it does not say whence comes the magnetic polarity of the sun. It implies the intervention of a magnet whose intensity is superior to that of the sun, acting on this last by induction, and impressing a polarity which the sun transmits to other planets of the system. It is the hypothesis reversed of the central magnet, for it places in space the magnetic mass which some physicists have supposed to exist within the earth.

The real cause of the magnetic polarity of the planets, is in my view the same for all, and Arago's experiment conducts to it in a straight line. It results even from the condition of their existence. Each star turning around a central axis, and in determinate curves, is influenced by the mass of these stars and their velocity at the circumference; in a word, the agent decomposing into two fluids the normal magnetism of the earth and the other planets, is their rotation. A geometer examining this opinion, would find, we believe, that the declination, inclination and the perturbations of the magnetic needle, are explained on this hypothesis much better than on any other.

Since my researches on circular electro-magnets and in general on bodies in rotation, I have sought much for experimental demonstration of this theory, and have now the conviction that this is impossible, as it is not possible for us while upon the earth to remove ourselves from the action of its own magnetism. Whenever a development of magnetism under the influence of rotation is observed, it is common to attribute it to the inductive action of the earth, rendered so striking by the experiments of Arago and Mr. Barlow.

Alongside of the different sources of magnetism mentioned in Treatises on Physics,—friction, pressure, percussion, torsion,—we should add

* Poggendorff's Annalen, iv, 1.

† See Proceedings, Brit. Assoc., 1853, Sept. 7, Report of Col. Sabine.

‡ Sur la chute d'une bolide par M. N. Nicklès and J. Nicklès, Compt. Rend. de l'Acad., xix, 1035.

rotation, a mechanical action of equal title with the preceding, and whose effects, produced through a subdivision like that of magnetic polarity, are found grouped at the extremities of the axis in rotation; in the same manner as the poles develop at the extremities of a bar of iron when it is subjected to torsion.

Artificial magnets.—For some time, permanent magnets have been made from cast iron by the aid of an electric current. The only difficulty consists in tempering the metal. M. Florimond, Professor of Physics at Louvain, has recently given the results of some investigations on this subject to the Academy of Sciences at Brussels, detailing the effects from using magnets of this kind in the construction of magneto-electric machines, these magnets being much more economical on account of the difference in value of cast iron and steel. The following are some of his conclusions:

1. Gray metal gives more satisfactory results than white metal, which is moreover too brittle.
2. Magnets tempered at a low red heat lose all their magnetism in twenty-four hours.
3. They retain their magnetism perfectly when tempered at a bright red heat.

The following is the method of obtaining the maximum magnetic power. The bars are heated to a red heat in a blast furnace; they are taken out, and powdered over the two faces for $\frac{3}{4}$ths their length with the yellow prussiate of potash pulverized, and then they are plunged immediately into a large quantity of cold water, with violent agitation. When the bars are cooled, they are magnetized by means of a horse-shoe electro-magnet capable of lifting about 200 kilograms. The two poles of the magnet are applied at the place where the branches of the cast iron magnet become parallel; the poles are made to slide quite to the extremities of the branches, and then detached to repeat 3 or 4 times this same process of friction. After operating thus upon one of the faces, the other is subjected to the same treatment, taking care that the same poles are brought into contact with the same branches.

The poles of the bundle of cast iron magnets ought to be always kept in contact with an armature of wrought iron of a size proportional to that of the bundle. The bars of cast iron should be a little thicker than those of steel.

Ascensional force of Balloons in water.—On the 18th of last June, Doctor Gianetti of the mineral springs of Orezza, Corsica, made an experiment with a balloon as a piece of hydrostatic apparatus, before a scientific commission. The object of his balloon was to raise objects from the bottom in deep water; and the force of it is such that with a diameter of 4 metres he was enabled to raise at least 31,000 kilograms. His experiment was made with a balloon of 50 centimeters, which raised 150 kilograms. With hydrogen a much greater effect would be obtained. But Dr. Gianetti, having a practical end in view, uses carbonic acid, which he obtained by the decomposition of a carbonate by means of an acid, at *the bottom of the sea.* He is now proposing to adapt to it a clock movement, which by opening or closing the facet in the top, by which the balloon is filled or emptied, shall cause it to rise or sink at will. It is also proposed to use this invention in river navi-

gation, for the passage of sand banks; the apparatus for this purpose could be secured to the sides of the vessels, and would not add a thickness of more than 2 centimeters (8 tenths of an inch).

Manufacture of Sal-Ammoniac from the residues of gas works.—The Industrial Society of Mulhausen offers annually a number of prizes for inventions and improvements made during the year: and it also offers a prize to those who introduce a new branch of industry into the department of the Haut-Rhin. This last prize was taken by MM. Moerhlin and Stoll, who manufacture sal-ammoniac from the ammoniacal liquid of gas works. The main difficulty in the operation consists in separating the tar-like material which it contains. The following is the process adopted.

The ammoniacal liquid is mixed with slaked lime; then submitted to distillation in a boiler heated by steam; the parts volatilised pass into a worm, in which the larger part of the tar is deposited; the ammonia passes on into a Wolff's apparatus, where it leaves the foreign substances present, and finally is carried into cold water where it is condensed. In this state it is nearly free from its impurities; it is neutralized with chlorohydric acid and evaporated in a lead boiler. As it deposits it is withdrawn by means of a wooden rake; it is allowed to drain, and then introduced into a brick mould and subjected to strong pressure. Blocks of sal-ammoniac are thus obtained, which are dried in an oven heated by part of the heat furnished by the evaporating furnace.

Separation of bromine from iodine.—Balard's process, as carried on by M. H. de Luca, gives a method of recognizing traces of iodine and at the same time of separating it from bromine with which it is so often associated. It is based partly on the greater affinity of bromine for the metals and partly on the violet color which iodine communicates to sulphuret of carbon. An impure bromine is treated by potash (or carbonate of potash which may be more easily obtained pure and free from chlorine); it is evaporated and calcined, to transform the bromate into bromid; it is then neutralized by means of an acid; the liquid is put into a test tube, and a drop of sulphuret of carbon is introduced, after which a drop or two of bromine dissolved in distilled water is added; it is then agitated, and if there is iodine present, the sulphuret of cobalt is colored violet. It is colored yellow by bromine. It is important to avoid an excess of bromine, lest it form a bromid of iodine, which does not act. I have tried the process, and found it exact nearly to a tenth of a milligram of iodine.

Artificial Silicification of limestones.—It is some years since M. Kuhlmann of Lille proposed to preserve pieces of sculpture, etc., by impregnating them with a solution of silicate of potash. $SiO^3\,KO + CO^2\,CaO = SiO^3\,CaO + CO^2\,KO$. This process has been used on a grand scale in certain parts of the cathedral Notre Dame. The architect of the cathedral reports as follows: 1, that the infiltration of silica made "sur les terrasses et contre-fort du choeur," in October, 1852, have preserved the stone from the green moss that covers stones in moist places: 2, that the gutters and flagging of limestone subjected to this process present surfaces perfectly dry, covered with a silicious crust: 3, that upon the stones so prepared, dust and spider webs are

less common than upon the stone in the ordinary state. The report also states that tender stones have been rendered hard; they have lost part of their porosity, and after being washed, they dry more rapidly than stones not silicified. The process has succeeded completely on all calcareous blocks, whether isolated or forming part of the structure, new and old.

It is not yet known how this process will act on mortars; but if successful, the silicification of an entire monument may be accomplished, and its restoration when old. The whole exterior might be thus covered with a thick bed of artificial silicate of lime, and a whole edifice be protected by this means from all atmospheric causes of destruction.

Vitrification of Photographic pictures.—The author of this process, M. Plaut, first procured a photograph on glass covered with albumen, and subjected it gradually to a strong heat so as to redden the glass. The albumen was destroyed, and the photograph, if negative, became positive by reflection. The picture was made of pure silver which adheres quite strongly to the glass, so that it may be polished without alteration.

On exposing this glass to the action of hydrofluoric acid in vapor, an engraving of the design is obtained over parts not covered by the image formed of the silver. It may also be possible to strengthen the image by a galvanic deposit and make a kind of plate from which engravings could be taken.

If, in place of arresting the process at a red heat, it is continued until the glass enters into fusion, the image sinks into the interior of the glass without being altered, and covers itself with a vitreous varnish. It appears like a design of great delicacy, enclosed between two plates of glass; and if positive proofs are employed, the method may be used for making pictured glass which may without doubt be colored by the ordinary processes.

Photographic Portraits on linen cloth.—The Revue Encyclopedique of the Abbé Moigno, from which we have taken the preceding note, states that the problem of making photographs on linen has been resolved. The Abbé Moigno has assisted at the operations of M. Wulff, the inventor; he says nothing of the processes, and we only know that the photographs were taken on linen covered with collodion.

Pyrogallic acid in wood vinegar.—The value of pyrogallic acid in photography gives much interest to the fact brought out by M. Pettenkofer, that it is afforded by the condensing apparatus for purifying gas obtained from the distillation of wood, an invention in which M. Pettenkofer has taken the greater part. M. Pauli is now engaged in the study of this acid. It is without doubt, says Liebig, owing to the presence of pyrogallic acid that we must attribute the preference which dyers give to wood vinegar, an acid which has not been replaced by the ordinary acetate of iron.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the polarization of light by refraction through a metal.*—Biot found that two gold leaves are sufficient to polarize direct solar rays completely. Rollmann has examined the subject anew, and has employed the gold leaves both as a polarizing and as an analyzing arrangement. When the light is very intense, only a single leaf can be employed, as otherwise the field of view appears too dark. When used as an analyzer, a gold leaf shows very distinctly the colors of thin plates of gypsum, cooled glasses, &c., but these are naturally modified by the peculiar blue green color of the gold. If we allow plane polarized light to pass through an inclined gold leaf, and examine by a tourmaline in the light so transmitted a plate of calcspar cut perpendicular to the axis, we shall observe the phenomena of elliptic polarization, when the gold leaf and the analyzer are turned to an angle of 45° with the planes of polarization. The colored rings are narrower in the first and third quadrants than in the second and fourth, the cross is converted into two hyperbolas, whose branches do not meet. When in the above experiment, we leave every thing else unchanged, and examine the calcspar with the analyzer, by means of the light reflected from the gold leaf in place of that transmitted, we observe the complementary figure such as we obtain it when we employ the transmitted light, and the tourmaline is turned through 90°. The tourmaline must be green in order to transmit the light well. Brewster's discovery of the elliptic polarization by metallic reflection is thus extended and completed.—*Pogg. Ann.*, xc, 188.

2. *Additional experiments on the internal dispersion of light.*—In a lecture delivered before the Royal Institution in London, Prof. Stokes has communicated some new observations on internal dispersion, which are of much interest. In accordance with an observation of Faraday, Stokes has found that the blue flame of sulphur burning in oxygen is a source of rays which exhibit the phenomena extremely well. Letters written upon white paper with a solution of chinin, immediately become visible when illuminated with this light, particularly when it has passed through a blue glass, although they are invisible in gas light. The letters remain visible when observed through a glass containing a thin layer of a solution of chromate of potash, but they instantly vanish when this glass is interpolated between the flame and the paper, the solution being impervious to the rays which occasion the color. The author points out in the next place the advantages which prisms and lenses of rock-crystal possess over those of glass, in experiments of this kind, inasmuch as they readily transmit the invisible rays. By employing the light of the powerful galvanic battery of the royal institution, and lenses and prisms of quartz, the author obtained a spectrum six to eight times as long as the ordinary visible spectrum, and crossed from one end to the other with bright bands. The interposition of a plate of glass shortened the spectrum to a small fraction of its original length, the highly refrangible portion being entirely absorbed. The discharge

of a Leyden jar gave a spectrum which was at least as long, but which was not perfectly similar to the others, as it consisted only of insulated bright bands. Stokes remarks finally that in winter, even in bright sunshine, he could obtain no such extended spectrum; as the spring advanced, the light constantly improved; he could not, however, see so far into the spectrum as at the end of the last August. The Earth's atmosphere was evidently not transparent for the very highly refrangible rays of the sun's light.—*Pogg. Ann.*, lxxxix, 627.

3. *On Chemical affinity.*—Bunsen has published an elaborate investigation of the laws of chemical affinity as governing the combination of a body when brought at the same time into contact with two or more other bodies. The views of Berthollet upon this subject are familiar to chemists; Bunsen has however found that Berthollet's law is inaccurate. He substitutes for it as the result of his researches, a new law which may be expressed in the four following propositions.

(1.) When a body A is brought into contact with two or more bodies B, B′, &c., present in excess and under circumstances favorable to combination, the body A, selects only such quantities of the bodies B, B′, &c. as are to each other in a simple stöchiometrical proportion so that together with 1, 2, 3, 4, &c. atoms of the one compound 1, 2, 3, 4, &c. atoms of the other are formed.

(2.) When one equivalent of the compound A+B, is formed in this manner together with one equivalent of the compound A+B′, the quantity of the body B compared with that of the body B′ may be increased up to a certain limit without changing the ratio of the numbers of equivalents. If, however, this limit is passed the ratio of the equivalents suddenly springs from 1 : 1 to 1 : 2, 1 : 3, 2 : 3, &c. The quantity of the substance may now again be increased without changing the ratio of the equivalents until a second limit is attained, when the ratio passes into another, &c.

(3.) When a body A reduces a compound B+C present in excess so that C becomes free a compound of A and B being formed, then if C can exert a reducing return action upon A+B, the final result of the decomposition is such that the reduced position of B+C is in a simple equivalent ratio to the now reduced portion.

(4.) In these reductions also the quantity of one substance may be increased up to a certain limit without changing the atomic ratio. Above this limit, sudden changes of the ratio may occur but always according to small rational numbers.

These laws obviously hold good only when the combinations concerned take place simultaneously, since otherwise the relation of the masses combining is constantly varying. The author consequently studied them in gaseous mixtures, by igniting oxygen with variable quantities of combustible gases.—*Ann. der Chemie und Pharmacie*, lxxxv, 137.

4. *On the supposed new metal, Aridium.*—Some years since Ullgren published a paper upon a substance found by him in a Norwegian chromic iron ore, and which he considered as the oxyd of a new metal closely resembling iron in its chemical properties and relations. Bahr has carefully examined the mineral in question, and finds that the so-called oxyd of Aridium is merely oxyd of iron with a little phosphoric acid and oxyd of chromium.—*Journal für praktische Chemie*, lx, 27.

5. *Preparation of pure Caustic Potash.*—Wöhler has given a very simple and elegant method of preparing caustic potash in a state of chemical purity. One part of pure saltpetre in powder is to be mixed with from two to three parts of metallic copper cut into small pieces, and the whole heated to a moderate red heat for half an hour in an iron or better still in a copper crucible. After cooling, the mass is to be treated with water, and the resulting lye poured into a narrow cylinder which is then to be carefully closed. After the oxyd of copper has completely settled, the supernatant liquid may be drawn off with a syphon. It contains no traces of copper. The solution is best preserved free from carbonic acid by Mohr's method, namely, by closing the bottle with a cork through which passes air-tight a tube open at both ends, and filled with a coarse mixture of Glauber salt and caustic lime. Iron decomposes saltpetre as completely as copper, but it cannot be employed to prepare pure potash in consequence of its containing carbon, silicon, phosphorus, &c. When the above proportions of copper and saltpetre are used, a portion of the copper is obtained in the form of suboxyd. For a second operation we may take 1 part of nitre, 1 of this oxyd, and 1 of metallic copper. After complete washing, the oxyd of copper may be dissolved in sulphuric acid, and thus converted into blue vitriol.—*Annalen der Chemie und Pharmacie*, lxxxvii, 373.—[This process will be particularly convenient if, as appears probable, the resulting oxyd of copper is in a proper condition to be used in organic analysis. If not, it might be reduced at a low red heat by coal gas, and again employed to decompose nitre.—W. G.]

6. *Density of Selenium.*—Schaffgotch has determined the density of Selenium, and deduces from a great number of experiments the following conclusions:

(1.) Selenium has two different specific gravities, (at 16° R.) namely, 4·282, and 4·801. The smaller number belongs to an amorphous and glassy condition; the higher one to a granular crystalline state; the two states may be converted into each other at pleasure.

(2.) The blood-red flocky Selenium as precipitated in the cold, has the density of amorphous Selenium, whether its color and apparent volume have been changed by heat or not.

7. *New Alkaloids.*—How has studied the action of the iodids of methyl, ethyl and amyl upon morphine and codein. When finely pulverized, morphine is digested in a closed tube with an alcoholic solution of iodid of ethyl, a white crystalline substance separates, which after recrystallization, is obtained in fine white needles. These are the iodid of ethyl-morphin, the formula of which is $C_{34}H_{18}(C_4H_5)NO_6+HI$. The base was isolated by treating the iodid with oxyd of silver, and appeared as a very caustic liquid of a reddish brown color; which gave no crystals on evaporation, but only a semi-transparent dark colored mass which was deposited from a boiling solution in alcohol as a microscopic crystalline mass. This powder is readily dissolved in muriatic acid to a yellow solution, which gives heavy yellow precipitates with chlorid of platinum and bichromate of potash. The oxyd is readily soluble in water: its probable formula is $C_{34}H_{18}(C_4H_5)NO_6$. With iodid of methyl a similar base was produced, the iodid of which has the formula $C_{34}H_{18}(C_2H_3)NO_6+HI$; the author terms it me-

thyl-morphin. When morphin is heated with chlorid of amyl, fusel oil and chlorid of morphin-ammonium are produced. Codein digested with iodid of ethyl yields a highly crystalline colorless substance which is the iodid of ethyl-codein-ammonium, and which has the formula $C_{36}H_{20}(C_4H_5)NO_6+HI$. Iodid of methyl yields a similar compound with codein. Methyl-morphin is isomeric with codein, but differs from it in chemical and physical properties.—*Journal für prakt. Chemie*, lix, 489.

8. *Preparation of Valerianic Acid from Fusel Oil.*—GRÜNEBERG recommends the following proportions as the most advantageous. $2\frac{3}{4}$ lbs. of bichromate of potash are to be introduced into a retort, and $4\frac{1}{2}$ lbs. of hot water poured upon the salt. A cooled mixture of 1 lb. of fusel oil and 4 lbs. of sulphuric acid diluted with 2 lbs. of water is to be allowed to flow very slowly and in a thin stream into the liquid in the retort, and the whole is then to be distilled. The distillation goes on quietly, and 9 ounces of oily valerianic acid are obtained.—*Jonrnal für prakt. Chemie*, lx, 169.

9. *Constitution of Butter.*—HEINTZ has communicated an elaborate paper on the constitution of butter, the results of which are as follows:

(1.) The margaric acid prepared by Bromeis from butter is a mixture of stearic and palmitic acids.

(2.) The fixed fluid acid which is contained among the products of the saponification of butter consists chiefly of common oleic acid, and not as Bromeis believed, of a different acid. There is no butter-oleic acid. Butter therefore contains common olein.

(3.) Among the products of the saponification of butter there is found a fatty acid, the hydrate of which contains more than 38 equivalents of carbon to 4 equivalents of oxygen. This acid, butic acid, has very probably the formula $C_{40}H_{40}O_4$. It is with great difficulty soluble in cold alcohol, and corresponds to a fat contained in butter which may be called butin.

(4.) Stearic acid is also contained among the products of the saponification of butter, though not in predominating quantity. Butter therefore contains stearin.

(5.) The largest proportion of the solid fatty acids in butter consists of palmitic acid. The largest proportion of the solid fats consists therefore of palmitin.

(6.) Cocinic acid cannot be detected in butter.

(7.) The portion of the solid fatty acids most soluble in alcohol consists of myristic acid. The presence of myristin in butter is therefore to be inferred.

Heintz points out the remarkable fact that in all the acids contained in butter, the number of equivalents of carbon and of hydrogen is divisible by 4. The same law holds good with respect to cocoanut oil. Heintz considers it therefore probable that the cetic and cocinic acids which he detected in small quantity in spermaceti are mixtures, since the numbers of equivalents of carbon which they contain are not divisible by 4 like those of the other acids in spermaceti: he proposes to resume the subject, operating upon 10 lbs. of spermaceti.—*Pogg. Ann.*, xc, 137.

10. *Preparation of Ferrocyanhydric Acid.*—LIEBIG gives the following simple method of preparing this acid. When a cold saturated solution of ferrocyanate of potash is mixed with its own volume of fuming muriatic acid added in small portions at a time, a snow-white precipitate of pure ferrocyanhydric acid is thrown down. These are to be washed with muriatic acid, dried upon a brick, and dissolved in alcohol; from the alcoholic solution the acid may be obtained in beautiful crystals.—*Ann. der Chemie und Pharmacie*, lxxxvii, 127.

11. *Separation of Nickel from Cobalt.*—LIEBIG has found that when a current of chlorine is passed into a cold solution of the double cyanides of cobalt and potassium and of nickel and potassium, the liquid being kept alkaline by the addition of caustic soda or potash, the nickel is completely converted into sesquioxyd and precipitated, while the cobalt remains in solution as unaltered double cyanid. The sesquioxyd of nickel may be washed and ignited, and the nickel weighed in the form of protoxyd; it is perfectly free from cobalt. The solution after passing the chlorine must still be alkaline. The smallest trace of nickel gives an inky black color when dissolved in cyanid of potassium, and treated with chlorine. This method of separating cobalt and nickel has perhaps some advantages over Liebig's second method which, it will be remembered, consists in boiling the mixed double cyanids with oxyd of mercury, which precipitates the nickel but not the cobalt.

12. *On a general method of volumetric analysis.*—BUNSEN has given a very accurate and elegant method for the volumetric determination of a great number of substances. The principle of the method consists in bringing the substance to be determined into such a state of oxydation that when boiled with muriatic acid chlorine shall be evolved. The chlorine is to be conducted into a solution of iodid of potassium, and the evolved iodine volumetrically determined by means of a solution of sulphurous acid. In those cases in which a substance is susceptible of two degrees of oxydation, neither of which evolves chlorine with muriatic acid, the author boils with muriatic acid and a known weight of bichromate of potash; a portion of the chlorine goes to peroxydize the oxyd analyzed, while the other portion passes off and sets free an equivalent quantity of iodine. The author gives a number of examples of the application of the method, in which results of surprising accuracy were obtained: for the details of the method, which would carry us beyond our limits, we must refer to the original memoir, which can hardly fail to mark the commencement of a new era in analytical chemistry. In the original memoir the author points out the advantages of the method in the determination of chlorine, bromine, iodine, sulphurous acid, sulphydric acid, the chlorates, chlorites, and hypochlorites, chromates and iodates, the superoxyds of lead, manganese, nickel, cobalt, &c., and finally the oxyds of iron and of cerium, and arsenious acid. To show the accuracy of the method, we give the results of two analyses, the first of a magnetic iron from the Tyrol, the second of a mixture of white arsenic and gypsum. The ore contained:

	By calculation.	By analysis.
Fe_2O_3	68·97	68·96
FeO	31·03	31·04
	100·00	100·00

The mixture contained AsO_3 33·15, and CaO, SO_3 66·85; the analysis gave AsO_3 33·14, and CaO, SO_3 (by difference) 66·86.—*Ann. der Chemie und Pharmacie*, lxxxvi, 265.

13. *Volumetric determination of manganese.*—KRÜGER has studied in Bunsen's laboratory the application of the volumetric method to the determination of manganese more especially when combined with other oxyds. The method of determining the quantity of manganese in the common deutoxyd will serve as an example of the process employed. A few decigrammes of the oxyd are introduced into a small flask which is then filled two-thirds full of pure concentrated muriatic acid. The chlorine evolved on heating is conveyed by a glass tube attached to the flask by vulcanized rubber into a solution of iodid of potassium. An equivalent of chlorine sets free an equivalent of iodine which remains dissolved in the excess of iodid of potassium and colors the solution brown. After the evolution of chlorine has ceased, a measured portion of the normal solution of sulphurous acid is added till the brown color has vanished. The excess of sulphurous acid added is then determined by means of a titrated solution of iodine in iodid of potassium, a few drops of a clear solution of starch having been added. The quantity of this solution required to oxydize one measure of the normal solution of sulphurous acid having been determined, the quantity of manganese may be calculated. It is well known that the oxyds of manganese are all converted by ignition into Mn_3O_4. In the presence of strong bases, however, this is not always the case, and a special investigation of this point was necessary. Krüger obtained the following results. To determine manganese in the presence of iron, the iron is to be peroxydized and the two oxyds precipitated together by carbonate of soda: the precipitate is to be well washed, dried and ignited, and then the manganese determined volumetrically. It is present as Mn_3O_4. The determination of manganese in presence of alumina and glucina is precisely similar; in both cases the oxyd after ingnition is Mn_3O_4. When, however, the mixed carbonates of manganese and copper, lead, cadmium, bismuth, zinc, magnesia, baryton, stratia or lime are ignited, the manganese is always present in the form of sesquioxyd and this must be borne in mind in calculating the result of the volumetric determination. The author further remarks that in order to determine manganese when only iron or alumina are present it is sufficient to ignite the compound with free access of air and then submit a weighed portion of the ignited compound to the volumetric determination calculating the manganese as Mn_3O_4. When one or more of the other oxyds are present in quantity sufficient to form a manganite, (Mn_2O_3 as manganous acid,) the same process may be empolyed, but the manganese must be calculated as Mn_2O_3. When the quantity of the other base is unknown or insufficient to form a manganite, a weighed quantity of the compound may be dissolved in muriatic acid and half its weight of oxyd of zinc added. The mixed oxyds are then to be precipitated with carbonate of soda, and the precipitate washed, dried and weighed. The manganese in a weighed portion of this precipitate may then be determined volumetrically: it is present as Mn_2O_3. The results obtained by the method in question are remarkably accurate.—*Ann. der Chemie und Pharmacie*, lxxxvii, 257.

14. *Vessels for the preservation of fluohydric acid.*—Städeler has found that gutta percha and vulcanized India rubber resist the action of fluohydric acid almost completely. A solution of the acid which was so concentrated as to fume in the air, was found, after having been for some time preserved in a bottle made of gutta percha, perfectly colorless and clear. The gutta percha had undergone no change, but was somewhat brighter colored on the inside.—*Ann. der Chemie und Pharmacie*, lxxxvii, 137.—[It would doubtless be possible to cover the inside surface of a glass bottle with gutta percha by pouring in a solution of the gum resin in chloroform.—W. G.] W. G.

II. Mineralogy and Geology.

1. *Parophite.*—A rock allied to the Dysyntribite of Shepard, and Rensselaerite of Emmons, has been named Parophite by T. S. Hunt, (Logan's Rep. Geol. Survey of Canada, Quebec, 1852, p. 95.) The name alludes to its resemblance to Serpentine, notwithstanding its non-magnesian character. It occurs imperfectly schistose, as well as massive, and sometimes a perfect schist; rarely botryoidal, with an appearance of concentric structure. Texture granular to compact. G. 2·7–2·784. Color pale greenish, yellowish green, olive green, ash grey, reddish. Lustre waxy, shining; subtranslucent. Hardness not over 2·5–3; cuts with a knife like massive talc. Composition, according to Mr. Hunt—

	$\dddot{Si}$	$\bar{\bar{A}}l$	$\dot{F}e$	$\dot{C}a$	$\dot{M}g$	$\dot{K}$	$\dot{N}a$	$\dot{H}$	
1. Schistose,	48·50	27·50	5·67	1·80	2·24	5·30	1·91	7·00	G.=2·705
2. Schistose,	48·42	27·60	4·50	2·80	1·80	5·02	2·78	6·88=99·80	G.=2·714
3. Botryoidal,	49·13	27·80	5·90	3·80	1·40	*undet.*		6·30	G.=2·784
4. Schist,	48·10	28·70	4·80	2·10	1·41	4·49	1·58	8·40=99·53	

Crystalline limestones of the Vosges.—Delesse describes with much detail this rock, (Bull. Soc. Geol. France, ix, 120.) He mentions the following minerals as found in the limestone which occurs in gneiss: *Phlogopite*, *pyrosclerite*, *green and gray pyroxene*, *brown hornblende*, *graphite*, *spinel*, *chondrodite*, *magnetic pyrites*, *iron pyrites*, *orthoclase*, *a triclinic feldspar*, *sphene*, *quartz*, *tremolite*, *chlorite*, etc. There are frequent concentric nodules in the rock, which consist, beginning at the centre, of feldspar, pyrosclerite, mica. Amphibole, pyroxene, sphene are common in the feldspar or pyrosclerite of these nodules. Delesse shows that the pyrosclerite has resulted from the alteration of the feldspar nodule previously formed. This paper enters into important discussions respecting the origin of crystalline limestones and their minerals.

Pyromeride of the Vosges; M. Delesse, (Bull. Geol. Soc. France, [2], ix, 175.)—This Pyromeride is a kind of quartziferous porphyry. containing small globules through it. The variety from the Vosges closely resembles that from Corsica. Delesse finds that the globules consist mostly of silica, being a mixture of silica (over 60 per cent.) and feldspar. He obtained in an analysis :—

$\dddot{Si}$	$\bar{\bar{A}}l$	$\bar{\bar{F}}e$	$\dot{C}a$	$\dot{M}g$	$\dot{K}$, $\dot{N}a$ (by diff.)	$\dot{H}$
88·09	6·03	0·58	0·28	1·65	2·53	0·48=100

The origin of the globules is attributed to the tendency of feldspar to crystallize, and the indirect action of the excess of silica present. There is an analogy between them and the so-called perlites and retinites. Feldspar never takes the globular form, except in rocks rich in silica. The rock contains specular iron. Delesse concludes that the silica was introduced, subsequent to the first origin of the rock. It occurs either in veins or intimately mixed with the rock material. Other rocks may beome pyromerides through a penetration with silica.

Pitchstone from the trap of Isle Royal.—The following analysis is by Foster and Whitney (Rep. Lake Superior, Part ii.):

$\dddot{\text{Si}}$	$\ddot{\bar{\text{Al}}}$	$\ddot{\bar{\text{Fe}}}$	$\dot{\text{Ca}}$	$\dot{\text{Mg}}$	$\dot{\text{Na}}$, *trace of* $\dot{\text{K}}$	$\dot{\bar{\text{H}}}$
62·51	11·47	11·05	2·67	2·11	3·03 (*loss*)	7·14

B.B in the forceps, swells up, becomes nearly white, and then fuses without much difficulty to a grayish glass. Imperfectly attacked by hydrochloric acid when pulverised.

Oxyd of Zinc.—According to G. Rose, (Kryst. Chem. Min., 1852, p. 64,) artificial crystals of oxyd of zinc, a furnace product, sometimes present a six-sided pyramid terminating the hexagonal prism, the former replacing the basal edges of the latter: and the angle of the pyramid is 127° 40′, very near the corresponding angle in corundum, which is 128° 3′—thus showing an approximate isomorphism between a protoxyd and a sesquioxyd.

Crystallized Furnace Products.—F. Sandberger has announced the occurrence as furnace products, of graphite in 6-sided tables near Dillenburg; metallic copper in threads and rarely octahedral crystals, near Dillenburg; antimonial nickel in long hexagonal needles, at Ems; galena in cleavable cubes, at Holzappel and Ems; magnetic iron in octadra; $3Cu^2\,O+SbO^3$ in copper red or yellow hexagonal tables, at Dillenburg; $Ti\,Cy+3Ti^3N$ in Bodenstein.

Fibrous amianthoid substance, a furnace product from Westphalia.—Schnabel obtained for this substance,

$\dddot{\text{Si}}$ 98·13, $\ddot{\bar{\text{Al}}}$ 1·24, $\dot{\text{Ca}}$ 0·46, $\dot{\text{Mg}}$ and $\dot{\text{Fe}}$ *a trace*, =99·83

G.=2·59.—(Pogg. Ann., lxxxv, 462.)

Dolomite.—M. J. Durocher has obtained Dolomite artificially through the action of magnesia vapors. He put in a gun barrel some anhydrous chlorid of magnesium and a porous carbonate of lime, the latter being so placed that it could be reached only by vapors from the former. The gun barrel was closed and then kept at a low red heat for three hours. The limestone when taken out, was partly scoriaceous externally, and covered with a mixture of chlorid of calcium and chlorid of magnesium. Within, it was altered mostly to a dolomite, as ascertained by analysis.

Pseudomorphous Minerals :—

Pinite after Labradorite.—Resembles a yellowish gray talc-like mica. H.=2·5, G.=2·832. Analysis by A. Knop and W. Knop, (Pharm. Centr. 1852, 165; Lieb. u. Kopp's Jahresb. f. 1851, 822):

$\dddot{\text{Si}}$	$\ddot{\bar{\text{Al}}}$	$\dot{\text{Fe}}$	$\dot{\text{Ca}}$	$\dot{\text{Mg}}$	$\dot{\text{K}}$	$\dot{\text{Na}}$	Fl	$\dot{\bar{\text{H}}}$
55·18	27·51	4·08	0·29	1·22	3·36	4·49	0·07	3·74=99·94

Formula deduced, $\dot{\text{R}}^3\,\dddot{\text{Si}}^4+5\ddot{\bar{\text{Al}}}\,\dddot{\text{Si}}+3\dot{\bar{\text{H}}}$.

Karpholite after Wolfram.—R. Blum, (Pogg. Ann., lxxxiv, 154.)

On Serpentine after Hornblende, Augite, Diallage, Schillerspar; by G. Rose, (Pogg. Ann., lxxxii, 511.)

On White lead ore after Linarite; by W. Haidinger, (Jahresb. der k. k. oestr. geol. Reich., 1851, ii, 78, and Lieb. u. Kopp, Jahresb. f. 1851, p. 824.)

On the waters of the Great Salt Lake, Rocky Mountains; by Dr. L. D. Gale, (Stansbury's Expedition to the Great Salt Lake, Philadelphia, 1852).—Amount of solid contents, 22·422 per cent. Specific gravity, 1·170. Composition:

Chlorid of sodium,	20·196
Sulphate of soda,	1·834
Chlorid of magnesium,	0·252
Chlorid of calcium,	*trace.*

On the Waters of the Warm and Hot Springs of Salt Lake City; by Dr. L. D. Gale, (ibid.) The mineral water of the warm spring has a strong smell of sulphuretted hydrogen. Specific gravity 1·0112. Solid matter afforded on evaporation 1·08200 p. c. Analysis afforded,

Sulphuretted hydrogen uncombined,	0·037454
" " combined,	0·000728
Carbonate of lime precipitated by boiling,	0·075000
" magnesia " "	0·022770
Chlorid of calcium,	0·005700
Sulphate of soda,	0·064835
Chlorid of Sodium,	0·816600
	1·023087

The Hot Spring has the specific gravity 1·0130, and yielded 1·1454 per cent. solid contents. Composition in 100 parts:

Chlorid of sodium,	0·8052,	Chlorid of magnesium,	0·0288,
Chlorid of calcium,	0·1096,	Sulphate of lime,	0·0806,
Carbonate of lime,	0·0180,	Silica,	0·0180=1·0602

Analyses of several native Borates; by Prof. Bechi, (from a letter from Prof. Meneghini to J. D. Dana, dated Pisa, July 26, 1853.)—The borates examined by Prof. Bechi occurs as incrustations at the baths of the Lagoons of Tuscany.

(1.) *Lagonite.* Analysis:

$$\dddot{B}\ 47{\cdot}955 \quad \dddot{\overline{F}}e\ 36{\cdot}260 \quad \dot{H}\ 14{\cdot}016 \quad \dddot{S}i,\ \dot{M}g,\ \dot{C}a,\ \textit{and loss},\ 1{\cdot}769=100$$

Formula hence deduced, $\dddot{\overline{F}}e\,\dddot{B}^3+3\dot{H}$.

(2.) *Hayesine?* Analysis:

$$\dddot{B}\ 51{\cdot}135 \quad \dot{C}a\ 20{\cdot}850 \quad \dot{H}\ 26{\cdot}250 \quad \dddot{S}i,\ \dddot{\overline{A}}l,\ \dot{M}g\ 1{\cdot}750$$

leading to the formula $\dot{C}a\,\dddot{B}^2+4\dot{H}$.

(3.) *Borax?* Analysis:

$$\dddot{B}\ 43{\cdot}559 \quad \dot{N}a\ 19{\cdot}254 \quad \dot{H}\ 37{\cdot}187=100$$

whence the formula, $\dot{N}a\,\dddot{B}^2+6\dot{H}$.

(4.) *Larderellite,* (new species.)—White and very light, tasteless, appearing under the microscope to be made up of minute oblique rect-

angular tables; M : T=110°, according to a measurement by M. Amici. Analysis:

$\dot{\ddot{B}}$ 68·556 NH⁴O 12·734 $\dot{H}$ 18·325

The formula deduced is $NH^4O\ddot{B}^4+4\dot{H}$. It dissolves in hot water, and is transformed into a new crystallized salt, which is represented by the formula $NH^4O\ddot{B}^6+9\dot{H}$.

On Melan-Asphalt; by C. M. WETHERILL, (Trans. Amer. Phil. Soc., x, 353.)—This mineral is the same that has been pronounced bituminous coal by other investigators. It is from the Albert coal mine, New Brunswick. Some of the reasons for considering it coal are cited in this Journal, vol. xiii, 277. Dr. Wetherill states that E. Durand of Philadelphia, obtained for the solubility of the asphaltum of Cuba 34 parts in ether, and 60 parts in oil of turpentine, with 6 residue; and of the Hillsborough material, 4 parts in ether, and 30 in turpentine, with 36 of residue. Cannel coal gives no solution with turpentine.

Analyses, afforded—

	Carbon.	H	O, N
1. Asphaltum of Cuba,	82·670	9·141	8·189=100
1. Melan-asphalt,	86·123	9·871	4·906=100

Dr. Wetherill calculates the formula $C^{68}H^{42}ON$, from the latter analysis. The Hillsborough product is stated to be unlike coal in becoming electric by friction. [It may be questioned whether this substance can be considered a simple chemical compound. It is more probable from the trials with solvents and other tests that there is a very large excess of carbon, as impurity.]

2. *Thalia.*—The following letter from Dr. Genth to Dr. D. D. Owen, respecting his new earth Thalia, written, as he informs us, in June last, is from the Proceedings of the Academy of Natural Sciences of Philadelphia. Dr. Genth writes us that his investigations were independent of those of Professor J. Lawrence Smith.

"I have just completed the experiments with your thalia, and have come to the conclusion that it is nothing but magnesia. Magnesia shows sometimes such a strange behavior with reagents, that one is inclined to think it a new earth. I had the same case with my analyses of Kämmerite (Rhodophyllite.) It is possible that the relations which exist in the mineral had not been destroyed, and that you have a solution of the mineral,—for instance, a solution of aluminate of magnesia. I separated both with acetate of potash, and free acetic acid and carbonate of baryta. The only strange reaction was, that it fell down with $NH_4O, \bar{O}$ in presence of NH_4Cl, but I find it now in all the magnesian minerals which I examine in a similar manner.

From the oxalate of your thalia I prepared the pure earth. With cobalt solution before the blowpipe it gave a flesh colored mass. Dissolved readily in very dilute acid, and gave no precipitate with ammonia in presence of chlorid of ammonium, and all the reactions of magnesia. The sulphate gave with sulphate of ammonia the well known double salt in oblique rhombic prisms. The pure sulphate with seven equivalents of water crystallized right rhombic, and had the form, appearance, taste, and gave all the reactions of epsom salt. It gave me 50·8 per cent. of water, 35·5 per cent. of sulphuric acid, which also proves that I

had sulphate of magnesia. The analysis of the mineral is, according to Dr. J. L. Smith, and also the researches in my laboratory:

	$\dot{H}$	$\dddot{Si}$	$\overline{\dddot{Al}}$	$\overline{\dddot{Fe}}$	$\dot{Ca}$	$\dot{Mg}$	$\dot{K}$	
J. L. Smith,	20·66	45·66	4·87	2·09	3·07	22·10	0·15=98·45	
E. Reakirt,	19·96	44·07	4·72	1·70*	3·75	21·49	not det.	
							$\dot{K}$	$\dot{Na}$
P. Keyser,		44·66	7·79		26·60		0·12	0·16

According to these analyses the mineral is Saponite."

3. *A new Meteorite from Tennessee;* by Prof. J. LAWRENCE SMITH, (from a letter to J. D. Dana.)—The meteoric iron was found in East Tennessee a short while ago, and weighed originally over 60 lbs. It is a highly interesting one, and has furnished for the first time the solid protochlorid of iron, found in a fissure. It is also rich in the phosphuret of iron and nickel, and furnishes material for a full investigation of this latter mineral. The examination is nearly complete, and when finished, a full history of the meteorite will be given.

4. *On the Identity of Owenite and Thuringite;* by Prof. J. LAWRENCE SMITH, (from a letter to J. D. Dana.)—The perfect correspondence of the compositions of Owenite and Thuringite, except in the alumina, led me to an examination of both these minerals. The analysis of Owenite accords with Dr. Genth's; that of Thuringite shows, that in the former analysis of it, sixteen per ct. of alumina has been overlooked. Full details of these particulars will appear in the fourth part of the reëxamination of American minerals.

5. *On the probable depth of the Ocean of the European Chalk Deposits;* by Prof. H. D. ROGERS, (Proc. Bost. Soc. Nat. Hist., 1853, 297.)—Various geologists, and among them Prof. Ed. Forbes, in his excellent and learned Palæontology of the British Isles in Johnston's Physical Atlas, have suggested that the Ocean of the Chalk deposits of Europe was a deep one; and in evidence of this, Prof. Forbes cites the "striking relationship existing to deep-sea forms of the English Chalk Corals and Brachiopods, adding that the peculiar Echinoderms, (Holaster, Galerites, Ananchytes, Cidaris, Brissus, and Goniaster) favor this notion, as also the presence of numerous Foraminifera.

I beg leave to present a difficulty in the way of this conclusion. Several of these genera of Echinoderms, as *Ananchytes*, *Cidaris*, *&c.* occur in the Green Sand deposit of New Jersey, referable by every fossil test to the age of the Green Sand and Chalk of Europe. And this American stratum was unquestionably the sediment of quite shallow littoral waters. That they must have had a trivial depth is proved by the circumstance that they repose in almost horizontal stratification, at a level of not more than from one hundred to two hundred feet lower than the general surface of the hills and upland region to the N. W. of the margin of the zone they occupy as their outcrop. It is obvious that a depression of the cretaceous region, such as would cover the present deposits with a deep sea, would have likewise overspread the low Gneissic hills to the N. W. of the Delaware, which present no traces of having ever been submerged during the cretaceous or any secondary period.

* The sesquioxyd of iron and alumina contain a trace of silica, which was not separated.

Mr. Ayres remarked, that of those genera of Echinoderms, which Mr. Forbes regarded as deep sea genera, two or three are found in North America in water not two hundred feet deep. Terebratula, which has been generally regarded as only an inhabitant of very deep water, and whose structure has been described as admirably adapted to the depth at which it has been found, and which Prof. Owen has demonstrated cannot exist at a depth of less than two or three hundred fathoms, exists at Eastport, Me., in water so shallow that it can be taken by hand. In the same locality and position, Radiata are found which have heretofore been thought to be only inhabitants of deep water. Some of Mr. Forbes's genera are also found in less than ten fathoms of water.

III. Botany and Zoology.

1. *Salad for the Solitary; by an Epicure.* New York: Lamport, Blakeman & Law.—A book which has a very large sale, and is commended as a model of erudition. A chapter in it, which treats of curious matters concerning plants, having casually attracted our attention, we here refer to it, to illustrate, *negatively*, the proposition, that to moralize edifyingly upon any department of nature, or to draw felicitous illustrations, requires that an author should know somewhat of the subject matter he writes about. The following specimens are culled from a dozen pages of the book (from p. 185 to 197). The Papyrus of Egypt is said to be a tree! Of the tree which furnishes the Peruvian Bark, "its trunk, owing to the *frequent scaling of the bark*, is said to be seldom seen thicker than the arm, although it attains a great height." Camphor is said to be distilled "from the *roots* of a tree, growing in *Borneo and Sumatra*." "It is the leaves of plants and trees that act upon the air *like* human lungs, by absorbing carbon and evolving vital air for animal respiration,"—a curious and confusing way of expressing what is meant. The Cypress tree at Chapultepec, near the city of Mexico, our author affirms to be "one hundred and seventeen feet ten inches in girth." The two latest measurements that we have heard of made it 41 or 45 feet: but this was several years ago! On reading further, we perceive that the author has mixed up various famous Cypresses of Mexico into one *salad;* and has then applied De Candolle's (or as he writes De Candalle's) remark on the tree of Santa Maria del Tule to that of Chapultepec. Contrary to the best-grounded opinions, we are told that the Great Chestnut of Mount Etna was probably formed of a single trunk. Of "the colossal Water-lily of British Guiana," doubtless the Victoria, it is gravely said that "its flower measures from four to five feet in circumference." Our Magnolia grandiflora is said to be a "tropical plant;" with "leaves from eight to nine feet in length: its beautiful white blossoms are of like dimensions." Truly of such a tree he would have reason to say: "it is doubtless one of the most superb vegetable productions of which we have any knowledge." If we reduce these feet to as many inches, the statement would even then be somewhat exaggerated. The Tulip-tree, which surely the author might be personally familiar with, is said to have "*brilliant* glossy leaves, and blossoms, giving their odor to the

stars, and despising the minor denizens of the forest." To the stars, indeed, they must give their odor; for they have none for man. Discoursing of flowerless vegetation, our author states, that "even yeast .... is supposed by botanists to belong to this *genera* of the vegetable world;"—so that, after all, the botany is as good as the grammar. As a piece of vegetable morphology, we are told that "seeds are merely leaves preserved in peculiar cerements;" and in respect to these coverings a series of statements follows, the logical connexion of which may perhaps be divined. If successful, the reader may next attempt to extricate the author's meaning from the confused statements respecting the boundary between the vegetable and the animal kingdoms. Lastly, the Venus' Fly-trap is called "a native of Canada, which, not unlike other natives of that soil, discovers singular irritability of temperament." Unfortunately for the hit, such as it is, the Fly-trap is exclusively found in North Carolina. Whatever the rest of the book may be, surely, as to this chapter, the intelligent reader will hardly be able to appropriate one of the mottoes of the volume, taken from old Quarles.

"The *herbal* savor gave his sense delight."

Let us open the book in another place. On p. 92, we read that "paper is produced from a beautiful fibrous plant, called Linum, or flax, the *leaf* of which is rotted, and passing through certain processes, becomes *cotton cloth*," &c. Truly, ignorance, however preposterous, is not necessarily a sin *per se*. Its heinousness depends very much on the use that is made of it. A. G.

2. *Lindley: The Vegetable Kingdom; or the Structure, Classification and Uses of Plants, illustrated upon the Natural System; with upwards of 500 illustrations.* Third edition: with corrections, and additional genera. London: Bradbury & Evans, 1853. Roy. 8vo.—By intercalation (the work being stereotyped) and otherwise, this invaluable book is extended to about a thousand pages; and the latest additions to the science down to the time of publication are incorporated. It is an indispensable work, especially to teachers and travelling botanists. No other single volume contains a tithe of the valuable information that is condensed into this. The author's untiring industry and vigilance are shown in the accumulation and careful elaboration of this vast amount of materials. His signal ability and acumen are equally displayed in the copious critical matter, often of great importance, and in the defence of his peculiar views of the classification and affinities of plants; many of which, nevertheless, have been, and may be, we think, successfully combated. A. G.

3. *De Candolle's Prodromus.*—Our Botanists may be glad to know that the printing of the fourteenth volume of this work (to contain the *Polygonaceæ*, *Thymelaceæ*, *Proteaceæ*, &c.) has at length commenced. Probably the volume cannot be published before the early spring.

A. G.

4. *Observations on the habits of certain Crawfishes*, (in a letter of Dr. R. P. Stevens to the Smithsonian Institution.)—While examining a coal mine, along the banks of Coal Creek, a tributary of Green River, Bureau Co., Ill., I found innumerable little paths of an *Astacus*

leading from the water, along the sands and up into the neighboring low lands. Visiting these paths early in the morning, while yet the fogs were unrisen, I often found the *Astacus* returning from his meadow rambles, but never could ascertain the precise object of these rambles, whether predatory or otherwise.

A very interesting fact in relation to the habits of that animal I had an occasion to witness on the prairie near Chicago. On the ridge dividing the waters of the Chicago River and the Aux Plaines, is a wet marsh, lying near the deposit of bituminous limestone. The marsh, at the time of my crossing it, was quite dry. In search of fresh water shells, my attention was soon directed towards a large number of the paths of the *Astacus*, some of them showing evidences of a very recent traveller. I had not met with any water since crossing the Chicago River, two miles distant. Curiosity led me to follow up one of the freshest trails, until I discovered the retreat of the Crawfish. In my examination I crossed and recrossed the marsh in order to ascertain whether there was anywhere some little pool of water, but finding none I was soon delighted to find an *Astacus* leisurely travelling along his own highway, bearing in his mandibles a coleopterous insect. I followed on his trail to know what would become of him, when suddenly he was lost. By careful examination in the rank herbage, I found he had disappeared in a well or cistern about 10 or 12 inches deep and 1½ wide. Here was his own pool, provided for by himself, for the long summer draughts. In following up other trails I invariably found them terminating in similar pools. In many the inmate was present, whilst absent in others."

Extract from a second letter from Dr. Stevens, dated Nov. 23.—

"Our friends, the *Astaci*, increase in interest as I become more and more acquainted with their habits and instincts. I have learned this month that they are migratory, and in their travels are capable of doing much damage to dams and embankments. On the little Genesee, they have within a few years compelled the owners of a dam to rebuild it. The former dam was built after the manner of dykes, i. e. with upright posts, supporting sleepers laid inclining at an angle of 45° up the stream. On these were laid planks, and the planks covered with dirt. The *Astacus* proceeding up stream, would burrow under the planks where they rested on the bottom of the stream, removing bushels of dirt and gravel in the course of a night. I have seen this season, where they had attempted the present dam, piles of dirt, of at least one bushel.

They now travel over the dam in their migration, often climbing upright posts two or three feet high, to gain the pond above."

It is to be regretted that no specimens were procured in order to ascertain to which species the above Crawfishes belong: whether to *Cambarus fossor*, or *C. diogenes*, or some other. This deficiency, however, Dr. Stevens promises to supply hereafter.

No mention is made by Dr. Stevens of any mud chimneys being built upon the exterior surface of the wells, such as are constructed by *C. diogenes* in the district of Columbia. S. F. Baird.

IV. Miscellaneous Intelligence.

1. *On the Earthquake at Manilla, of Sept.* 16, 1852, (Proc. Bost. Soc. Nat. Hist., 1852, 300.)—The first shock occurred at 7 o'clock on the evening of September 16, 1852. The inhabitants all ran into the streets, expecting every moment the houses to fall into ruins, foreigners looking on with awe and astonishment, and the natives, better aware of the danger, were on their knees devoutly praying. The houses are built of stone, with very thick walls, and rather low, in order to withstand better such shocks of earthquakes, and yet many of them were completely destroyed. In one of the strongest houses, an occupant writes, that the lower story did not move much, but the upper one swayed to and fro, to use his expression, "like a blade of grass in the wind." The noise made by the breaking of walls, the falling of furniture, and the cracking and creaking of the timbers was such as to impress every one with an exaggerated idea of the destruction of property. The shock lasted about one and a half minutes; during the evening there were four more distinct shocks, at regular intervals of about an hour, namely, at eight, nine, ten, and eleven, and another at four the next morning. At each shock the great bell of the cathedral tolled, followed by all the bells of the city.

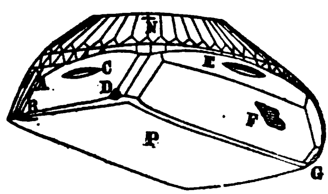
At night the city was almost deserted, from the danger of remaining in houses with tiled roofs; the inhabitants fled to the native houses of the suburbs with thatched roofs, and many slept in boats on the river. For two or three days after, there were several slight shocks, and for weeks several ships in the river were used as lodging houses.

This was the longest and most severe earthquake that has visited these islands for two hundred years. The damage to property was considerable, though the loss of life was small; only three or four lives are known to have been lost. Almost every stone house suffered more or less, according to its strength; nearly all the government barracks, the custom house, colleges, palace, theatre, and many private dwellings were rendered completely untenantable. Two churches were destroyed. One, the oldest in Manilla, founded nearly three hundred years ago by the Jesuits, very large, with walls and arches four feet thick, was thrown down into one immense mass of ruins. The movement was not slow and gradual, like a long heavy swell, but a quick succession of short sudden shocks. The effects of the shocks were different in different parts of the island; there did not seem to be any regular track pursued by the earthquake; in places within a few miles of each other, in one it was not felt at all, while in the other it was quite severe. At Mariveles, just across the Bay from Manilla, the earth opened with an eruption of black sand, which covered the country for a considerable extent; how large the opening was at the time is not known, but it is now seven hundred yards long and one yard wide. The volcanoes at Albay and Taal, which have not been in operation for many years, have been since discharging lava, stones, &c., with considerable activity."

Observations by Prof. H. D. Rogers.—Prof. Rogers referred to the circumstance that the undulatory movement of an earthquake is felt

much more sensibly at a point above the earth's surface than directly upon it. An instance illustrating this had come within his own knowledge. The earthquake which destroyed the principal city of Guadaloupe was felt in the city of New York, but only in the fourth story of a printing office. The sound generally precedes the shock, as has been observed in this country. In North America, the undulation is always parallel to the physical features of the continent, making it reasonable to believe that through a long series of epochs the motion has been in one rather than various directions, as supposed by Elie de Beaumont. There are two movements in earthquakes; an undulating and a molecular movement. The latter, Prof. Rogers thought was the movement which attracted most observation, giving rise as it does to sudden and abrupt changes of relation on the surface of the earth at places where the formation of the strata admits of more or less freedom of movement, causing the sudden shocks which are so destructive. Prof. Rogers gave an account of some of the opinions of geologists as to the thickness of the earth's crust. He gave it as his own opinion, that in most places it is not more than ten miles thick.

2. *The Koh-i-noor Diamond*, (from a Lecture by Prof. TENNANT.)—On closely examining the Koh-i-noor Diamond at Buckingham Palace, in company with my friend, the Rev. W. Mitchell, I found that two of the larger faces were cleavage planes; one of them had not been polished, or it had been so slightly polished that the effect was not discoverable. By measuring the stone with a goniometer, and ascertaining the inclination of its larger faces, 109° 28′, I ascertained which were the cleavage planes and which the cut planes of the diamond. Upon further examination I found two other cleavages, which make the stone correspond with an octahedron. * *



The flaws in the Koh-i-noor are shown in the annexed figure.

P, is a large plane at the base of the diamond, which is a cleavage plane. F, also a large cleavage plane, produced by a fracture—this had not been polished—and being inclined to the plane P at an angle of 109° 28′, it afforded a satisfactory means for determining the direction of the cleavage planes of the stone.

A shows a flaw running parallel to the cleavage plane F: this constituted the principal danger to be apprehended in cutting the stone, and was most skillfully ground nearly out before any of the facets were cut. This flaw seemed to proceed from a fracture marked B.

C and E were little notches cut into the stone for the purpose of holding the diamond in its original setting.

N, a small flaw, which almost required a glass to see it, evidently parallel to the plane P.

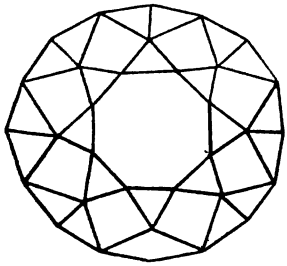
D, a fracture from a blow or fall, showing at its base a cleavage plane.

There is every probability that the Koh-i-noor is only a portion of the original diamond of that name, as procured from the mines of Golconda. My own opinion is, that in its original form this diamond was

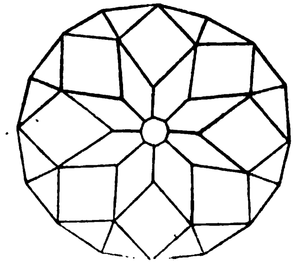
a rhombic dodecahedron, and that in its present state it is about one-third of the original size. I am confirmed in this opinion by Tavernier, who states that it originally weighed $787\frac{1}{2}$ carats; after having been broken or cut, it weighed $279\frac{9}{16}$; and if we make allowance for the difference between the French and English grains of that period, it would reduce it to 252 carats of the present time.* If we give Tavernier credit as to the original weight of the stone, we may indulge in a very reasonable supposition that two other remarkable existing diamonds once formed part of it. Dr. Beke, in a paper read before the British Association at Ipswich, in 1851, says, " At the capture of Coochan, there was found among the jewels of the harem of Reeza Kooli Khan, the chief of that place, a large diamond slab, supposed to have been cut [*broken* ?] from one side of the Koh-i-noor, the great Indian diamond, now in the possession of her Majesty. It weighed about 130 carats, showed the marks of cutting on the flat and largest side, and appeared to correspond with the Koh-i-noor." Another diamond, which singularly corresponds with the Koh-i-noor, is the great Russian diamond: and it is not improbable that they all formed one crystal, and that, when united, they would, allowing for the detaching of several smaller pieces in the process of cleaving, make up the weight described by Tavernier.

Note.—Since the date of my lecture the recutting of the Koh-i-noor has been successfully accomplished by the Messrs. Garrard. The brilliancy and general appearance of the diamond have been much improved, but the weight has been reduced more than a third. As exhibited in the Crystal Palace, the Koh-i-noor weighed $186\frac{1}{16}$ carats. I am indebted to Messrs. Garrard for the following account :—

" In cutting diamonds from the rough, the process is so uncertain that the cutters think themselves fortunate in retaining one-half the original weight. The Koh-i-noor, on its arrival in England, was merely surface cut, no attempt having been made to produce the regular form of a brilliant, by which alone lustre is obtained. By reference to the figures,



Upper Surface.



Under Surface.

THE KOH-I-NOOR IN ITS PRESENT STATE.

which are the exact size of the Koh-i-noor, it will be clearly understood that it was necessary to remove a large portion of the stone in order to

* " Les Six Voyages, &c., Seconde Partie, Paris, 1676." Tavernier, at page 249 of this volume, states the weight of the diamond to be $787\frac{1}{2}$ carats; but at page 334 he calls it 793 carats.

obtain the desired effect, by which means the apparent surface was increased rather than diminished, and the flaws and yellow tinge were removed.

"The process of diamond-cutting is effected by an horizontal iron plate of about 10 inches diameter, called a *Schyf*, or *mill*, which revolves from two thousand to three thousand times per minute. The diamond is fixed in a ball of pewter at the end of an arm, resting upon the table in which the plate revolves; the other end, at which the ball containing the diamond is fixed, is pressed upon the wheel by moveable weights at the discretion of the workman. The weights applied vary from 2 to 30 lbs. according to the size of the facets intended to be cut. The recutting of the Koh-i-noor was commenced on July 16th, 1852, his Grace, the late Duke of Wellington being the first person to place it on the mill; the portion first worked upon was that at which the planes P and F meet, as it was necessary to reduce the stone at that part, and so to level the set of the stone before the table could be formed; the intention being to turn the stone rather on one side, and take the incision or flaw at E, and a fracture on the other side of the stone, not shown in the engraving, as the boundaries or sides of the girdle. The next important step was the attempt to remove an incision or flaw at C, described by Professor Tennant and the Rev. W. Mitchell as having been made for the purpose of holding the stone more firmly in its setting, but pronounced by the cutters (after having cut into and examined it,) to be a natural flaw of a yellow tinge, a defect often met with in small stones. The next step was cutting a facet on the top of the stone immediately above the last-mentioned flaw; here the difference in the hardness of the stone first manifested itself, for while cutting this facet, the lapidary, noticing that the work did not proceed so fast as hitherto, allowed the diamond to remain on the mill rather longer than usual, without taking it off to cool: the consequence was, that the diamond became so hot from the continual friction and greater weight applied, that it melted the pewter in which it was imbedded. Again, while cutting the same facet, the mill became so hot from the extreme hardness of the stone, that particles of iron mixed with diamond powder and oil became ignited. The probable cause of the diamond proving so hard at this part is, that the lapidary was obliged to cut directly upon the angle at which two cleavage planes meet, cutting across the grain of the stone. Another step that was thus considered to be important by the cutters was removing a flaw at G. This flaw was not thought by Professor Tennant and Mr. Mitchell to be dangerous, because, if it were allowed to run according to the cleavage, it would only take off a small piece, which it was necessary to remove in order to acquire the present shape. The cutters, however, had an idea that it might not take the desired direction, and, therefore, began to cut into it from both sides, and afterwards directly upon it, and thus they succeeded in getting rid of it. While cutting, the stone appeared to become harder and harder the farther it was cut into, especially just above the flaw at A, which part became so hard, that, after working the mill at the medium rate of 2400 times per minute, for six hours, little impression had been made; the speed was, therefore, increased to more than 3000, at which rate the work gradually proceeded. When

the back (or former top) of the stone was cut, it proved to be much softer, so that a facet was made in three hours, which would have occupied more than a day, if the hardness had been equal to that on the other side; nevertheless, the stone afterwards became gradually harder, especially underneath the flaw at A, which part was nearly as hard as that directly above it. The flaw at N, did not interfere at all with the cutting. An attempt was made to cut out the flaw at A; but it was found not desirable, on account of its length. The diamond was finished on September 7th, having taken thirty-eight days to cut, working twelve hours per day without cessation."

By permission of her Majesty, models of the Koh-i-noor, as it arrived in this country and in its present state, have been placed with the diamonds in the mineralogical department of the British Museum.

3. *Abstract of a Meteorological Register, kept at the Tennessee Institution for the Deaf and Dumb, Knoxville, Tennessee, for the year* 1852; by O. W. Morris, Principal. The Latitude is 35° 56′; Longitude west from Washington, 6° 57′; elevation above tide, 960 feet.*

Months.	Barometer. Maximum	Barometer. Minimum.	Barometer. Means.	Barometer. Range	Thermometer. Maximum.	Thermometer. Minimum	Thermometer. Means.	Thermometer. Range	Cloudiness.	Prev'g Winds
Jan.	19, 29·692	5, 28·434	29·127	1·258	31, 63°·2	19, −4°·	30·513	67°·2	4·44	N.W.
Feb.	19, 29·431	28, 28·474	29·073	·957	24, 67·	3, 21·6	42·810	45·4	5·49	W.
March.	3, 29·451	28, 28·434	29·027	1·020	26, 79·8	20, 21·	52·401	58·8	5·02	W.
April,	10, 29·146	5, 28·348	28·856	·798	30, 79·	1, 32·	50·423	47·0	4·90	W.
May,	6, 29·442	27, 28·827	29·054	·615	2, 84·	21, 43·2	64·689	40·8	4·74	W.
June.	14, 29·391	8, 28·732	29·094	·659	23, 86·6	11, 50·5	68·498	36·1	4·32	W.
July.	21, 29·292	30, 28·833	29·068	·459	3&24, 92·5	3, 59·	74·096	33·5	4·01	N.
Aug.	30, 29·318	27, 28·775	29·086	·541	19, 87·5	7, 55·9	69·953	31·6	5·44	N.E.
Sept.	18, 29·316	11, 28·747	29·120	·569	2, 85·4	14, 45·	66·452	40·4	4·90	N.E.
Oct.	3, 29·360	9, 28·779	29·124	·581	8, 83·2	25, 39·6	59·656	43·6	3·79	N.E.
Nov.	29, 29·447	26, 28·488	29·048	·959	5, 71·2	19, 24·6	44·119	46·6	5·10	N & NE
Dec.	29, 29·425	7, 28·667	29·078	·75[illegible]	30, 64·8	14, 25·1	14·514	39·7	6·53	N.E.
An. M'ns.	29·392	28·628	29·063	·764	78·68	43·37	55·667	45·51	4·89	W.

The mean temperature of the winter months of 1851–1852, was 39°·279; of the spring months, 58°·838; of the summer months, 70°·849; of the autumnal months, 56°·742. The highest degree of the ordinary thermometer, as noted, was twice 92·5° in July; the lowest was −4° in January. The winter of '51–'52 was an uncommonly cold one for that country.

Rain fell on 114 days; it was accompanied by thunder and lightning on 32 days, on some of them twice, and on one day three times, with very heavy thunder and vivid lightning. On the third of May the lightning struck an oak tree within one hundred feet of the building.

Snow fell on 11 days, to the depth of 3½ inches; the first snow in the winter was on the 12th of December, and the last snow in the Spring was on the 2nd of April.

The mean relative humidity of the atmosphere for the winter months was 72·96; for the spring months, 66·89; for the summer months, 78·52; for the autumnal months, 77·79; and for the year it was 73·99. The most prevalent wind was the west, the next the northeast, and the next the northwest.

* In the columns Maximum and Minimum, the day of the month precedes the statement of the observation.

The Aurora Borealis was observed on four nights only, viz: Jan. 19th, Feb. 19th, April 22d, and May 9th; that on the 19th of January was the only one that exhibited any perceptible motion, the others were a steady light; that on the 9th of May was a faint light, only a few degrees above the horizon. A solar halo was noticed once, and lunar haloes on fourteen nights. Both solar and lunar haloes were followed by rain or snow within a short period of time.

A shock of an earthquake was felt on the 27th of October, at 11.30 A. M., barometer 29·171, thermometer 71·8°. At 6 A. M. the barometer was 29·224, and at 10 P. M. 29·212;—the direction of the wave was from west to east; the air was very sultry, with no perceptible wind, the atmosphere hazy and smoky. The loose boards on the buildings rattled, and so did crockery, &c. on the shelves, looking glasses and other pendant articles swung from side to side, horses that were trotting or walking in the road involuntarily stopped, and the domestic animals seemed stupefied and in some instances frightened. The jar was accompanied by a noise resembling distant thunder for a second or two, then a loud report like firing heavy artillery, followed by a sound resembling the reverberations of cannon among the mountains, dying away in the distance. Many people throughout the country were much alarmed, and ran out of their houses.

At 8 o'clock P. M. of the same day there was a very bright meteor; its course was from N. W. to S. E., leaving a track of bright light, 8 or 10° in length, which continued several seconds; it was at some distance above the earth; no report was heard when it disappeared.

4. *On the Binocular Microscope, and on the Stereoscopic Pictures of Microscopic Objects;* by Professor C. WHEATSTONE, F.R.S., (Quart. Jour. Mic. Sci., No. iv, July, p. 99.)—In Section 11 of my first Memoir on Binocular Vision, published in the Philosophical Transactions for 1838, I have alluded to the illusions to which microscopic observers are liable, from their inability to judge correctly the relief of objects when one eye only is employed. This indetermination of the judgment exists whenever a shadowless object is regarded with a single eye. Frequently an elevation appears as a depression, a cameo as an intaglio, a hollow pyramid (as a crystal of muriate of soda) as a pyramid in relief, &c., and *vice versâ;* but this indecision is entirely removed when the object is viewed with both eyes simultaneously. No mistake, if the object be a near one, can then be made with regard to its relief; and the relative positions of every point, in depth as well as in length and breadth, can be directly determined.

The stereoscope affords a convincing proof that the two projections of an object presented to the two eyes, suggest the real object far more effectively to the mind than a single projection to one eye does; and those who have paid much attention to the appearance of binocular pictures in the stereoscope, will not have failed to remark, that not only is double vision of importance to enable us more accurately to judge of the relief of bodies, but it also occasions us to perceive things which pass entirely unnoticed when monocular pictures alone are regarded. Fully impressed with these views, and convinced, from the reasons above stated, that a binocular microscope would possess great advantages over the present monocular instrument, I, shortly after the publica-

tion of my first memoir, called the attention both of Mr. Ross and Mr. Powell to this subject, and strongly recommended them to make an instrument to realize the anticipated effect; their occupations, however, prevented either of these artists from taking the matter up. The year before last, previous to the publication of my second memoir, I again urged Mr. Ross, and subsequently Mr. Beck, to attempt its construction, and for a short time they interested themselves in the matter, but ultimately relinquished it for want of time, and in my opinion overestimating the difficulties of the undertaking.

It appears, however, from a communication in the 'American Journal of Science' of January 1853, which has been reprinted in the last number of the 'Microscopical Journal,' that such an instrument has been actually constructed by Professor J. L. Riddell, of New Orleans, and the results expected have been obtained. The method Mr. Riddell employs is similar to the one I recommended to Mr. Beck. After the rays from the object pass through the compound object-glass in the usual manner, he deflects them by means of a system of rectangular prisms into two directions parallel to the original, and sufficiently separated for the images to be seen by each eye. As in this arrangement there must be a considerable loss of light, I have proposed another which will not have this disadvantage, and which I will shortly submit to the Society.

A binocular microscope is, however, by no means a novelty, and its invention dates nearly two centuries back. I have found, in the library of the Royal Society, a work entitled 'La Vision parfaite, ou les Concours des deux Axes de la Vision, en un seul point de l'Objet: Par le P. Cherubin d'Orléans, Capucin.' This work was published at Paris in 1677, and in it eight chapters and a plate are devoted to a minute description of the instrument, which he informs us he constructed, and presented to the Dauphin. The following is an extract from the Preface:—

"Some years ago I resolved to effect what I had long before premeditated, to make a microscope to see the smallest objects with the two eyes conjointly; and this project has succeeded even beyond my expectation, with advantages above the single instrument so extraordinary, and so surprising, that every intelligent person to whom I have shown the effect has assured me that inquiring philosophers will be highly pleased with the communication. For this reason I have determined to make it the principal subject of the present work."

And the second part, which contains a description of the instrument, is thus headed:—

"Section the first, in which is taught the method of constructing the newly-invented microscope to see the smallest objects very agreeably and conveniently, represented entire to the two eyes conjointly, with a magnitude and distinctness which surpasses everything which has been hitherto seen in this kind of instrument."

In the Père d'Orléans binocular microscope, two object-glasses have their lateral portions cut away so as to allow of close juxtaposition, and these nearly semi-lenses are so arranged, that their axes correspond with the two optic axes passing through the tubes containing the eye-

pieces. The author's aim in its construction was solely the reinforcement of the impression by presenting an image to each eye, for he assumes, according to the then prevalent error, that vision by the two organs conjointly is naturally and necessarily unique, from the perfect conformity of all the homonymous parts of the two images of the object on the two retinæ. The real advantage of such an instrument entirely escaped his attention; viz., that of presenting to the two eyes the two *dissimilar* microscopic images of an object, under precisely the same circumstances as the two unlike images of any usual object is presented to them when no instrument is employed, by which simultaneous presentment the same accurate judgment as to its real solid form, and the relative distances of all its points, can be as readily determined in the former case as in the latter.

In the construction of a binocular microscope there is one thing especially to be attended to—viz., that the images be both direct, for in this case only a true stereoscopic representation will be obtained. If the images, on the contrary, be inverted, a pseudoscopic effect would be produced which will give a very erroneous idea of the real form. The reason of these effects is fully explained in Sections 5, 10, 22, 23, of my Memoirs. The reversal of the images by reflection from mirrors or reflecting prisms, will produce the same result as to the stereoscopic and pseudoscopic appearances as their inversion by lenses. The binocular microscope constructed by the Père d'Orléans was pseudoscopic, though he describes one which, had it been made, would have been stereoscopic; he was, however, quite unaware that there would be any difference of this kind between them. The pseudoscopic effects when inverted images are presented, and the natural appearances when erecting eye-pieces are employed, have not escaped the observation of Mr. Riddell.

Besides actual inspection by means of the binocular microscope, there is another way in which the advantages of binocular vision may be applied to microscopic objects. The beautiful specimens of photography, reproducing the highly magnified images of objects, inserted in a recent number of the Microscopic Journal, makes one regret that they were not accompanied by their stereoscopic complements. A very simple modification of the usual microscope would fit it for producing the two pictures at the proper angles; all that is necessary is to cause the tube of the microscope to move independently of the fixed stand round an axis, the imaginary prolongation of which should pass through the object. A motion of 15° would include every difference of relief which it would be desirable to have, and it is indifferent in what direction this motion is made in respect to the stand. The pair of stereoscopic pictures may be obtained by a still simpler method, which requires no alteration in the microscope; the object itself may be turned round on an imaginary axis within itself, from 7° to 15°. But this method is inapplicable unless the light be perfectly diffused and uniform so as to avoid all shadows, the presence of which would give rise to false stereoscopic appearances. In the former case, where the object remains stationary and the tube moves independently of the frame, the arrangement of the light so as to cast single shadows might be an advantage, and assist the visual judgment.

5. *On the periodic and non-periodic variations of Temperature at Toronto in Canada from* 1841 *to* 1852 *inclusive;* by Colonel Edward Sabine, R.A., Treasurer and Vice-President of the Royal Society, (Phil. Mag., [4], v, 376.)—The principal object of this communication is to make known the non-periodic variations of temperature for every day in the twelve years, from 1841 to 1852 inclusive, at Toronto in Canada. The non-periodic variations are those differences of the temperature from its mean or normal state which remain after all the known periodical variations are allowed for, and are such as are generally accompanied by peculiarities of wind or of other meteorological circumstances. Recent investigations have led to the inference that opposite conditions of weather prevail simultaneously in the same parallels of latitude under different meridians, and that in particular Europe and America usually present such an opposition, so that a severe winter here corresponds to a mild one there, and *vice versâ;* and recent theories of the distribution of heat on the surface of the globe profess to furnish the explanation. To place the facts on indisputable ground, it is requisite that a comparison should be made of unexceptionable records of the non-periodic variations in Europe and America, continued for a sufficient time to afford a proper basis for inductive generalisation. Toronto, from its latitude 43° 40′ N. and inland situation, is well suited to supply such a comparison with stations in the middle parts of Europe where similar records have been kept; and the twelve years embraced by the observations, viz.: 1841 to 1852, have been years of unusual meteorological activity in Europe.

Details are given in the commencement of the paper showing the care bestowed on the examination of the thermometer employed, with a standard divided "*à l'échelle arbitraire*," by the method of M. Regnault; as well as the precautions adopted for its fair exposure, and for its protection from rain and radiation. The observations were made by the non-commissioned officers of the detachment of the Royal Artillery employed in the duties of the observatory.

The period of twelve years comprises two series, in one of which the thermometer was observed hourly, and in the other less frequently, each observation in the second series receiving however a correction to the mean temperature of the day furnished for every hour and every day of the year by the first series. The two series, each of six years, are separately discussed; from the first series equations are derived from the mean monthly temperatures by the method suggested by Bessel (Astron. Nach. No. 136), whereby the most probable values of the temperature, on every day and every hour, are computed corresponding to the whole body of the observations. These the author regards as approximate normal values, and by comparing with them the actual daily temperatures,—which in the first six years are the means on each day of twenty-four equidistant observations, and in the second six years the means of all the observations made on each day, each observation having been corrected for the hour in the manner described,—the non-periodic variations for every day in the year are obtained and are given in a table.

From the approximate normal temperatures the author has represented in a plate the phenomena of the temperature at Toronto, according

to a method which, if applied to the different meteorological elements and in different localities, might, he thinks, materially facilitate their intercomparison. This method, in which three variables are represented, one being dependent on the other two, is essentially the same that has been long used in magnetic maps, and in the ordinary isothermal maps; from which latter however it differs in this respect, that, whereas in the ordinary isothermal maps the two variables on which the variation of temperature is dependent are the geographical latitude and longitude, in the present case the two variables are the hour of the day and the day of the year. The variation of temperature is here referred therefore to *time* and not to *space;* a distinction which the author proposes to convey by employing the term Chrono-Isothermals, as applicable to lines of this description. From the delineation in the plate, and from the tables contained in paper, many characteristic and some peculiar features of the climate and meteorology of the part of the North American continent in which Toronto is situated, are readily perceivable. Several instances are pointed out; amongst these may be noticed the peculiar anomaly of the North American winter, which is very conspicuous in the plate; and the absolute as well as relative *variability* of the temperature at different seasons of the year, exhibited by means of a numerical index analogous to the probable error of the arithmetical mean of a number of partial results, and deduced in a similar manner from the differences of individual years, months, and days, from their mean values: whence it appears, in respect to the annual temperature, for example, that in any particular year there is an equal probability that its mean temperature will fall within the limits of 43°·8 and 44°·6, as that it will exceed those limits on either side.

Finally, the author has shown the "Thermic Anomaly" (as it has been recently termed) of the monthly and annual temperatures at Toronto by comparison with the normal temperatures computed by Dove (Verbreitung der Wärme, 1852), for the parallel of 43° 40′ N. from 36 equidistant points on the parallel; from which comparison it appears that after allowance has been made for the elevation above the sea (342 feet), every month of the year is colder than the normal temperature of the same month in the same parallel; that the thermic anomaly reaches its extreme in February, when it exceeds 10° of Fahrenheit; and that on the average of the whole year it is little less than 6°.

6. *On Periodical Laws in the larger Magnetic Disturbances;* by Captain YOUNGHUSBAND, R.A., F.R.S., (Phil. Mag., [4], v, 379).—In this communication the author has arranged, in tables, the disturbances of the magnetic declination at St. Helena and the Cape of Good Hope, for the purpose of exhibiting the systematic laws by which those phenomena are regulated, which were long described as irregular variations, because they were of occasional and apparently uncertain occurrence.

The frequency of the disturbances, and their amount, whether viewed separately as easterly or westerly movements, or as general abnormal variations (easterly and westerly being taken together), is shown to be dependent upon the hour of the day, the period of the year, and upon the year of observation. This dependence upon the year of observation affords additional testimony of a periodical variation in the magni-

tude of magnetic changes of the same character as that which has been found to exist at other places, and which has been considered to be coincident with variations of the solar spots.

The disturbances of larger amount only are noticed; those observations which differed by 2·5 scale divisions (1′·8 in arc at St. Helena, and 1′·9 in arc at the Cape) and upwards, from the normal place, were separated from the others and the values of the differences taken; there were therefore two series of figures to be dealt with, viz: the number of disturbances, and the aggregate amount of disturbance. These were separated into disturbances of the north end of the magnet towards the east and towards the west, and the effect of each considered separately.

The periodical character of disturbances at St. Helena and the Cape in a cycle of years is indicated so far as the limited extent of the observations would permit; sufficient however to point to the year 1843 as that of least disturbance at these two places, by showing a regular decrease from the previous years, and an increase in every succeeding year of observation. Though the hourly observations were discontinued before 1848, the year which Colonel Sabine has shown to be that of periodical maximum, (as 1843 was that of minimum magnetic activity at Toronto and Hobarton,) the observations now discussed are shown to be quite consistent with this period, and thus tend to establish it as a general law of magnetic phenomena. In the aggregate of each year the disturbances towards the west are shown to preponderate over those towards the east, both at St. Helena and the Cape of Good Hope; a similar preponderance of westerly over easterly has been found in every year of observation at Hobarton, but at Toronto the easterly disturbances exceeded the westerly both in number and amount in every year.

Arranging the disturbances into the several *months* of their occurrence, the greatest disturbance is found to occur in January and the least in June at St. Helena and the Cape of Good Hope; the same months being those of greatest and least disturbance at Hobarton, whereas at Toronto, *both* January and June are months of minimum disturbance, the maxima disturbance occurring there in April and September.

From this identity of the epoch of greatest and least disturbance,—at St. Helena, where the months of January and June are not those of opposite seasons, viewed either with respect to the sun's extreme altitude or to extreme periods of temperature,—at the Cape, situated in S. latitude 33° 56′,—and at Hobarton in S. latitude 42° 52′,—and contrasting this identity with a different law at Toronto in N. latitude 43° 39′, the author infers that the principal causes which produce an annual period of disturbance are not dependent upon local seasons. It is likewise pointed out that about the period of the equinoxes there is a tendency to maximum disturbances at all the stations, producing absolute maxima at Toronto, faintly but systematically indicated at the other stations.

The westerly disturbances were found to exceed the easterly in every month in the year at St. Helena and the Cape, which agrees with the results deduced from the Hobarton observations, while it appears

from the observations at Toronto that the easterly disturbances exceeded the westerly in every month. The average value of a westerly disturbance is greater than that of an easterly in every month at St. Helena and the Cape of Good Hope. The disturbances at Hobarton again coincide with this result; and in a slight and less perfectly marked degree, Toronto has the same peculiarity.

Arranging the disturbances into the several *hours* of their occurrence, the hours of the *day* are found to be those of greatest disturbance in a very considerable degree; the sum of the ratios, during the twelve hours of the day, being about seven times as great as the sum of those in the twelve hours of the night at St. Helena, and about 2·6 times as great at the Cape of Good Hope; while at Hobarton the sum of the twelve night ratios slightly exceeded the day; at Toronto the excess was larger, viz: as 1·3 to 1. The laws of easterly and westerly disturbances, in relation to the local hours, are then examined separately. At St. Helena and the Cape, the easterly day-disturbances exceed the easterly night-disturbances, and the westerly day-disturbances exceed the westerly night-disturbances. These results are compared with those at Toronto and Hobarton.

At St. Helena, although but comparatively few disturbances occur during the night hours, those disturbances are almost all westerly (183 disturbances, in all, occurred in nine night hours during five years, of which 174 were westerly and but nine easterly). In the day hours the westerly only *slightly* exceed the easterly disturbances. At the Cape, the westerly excess is less in the night and greater in the day than at St. Helena, and the night excess much greater than the day excess.

At St. Helena, the fact of the disturbances being more frequent in the day than in the night is consistent in every month of the year; this appears worthy of remark when it is remembered that at St. Helena the curve of the diurnal variation of the declination is precisely reversed at two opposite periods of the year; in one case corresponding to the curve of diurnal variation in middle northern latitudes, and in the other to that in middle southern latitudes.

The mean effect of the disturbances which have been separated as described, and which comprise all of largest magnitude, is a *constant westerly* effect at every hour both at St. Helena and the Cape of Good Hope, acting more energetically in the night than in the day. At Toronto, the mean effect is westerly in the day and easterly in the night; at Hobarton, easterly in the day and westerly in the night.

7 *Mode of Determining the Optical Power of a Microscope;* by Prof. Harting, (Quart. J. Mic. Sci., July, 1853, 292.)—I conclude by noticing another method of testing the optical power of the instrument, which, although rather troublesome, appears to me among the best, permitting us, as it does, to ascertain with a great degree of accuracy and certainty, the utmost limits of penetrating and separating power possessed by a microscope, and hence easily to express numerically its optical qualities in the most varied circumstances.

This method consists simply in subjecting to observation under the microscope the dioptric images of certain minute objects instead of the objects themselves. These images can be diminished at pleasure by withdrawing to a distance from the lens the object which forms them;

and hence we have it in our power to measure the extreme limits at which the object continues to be visible.

For the formation of the dioptric images achromatic object-glasses might be used; but even where those of the shortest focal length are employed, the object whose image it is required to form must be placed at a great distance. This would cause various difficulties, and only be practicable with a microscope placed horizontally—unless, indeed, the object selected were very minute, in which case the accurate determination of its diameter (from which that of its image must be afterwards deduced) would be rendered difficult.

Small air-bells in a fluid are for this purpose far better. I employ by preference a watery solution of powdered gum arabic, which always contains numbers of such air-bells originating in the air entangled among the particles of the powder. The water employed should have stood for a considerable time freely exposed to the air, or been shaken up with the air for some time; for when we use water which is not saturated with air, the bubbles in the fluid gradually become smaller, and images formed in them decreasing in magnitude, cause errors in the subsequent measurements, as we shall actually find to be the case.

A drop of the fluid must then be placed on a clean glass object-slide, and covered with a good clear mica plate, a ring shaped piece being interposed, in order to prevent the flattening of the air-bells by pressure. The object-slide is then placed under the object-glass upon the stage of the microscope, and an air-bell of suitable size for the formation of the images is sought for. All do not give images of the same degree of sharpness; a peculiarity dependent on the fact that some air-bells are in contact with the covering-plate, and consequently have their spherical form disturbed to some extent, or on the presence of small molecules in the fluid above or beneath the air-bell, or even in its interior, causing some haziness of the image, just as defective polish of a glass lens would do. It will, however, be always easy to find some which will form images of the utmost distinctness and purity.* This may be ascertained in the first instance by holding between the mirror and stage some easily recognized object, *e. g.* a piece of paper or the like. The image is always formed on the under surface of the air-bell, which must consequently be brought nearer to the object-glass than when it is desired to bring its margins into focus.

The object whose image is to be the subject of examination should be placed upon an apparatus, which can be moved upward and downward in the space between the mirror and stage. In some microscopes this can hardly be done, either from the space being too limited, or in consequence of the drum-like form of the foot of the microscope which quite envelops the space. If such microscopes, in place of a mirror, be provided with a reflecting prism, the object may be placed opposite the side external to the microscope. The instruments best adapted for

* The following example will demonstrate this. I brought a printed page of a book to such a distance from an air-bell that the length of the image of the whole page was 1-7th millimetre = about 1-180th of an inch, and that of the image of each letter about 1-480th millim. = 1-12,000th of an inch. In spite of their minuteness, these images, formed by reflected light, possessed such clearness and sharpness, that under a magnifying power of 154 diameters the whole page was without difficulty legible.

the manipulation which we are describing are, however, those whose illuminating apparatus consists of a mirror and converging lens, which can be shifted up or down. The lens being removed from the ring which supports it, the object is substituted in its place. The relative magnitudes of object and air-bell must be such that the image shall be exceedingly minute when the object is tolerably near to the stage. On afterwards increasing the distance between the object and air-bell, it is not difficult to find the limit at which the image (under a given magnifying power) is barely visible.

Of course it is impossible to measure *directly* the dimensions of this most minute visible image, for our best micrometric methods will here be found of no avail. Yet their size may be estimated with extreme accuracy in the following manner. At the same distance from the air-bell and in place of the object used, substitute another body, such as a piece of card, of 4 to 5 centimetres = $1\frac{3}{5}$ths to 2 inches diameter, which has been exactly measured. Let this be now again measured (by some of the micrometric methods elsewhere alluded to*), just as if it were a real object. By dividing the real diameter by the apparent diameter, the amount of diminution is found; and this is the same for all objects at a like distance from the air-bell. We have, consequently, nothing to do, in order to find the amount of diminution of the image of the more minute object, but to divide its *true* diameter by the figure expressing the diminishing power.

For example, let the true diameter of the greater object be 5 centimetres = to 1·969 English inches, and the diameter of its image = 32·2 micromillimetres,† = ·00127 English inches, then the figure expressing the amount of diminution will be $\frac{1\cdot969}{\cdot00127}$ = 1553 very nearly. If now the smaller object have a diameter of 175 micromillimetres = ·00689 English inches, then must its image at the limit of vision be in diameter = $\frac{\cdot00689}{1553}$ = ·0000044, or about $\frac{1}{225,000}$th of an English inch. When exact micrometric methods are employed, it is easy in this way to estimate the diameter of an image even to millionth parts of a millimetre, *i. e.* to 25,400,000th parts of an inch.

As for the object suitable for these investigations, it is plain that we have an extensive choice. To find the limit of vision for bodies of a round or long thread-like form, grains of pearl sago, or vegetable bodies, such as mustard-seed or the pollen-granules of many plants, hairs of animals, metallic wires, &c., may be employed. Small round openings and chinks may serve for the determination of the visibility of positive images of light. In the last case, care must of course be taken, by means of suitable screens, to shut off all light except what passes through the aperture. To determine the defining power, metallic wire gause is a suitable object, or two holes placed near each other in a black metallic plate. The images of such objects resemble exactly a double star viewed through a telescope (kijker). The bodies may likewise be placed in different circumstances in order to ascertain the influence of these upon the limits of vision. Thus we may use as an object a very

* See translation from *Het Mikroskoop* in Monthly Journal of Medical Science, June. 1852. p. 453. *et seq.*

† The micromillimetre is equal 1-1000th millimetres = ·0000394 English inches. See *Monthly Journal*, June, 1852, p. 456.

thin glass capillary-tube placed in water, and compare it with tender organic tubes and vessels, which may also be seen in water, but whose limit of visibility is of course far more circumscribed than that of absolutely opaque objects.

In fact this method admits of innumerable variations, and is consequently of most extensive application. Besides, when proper precautions are taken, it gives results perfectly sure and comparable. Especial care is however requisite in the mode of illumination. For it is certain, that when the field has a clear white ground, the contrast causes minute opaque bodies (*i. e.* objects which are dark by transmitted light) to continue visible, which against a grayish or light-blue background could not be seen. Hence it is by no means indifferent to receive on the mirror light from a white cloud, from a dull overcast, or clear blue sky. Artificial light cannot be used in these experiments, for the image of the flame becomes diminished like the object, and hence a clear field of view is not to be obtained. The observations must consequently be made by daylight; and whenever comparable results are sought for, the mirror should always be directed to the clear, blue, cloudless sky—this being a distinct atmospheric condition to which others in similar circumstances may refer in conducting the same experiment. The mode of ascertaining the limit of vision, with a given amount of illumination, may be gathered from different examples in the body of this work. It will likewise be found that for all such observations, even when the highest magnifying powers are employed, *the flat* mirror is perfectly sufficient, since in the image in the field of view formed by the air-bell, all the rays proceeding from the mirror are united and constitute an object of considerable luminous intensity.

8. *On an Instrument for taking Soundings;* by F. Maxwell Lyte, Esq., (Phil. Mag., [4] vi, 344.)—I see, from what Dr. Scoresby has brought before the Association at Hull, that there seems to be some difficulty about obtaining correct soundings in places where the currents are strong and flow in different directions at the different points of depth, causing the line to assume different curves in its descent; and when it comes to be measured over, after the weight has reached the bottom and been hauled up again, the measurement gives no approximate idea of the real depth. Now it is plain that this mensuration of the depth of water might be as well made by estimating its vertical pressure, as, in measuring the height of mountains, we measure the barometrical pressure of the air; and so I would propose to do it by an instrument constructed as follows:—

An accurately constructed tube of gun-metal or brass, or some metal not very easily corrodible by salt water, has a glass tube fitted on to it on the top by a screw joint, and again on the top of the glass tube is fitted a strong hollow copper ball by a similar screw joint. The lower tube, which we will call *a*, has a well-turned piston fitted to it, from which runs a rod which is only a trifle longer than the tube *a*, and just enters the tube *b* when the piston is at its lowest point. A well-made spring is placed in the tube *a* above the piston, and the tube *a* being narrowed at the top, so as just to admit the free passage of the rod, and the rod having a little button at its top, the piston is kept at its lowest point by the spring, except when sufficient pressure is applied from below

to compress the spring. The glass tube has a small ring fixed in it, just so as to stick at any point to which it is pushed, and the button at the top of the rod serves to push the ring straight, and the ring thus forms an index of the degree to which the spring has been compressed. The ball on the top serves as a mere reservoir of air to equalize the action of the apparatus as much as possible. The whole of this apparatus is enclosed in a wire cage for the sake of protection from blows. To graduate this apparatus, I let it down in a known depth of water, say ten fathoms, and having observed the point to which the ring in the glass tube is pushed, and having marked this point off, the ball is to be unscrewed, and with a small ramrod the ring is to be pushed down till it rests on the top of the piston-rod. The ball being replaced, the apparatus is sunk in twenty fathoms; after a similar manner it is sunk in thirty, and next in forty fathoms. This will test the accuracy of the apparatus; and the marks made on the glass tube *b* after each trial will give a scale from which the whole tube may be graduated, even to thousands of fathoms, if the tube be long enough or the spring strong enough. I have been induced to make this communication on account of the great use which may be made of such an apparatus.

9. *Louis Sæmann.*—Mr. Sæmann, who formerly travelled through the U. States, has arrangements at Paris for the sale of specimens or collections in Mineralogy, Geology, and Palæontology. To a thorough knowledge of minerals, Mr. S. unites a most excellent and obliging disposition; and his establishment is one of the largest in Europe. His address is Louis Sæmann, Comptoir Mineralogique et Palæontologique, Rue St. Andrè-des-Arts, No. 45, Paris.

10. *Cabinet of Minerals for sale.*—A large and excellent cabinet of minerals, and mineralogical works accompanying, is for sale at Washington. Address Fr. Markoe, Esq., Washington City. It is one of the best private collections in the country.

11. Obituary.—James E. Teschemacher died suddenly near Boston on the 9th of last November. Mr. Teschemacher, although engrossed with other cares, has been an unceasing and successful laborer in Science. He was an exact observer, and delighted in searching out with his microscope what passed unnoticed by others. He has contributed much to our knowledge of American minerals, both through the detection of rare forms of species and by his publications. A letter from him written but two days before his death, states his plans for research through the winter, and the progress he had already made in his chemical examination of the Chesterfield pyrochlore. He was most generous in communicating the results of his researches and allowing the free use of his Cabinet of Minerals to those who appreciated its excellencies. They were mostly miniature specimens, but often of rare interest. He has been laboring of late on the subject of the Fossils connected with coal, and the structure of the coal itself, and had collected much that was novel, which he was preparing for publication.

12. *Die Kreidebildungen von Texas, und irhe organischen Einschlüsse;* Von Dr. Ferd. Roemer. 4to. Bonn, 1852.—This work has already been briefly noticed in the September number of this Journal. The Introduction contains, 1. A sketch of the geographical situation and

general orographic condition of Texas; 2. General geognostic constitution of the country; 3. Diluvial and alluvial formations; 4. Tertiary formations; 5. Older or Palæozoic strata; and 6. Plutonic rocks.

The principal part of the volume is devoted to a geological description of the chalk formation of Texas, with an enumeration and description of its organic remains, which occupy eighty-eight pages of the volume. These are followed by descriptions of Palæozoic fossils, and of three species of fossil wood from the tertiary.

The cretaceous fossils figured occupy ten quarto plates, and number one hundred and twenty-four. Of these one hundred and one are new or yet undetermined species, and twenty-three are identical with species previously known. These are beautifully illustrated. The Palæozoic fossils number ten species, of which eight are new. These are chiefly of the carboniferous period, and we recognize them as forms which prevail farther to the north and west in the same formation. The three species of fossil wood from the tertiary, described by Unger are *Sillimania Texana*, *Roemeria Americana*, and *Thuyoxylon Americanum.*

The fossils here described and figured, had been already indicated in Dr. Roemer's previous work on Texas.*

The present work offers a very valuable accession to our knowledge of the American Cretaceous formation; contributing more species of fossils than have been published since the appearance of Dr. Morton's Synopsis in 1830.* Dr. Roemer has given some valuable observations on the climatic influences upon the fauna of the chalk period, and has instituted comparisons between this formation in Texas and other parts of America, as well as with the same formation in Europe.

On comparing these figures of Texan cretaceous species with collections from Nebraska, a few degrees farther north, we are struck with their almost total dissimilarity. In the little which we already know of it, we have but a foreshadowing of what is yet in store for us when this formation, which extends from the Tropic of Cancer to the 48th degree of latitude, shall have been completely explored. H.

13. *Geological Map of Keweenaw Point, Lake Superior, Michigan;* by J. D. Whitney, assisted by S. W. Hill and W. H. Stevens.—This is a large pocket map of the Lake Superior Mining Region, 2 feet by 4 in its dimensions. It gives an admirable view of the Geological structure of the region, and is excellent in illustration of an article in this volume from the Report of Messrs. Whitney and Foster. The different rocks are indicated as usual by colors, and many facts of great geological interest are indicated by their arrangement, and the positions of the various metallic veins which intersect them. The map contains also a section across Keweenaw Point by Copper Falls and Northwestern Mines. It is invaluable to geological science as well as to the topographer and traveller.

14. *People's Journal;* Vol. I, Nos. 1 and 2, November and December, 1853. 32 pp. large 8vo.—This new popular monthly opens with an article on Willison's Hand Thrashing Machine. The Journal is de-

* Texas: mit besonderer Rucksicht auf deutsche Auswanderung und die physicalischen Verhältnisse des Landes nach eigener Beobachtung geschildert; von Ferdinand Roemer. Mit einen naturwissenschaftlichen Anhange und einer topographische-geognostischen Karte von Texas. Bonn, bei A. Marcus, 1849.

voted to Farming, Agriculture, Horticulture, Mechanics, and practical science, and is very fully and handsomely illustrated with cuts representing machinery, implements of husbandry, fruit, cattle, plans of buildings, the National Exhibition of Horses at Springfield, American steamships, etc. No. 2 contains 72 wood-cuts, some of them illustrating important inventions at the Crystal Palace. Price 50 cts. each 6 months.

E. Hitchcock, D.D.: Outline of the Geology of the Globe, and of the United States in particular; with two Geological Maps and sketches of characteristic American Fossils. 8vo. pp. 138. Plates. *Boston*, 1853. Phillips, Sampson & Co.

Thomas Cole, A.M.: List of Infusorial Objects, found chiefly in the neighborhood of Salem, Massachusetts, with a sketch of the progress of this branch of Natural History. From the Proceedings of the Essex Institute. 18 pp. 8vo. *Salem*, 1853.

Magnetical and Meteorological Observations at Toronto, in 1843, 1844, 1845, printed by order of her Majesty's Government, under the superintendance of Col. Edward Sabine. 640 pp. 4to. *London*, 1853.

B. Studer: Geologie der Schweiz (Geology of Switzerland), in 2 vols. 12mo. *Berne* and *Zurich*, 1853.

E. F. Kelaart, M.D.: Prodromus Faunæ Ceylonicæ. 198 and 62 pp. 8vo. *Ceylon*, 1852.—Treats with considerable detail, of the Mammalia and Reptiles of Ceylon.

Annales de l'Observatoire Physique Central de Russie, Parts 1 and 2 for 1850. 808 and 250 pp. 4to. *St. Petersburg*, 1853.

Compte-Rendu Annuel addressé à S. Exc. M. de Brock, Min. des Finances, par le directeur de l'Observatoire Physique Central, A. T. Kupffer, année 1852. 72 pp. 4to. *St. Petersburg*, 1853.

C. F. Rammelsberg: Fünftes Supplement zu dem Handwörterbuch der chemischen Theils der Mineralogie. 270 pp. 8vo. *Berlin*, 1853.

C. F. Naumann: Anfangsgründe der Krystallographie, 2nd Edition. 292 pp. 8vo. *Leipzig*, 1854.

Liebig and Kopp: Jahresberichte for 1852. Parts I and II.

Proceedings Acad. Nat. Sci. Philadelphia, vol. vi, No. 10.—p. 375. Note on Cambarus Gambelii; *Agassiz*.—p. 377. Note on Cetacean remains in the U. States; *Leidy*.—p. 378. New Rana and Bufo; *Baird*.—p. 379. On Thalia; *Genth*.—p. 380. A new Salmo; *C. Girard*.—Note on Cambarus Gambelii; *Girard*.—p. 381. Notes on the Ornithology of Wisconsin; *P. R. Hoy*.—p. 386. On the American species of Esox; *C. Girard*.—p. 387. New species of Fish collected by J. H. Clark on the U. S. and Mexican Boundary Commission; *Baird* and *Girard*.—p. 390. New Arkansas fishes; *Baird* and *Girard*.—Note on Nebraska Mammalian and Chelonia Fossils; *Leidy*.—No. 11.—p. 395. Catalogue of Birds of Ohio; *N. C. Read*.—p. 402. New species of Pacane Nut; *J. Le Conte*.—p. 404. Three new American Arvicolæ, and remarks on other Rodents; p. 415. On Crotalus durissus and C. adamanteus; *J. Le Conte*.—p. 420. New Reptiles of the Exploring Expedition under C. Wilkes; *C. Girard*.—p. 425. Notes on the Ornithology of Wisconsin; *P. R. Hoy*.—p. 430. New species of Diatomaceæ, collected by U. S. Exploring Expedition under C. Wilkes; *W. H. Harvey* and *J. W. Bailey*.

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Vindiciæ Plinianæ, scripsit C. L. Urlichs. Fasc. I. Gryphiæ, 1853. In Libr. C. A. Kochiana, Th. Kunike.

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Leonhard Euler's Theorie der Bewegung fester oder starrer Körper mit Anmerkungen und Erläuterungen, herausgegeben von J. Ph. Wolfers. Dr. und Professor. 1st and 2nd parts. Greifswald, 1853. C. A. Koch's Verlagsbuchhandlung.

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[SECOND SERIES.]

ART. XII.—*On the Elastic Force of Heated Air, considered as a Motive Power;* by FREDERICK A. P. BARNARD, Professor of Chemistry and Natural History in the University of Alabama.

IN the discussions which have recently taken place with regard to the power, economy and value of hot-air engines, the attention of all who have participated in them appears to have been exclusively directed to the advantages and disadvantages presented by a particular case. The question seems not to have been contemplated in its more general aspect; or rather it seems to have been assumed that there exists no general question which the success or failure of this particular experiment will not settle. Those who are satisfied that the engines of Capt. Ericsson cannot succeed, seem ready at the same time to conclude that no engine whatever can be successful which proposes to derive its power from the same source. Experiment may prove this conclusion to be correct; but if so, it will have to be recorded as a fact in scientific history no less remarkable than true, that all those distinguished men whose labors in this branch of science have illustrated the present age, and have built up the entire theory of caloric as it is now received, have been completely in error in regard to one of their most important practical inferences, deduced from the careful study of years.

Thompson, Rankine, Joule and Regnault have all expressed the opinion that an air engine may be constructed which shall be

economically much superior to the steam engine.* One of these, Mr. Joule, has even published an outline of a plan which he would adopt in order to secure the advantages to be attained by the use of this motive power;† and he has shown, by the application of the convenient formula of Prof. Thompson, for determining what fraction of the heat expended is converted into power by thermo-dynamic engines, that an engine constructed on his plan would produce an amount of mechanical effect greater in the ratio of 6 to 5 than a condensing steam engine with fourteen atmospheres of pressure in the boiler, and in which the steam expands itself in the cylinder to the temperature of 80°. Compared with steam expanding in like manner from the pressure of five atmospheres, the economy would be nearly as 3 to 2; and taking the steam at the original pressure of two atmospheres it would be almost as 2 to 1 (1 : 1·8). But in these computations, the expansion of steam *in the cylinder* is presumed to carry the temperature down to 80°, a condition never fulfilled in practice in any steam engine. Comparing this proposed engine of Mr. Joule with a steam engine in which the expansion in the cylinder is from five atmospheres to one, as in the ordinary non-condensing engines, the result would be favorable to the air engine as 3½ to 1. And if we compare it with a non-condensing engine at two atmospheres, we shall find that the air engine would be eight times and upward superior to the other in point of economy.

In all the discussions which the engines of Capt. Ericsson have provoked, certain propositions, which are rendered true by the peculiarities of those engines, have been tacitly assumed to be true universally; while, nevertheless, they express only the necessities of that particular case. Such are the following, viz:

1. That the available power of air engines is restricted within narrow limits by the resistance of the supply cylinders.

2. That the economy of heat in such engines is to be measured by the degree of thoroughness with which the regenerators do their work; and is therefore greatly affected by the incompleteness of the transfer of heat, both ways, between them and the air.

3. That as the first condition foregoing requires a pressure above that of the atmosphere to be carried up to the end of the stroke, there must, in the nature of things, be a great waste of heat, by the expansion of the emergent air.

* This is the important point. The air engine need not stand condemned because it will not (if it will not) drive a Collins liner. The distrust with which its prospects seem to be at present regarded, has grown mainly out of the attempt to make it accomplish labor to which it may possibly not be adapted. Air engines may always be too bulky and too ponderous to be well suited to rapid locomotion: but they may nevertheless be employed to great advantage in driving machinery or serving generally as stationary powers.

† Lond. and Ed. Phil. Mag., Jan., 1853; and Phil. Trans., 1852, Part I.

4. That a very large additional waste must occur through chimneys and flues, and by radiation from the cylinders.

5. That the leakage must be enormous, as compared with what occurs in engines driven by steam.

6. And finally, that air *cannot be heated* in such quantity, with such rapidity, and to such a degree as the exigencies of the case require.

Not one of these propositions is necessarily or universally true; and it will presently be seen that the plan of Mr. Joule is in defiance of all of them. On the other hand, it must be admitted that there are some disadvantages quite inseparable from all air engines of whatever description. It is certainly true,

1. That the mean effective pressure must, in the case of every such engine, be moderate compared with the absolute tension of the air in the cylinders.

2. That the mean effective pressure, if the engine is worked strictly with a view to economy, bears a less ratio to the absolute pressure, than it might do with less economical arrangements.

3. That a large apparatus is necessary for the development of high power, even though the power be obtained economically.

4. That the highest economy requires so large expansion of the air in the working cylinder, as to make the counteracting pressure in the supply cylinder a great inconvenience, though not an insuperable difficulty.

Professor Thompson, of Glasgow, has shown that the fractional portion of the heat which in any thermo-dynamic engine is converted into force, is represented by the range of temperature through which the elastic fluid is cooled by its expansion, divided by the maximum temperature as measured from the absolute zero (459° below the zero of Fahrenheit).* It follows that, in order to apply the force of heated air economically, there must either be large expansion in the working cylinder, or the heat remaining in the air at its discharge must be absorbed by some contrivance like Ericsson's regenerator, to be afterwards employed again. Such a contrivance, apart from its necessarily imperfect absorbing power, has the disadvantage of being confined in its usefulness within rather narrow limits. Of these limits, the upper one is the temperature required to maintain the rarefied air, at its discharge, at the pressure of the atmosphere; and the lower is the temperature produced by compression in the supply cylinders. As the pressure increases, these limits approach each other; and it is quite evident that when they meet, the regenerator becomes, not only practically but theoretically, useless.†

* As to the strict applicability of this formula to the air engine, see a note below.

† It is a curious fact that these two limits meet at precisely the pressure which (with a given supply cylinder) makes the power of the engine a maximum. For if (adopting the symbols introduced further on, and those of a former article) we

But, if we draw our supplies from the atmosphere directly, a very moderate pressure as compared with what is often employed with steam, will throw the regenerator entirely out of the question. Taking the supply cylinder at two-thirds the working cylinder, as in Ericsson's engines, supposing the external temperature to be 60°, and assuming that the tension of the air at the end of the stroke is, as economy requires, no higher than that of the atmosphere, the temperature at discharge is found by simple calculation to be 319°·5 F. Now air at 60° F. and density 1, will, on compression to density 2·692, be raised to this same temperature, and will have an elasticity of almost exactly four atmospheres. An Ericsson engine working with this pressure would derive no advantage from a regenerator. And it is furthermore evident that any approach to this limit must render the regenerator of no practical value. When, in addition to this, we take into consideration the imperfect manner in which this contrivance does its work, the degree to which its numerous meshes of wire must necessarily obstruct the passage of the air in passing into and out of the cylinder, and finally its failure to secure the advantage for which it appears to have been originally introduced, of absorbing the heat of the issuing air *above* that which is sufficient to put it in equilibrium with the surrounding atmosphere, thus making it impossible with due regard to economy, to avoid a great periodical preponderance of resistance over power, we are justified in concluding that the regenerator is by no means an important appendage of the engine, and that, very possibly, it might be advantageously dispensed with altogether.

If, however, the supplies are drawn, not from the atmosphere, but from a reservoir containing air already under a higher than atmospheric pressure; and are returned, when discharged from the cylinder, to the same reservoir, we may obtain a higher working pressure without extraordinary elevation of temperature by compression in the supply cylinder. This is the arrangement which Capt. Ericsson is understood to have adopted in his new engines. But the necessity of employing a refrigerating apparatus to keep the reservoir cool, or rather to cool down the escaping

make $\tau_{,}$ (the temperature of discharge) equal to τ' (that of compression), and observe that $\tau' = \tau'' l^{\gamma-1}$, and $\tau' = \tau\left(\frac{mn}{l}\right)^{\gamma-1} = \tau\left(\frac{\tau'' m}{\tau\, l}\right)^{\frac{\gamma-1}{\gamma}}$, we shall have the equation,

$$\tau'' l^{\gamma-1} = \tau\left(\frac{\tau'' m}{\tau\, l}\right)^{\frac{\gamma-1}{\gamma}};$$

Whence

$$l = \left(\frac{\tau}{\tau''} m^{\gamma-1}\right)^{\frac{1}{\gamma^2-1}}$$

which is the value of l, as shown further on, at the maximum of power. The regenerator, therefore becomes useless, when the engine is producing the greatest effect; but, by way of compensation, a materially larger proportion of the heat supplied, is converted into useful labor, than when a lower pressure is used.

air from the point at which it leaves the regenerator to its original temperature, is a great objection to such an arrangement in an engine designed for locomotion either on the water or on land.

If, on the other hand, we abandon the regenerator, we must look for our economy in enlarging the expansion in the working cylinder, and thus increasing the range of temperature through which the heated air descends in working. The disadvantage, already mentioned as accompanying this arrangement, of creating a great counteracting force, at the period at which the positive power is reduced nearly or quite to zero, must be met and disposed of by mechanical expedients. This appears to have been the view taken of the subject by Mr. Joule in the plan he has sketched of an air engine,* designed to fulfill the criterion of perfection, according to the formula (already mentioned) of Prof. Thompson. Mr. Joule's engine, like Capt. Ericsson's, employs a working and a supply cylinder, and also a receiver for the compressed air. But Mr. Joule proposes to apply the heat directly to the receiver,† and not to the cylinder; and he employs nothing equivalent to Ericsson's regenerator. The capacity of his supply cylinder is three-fourths, instead of two-thirds, that of the working cylinder. In other respects, the manner of working of the two engines is not dissimilar; but there is a great dissimilarity between them in regard to the pressure they are designed to carry, and the extent of expansion which it is proposed to give to the air.

The minimum expenditure of heat for the production of a given power in any such engine is presumed by Mr. Joule to be attained when the expansion in working brings the heated air into exact equilibrium of pressure with the atmosphere. This, it will presently appear, is not rigidly true, though nearly so. The temperature at which the air must be discharged, therefore, is according to him, definitely fixed by the dimensions of the two cylinders, when the atmospheric temperature is constant. Mr. Joule assumes the temperature of the atmosphere to be 50° F. For convenience, let us measure temperatures from the absolute zero (459° below 0° F.); then this temperature will be 509°, which we will represent by τ.

The general equation,‡

$$p'=p\left(\frac{\tau+\theta}{\tau}\right)\varrho,$$

* Lond. and Ed. Phil. Mag., Jan., 1853. In the Phil. Trans., Part I, 1852, Mr. Joule enters into some very elaborate calculations in regard to the power of air engines under various suppositions, but the plan given in the Magazine is more practical than anything contained in the previous article.

† He also proposes another and more economical mode of heating, which is noticed further on.

‡ In this equation, p is the pressure at the absolute temperature τ, and density 1; while p' is the pressure at temperature $\tau+\theta$, and density ϱ. The density at discharge (i. e. ϱ) must necessarily be to the original density in the inverse ratio of the cylinders.

making $p'=p$, and $\varrho=\frac{3}{4}$, gives $\theta=\frac{1}{3}\tau$, which is the amount of heat to be added to τ to expand the air at the constant atmospheric pressure from density 1 and temperature 509° to density $\frac{3}{4}$. The air at discharge, therefore, will be $\frac{509^\circ}{3}=169\frac{2}{3}^\circ$ above 50° F., or at $219\frac{2}{3}^\circ$ F.—equivalent to an absolute temperature of $678\frac{2}{3}^\circ$, which we will represent by $\tau_{,}$.

Mr. Joule now proposes that the air shall be compressed in the supply cylinder to one-fourth its original bulk, which will make its temperature 898°·59 (=439°·59 F.); and that the air so compressed may be expanded to the relative density $\frac{3}{4}$, it must be further heated $\frac{898^\circ\cdot59}{3}=299^\circ\cdot53$, making the total maximum temperature =1198°·12 (=739°·12 F.) which we will represent by τ''. The heat of compression, 898°·59, may be put $=\tau'$.

By an application of the formula given on page 248 of the last volume of this Journal, we find the mean effective pressure of the engine working under these conditions to be 9·891 lbs. to the square inch. The absolute pressure will be, in the mean time, 105·92 lbs.

It may be objected that Mr. Joule proposes to carry his heat extravagantly high. All that can at present be said as to this point is, that the difficulty with regard to a high temperature has hitherto been to reach it. It has not yet been ascertained what degree of temperature would be seriously injurious to the materials of the machine. The great liability to loss of heat by radiation at these high temperatures, is a more serious consideration.

But Mr. Joule has undoubtedly placed his minimum temperature too low. Sixty degrees of Fahrenheit, which is the usual temperature to which scientific determinations are referred, is quite as low as we are justified in assuming the ordinary atmospheric temperature to be, in our estimates regarding a machine of this description. For though the mean temperature of the year may in northern latitudes be lower, yet as the engine is expected to work in all seasons, we ought rather to assume a temperature above the mean than below.

If we adopt 60° as the atmospheric temperature, we shall have

$\tau'=915^\circ\cdot5$ (=456°·5 F.)
$\tau''=1220^\circ\cdot7$ (=761°·7 F.)
$\tau_{,}=692^\circ$ (=235° F.)
P (mean pressure)=9·846 lbs. to the sq. inch.
p (absolute press.)=105·92 " " " as before.

The conditions here supposed are highly favorable to economy of heat, and do actually convert into useful labor, as will presently be shown, forty-three and a third per cent. of the caloric imparted to the air from the furnaces. They are not however alto-

gether the most favorable to the production of power, nor absolutely the best as it respects economy. Moreover they involve an absolute pressure greater than is required for the attainment of a higher mean effective pressure, and therefore a larger amount of useful labor.

By differentiating the formula already referred to, on page 248 of the last volume of this Journal,* in reference to m and l successively, we shall find that neither the ratio of capacities of the two cylinders, nor the degree of compression of the air as assumed by Mr. Joule, is that which is most favorable to power under the given working temperature.

If we make m variable, the maximum power occurs when

$$m = \frac{\tau l}{\tau''}\left(\frac{(\tau+\tau'')\gamma - \tau'' l^{\gamma-1}}{(2\gamma-1)\tau}\right)^{\frac{\gamma}{\gamma-1}}.$$

For a wide range of working temperatures and for a great variety of values of l, m falls but little below unity. It seems to increase with τ'', and to diminish as l diminishes: but it is only in extreme cases that it becomes less than ·90, while it is usually above ·95, and is sometimes higher than 1·00. In the present case, with $\tau''=1220^\circ{\cdot}7$ and $l=\cdot 25$, it is ·907, and the dependent variables will be as follows:

$\tau' = 967^\circ{\cdot}66$ $(=508^\circ{\cdot}66$ F.)
P $= 10{\cdot}447$ lbs. to the square inch.
$p = 128$ " " " "

To compare the working under these conditions with the former, in point of economy, we have to observe that the expenditure of heat is directly as $m(\tau''-\tau')$, and the useful effect, directly as P. The economy will therefore be as $\frac{P}{m(\tau''-\tau')}$. The values of this expression for the two cases are as the numbers 430 and 456, nearly; or as 1 : 1·057, the condition of maximum power relatively to m, being nearly 6 per cent., the most favorable to economy.

By differentiating with respect to l, we shall find that a maximum occurs when

* This formula is repeated and reduced to a more concise form upon page 481 of the last number of the Journal. It may be rendered still more convenient for use, by substituting for the symbol k (in the last form given) its equivalent $\frac{\tau''}{\tau}$. It then becomes

$$P = \Pi a\left[\frac{m}{\gamma-1}\left(\gamma\left(\frac{\tau+\tau''}{\tau} - \left(\frac{\tau'' m}{\tau\, l}\right)^{\frac{\gamma-1}{\gamma}}\right) - \frac{\tau''}{\tau} l^{\gamma-1}\right) - 1\right]$$

in which Π is the pressure against which the engine works, a is the area of the working piston, m is the ratio of the supply cylinder to the working cylinder in cross section, l is the fractional part of the stroke before cut-off, and τ and τ'' have the values assigned them in the text.

$$l=\left(\frac{\tau\, m^{\gamma-1}}{\tau''}\right)^{\frac{1}{\gamma 2-1}}.$$

And, in the present case, τ'' being $=1220°\cdot7$, and $m=\cdot75$, l will have the value ·37309 instead of ·25; and the dependent variables will be,

$\tau'=815°$ (356° F.)
P=10·823 lbs. to the square inch.
$p=70\cdot92$ " " " "

Comparing this arrangement with the first in regard to economy, we find it to be inferior, in the ratio of ·8264 to 1; but it has the advantage of requiring only about two-thirds as much absolute pressure in the cylinder.

Through the formulas above given, we find by a tentative process, that the absolute maximum of power under this temperature occurs, when the value of m is about 1·094, and that of l, ·4363. We have, under these conditions,

$\tau'=869°$ (410° F.)
P=12·069 lbs. to the square inch.
$p=88\cdot52$ " " " "

And this arrangement compares with the first, in point of economy, as ·7285 to 1. Thus the increase of power is accompanied by a more than proportional increase of expenditure, although the absolute pressure is still comparatively moderate.

The relative economy of these several arrangements, as compared with steam expanding from five or three atmospheres to one, is exhibited at one view in the following. The temperature of steam at 5 atmospheres is, according to the French academicians, 308°·8; and Prof. Thompson's formula for the per-centage of heat converted into useful labor gives,*

* The formula of Prof. Thompson is the following. Let H be the mechanical equivalent of the total amount of caloric entering any thermo-dynamic machine; let τ'' be the temperature of the elastic fluid at entering the machine, and $\tau_{,}$ the same at leaving it; then $\tau''-\tau_{,}$ is the range of depression of temperature in working; and, putting W to represent the total useful effect, it will generally be true that

$$W=H\,\frac{\tau''-\tau_{,}}{\tau''}.$$

The formula of Prof. Thompson is applicable, however, only when the conditions conform to the definition of a *perfect* thermo-dynamic engine, as laid down by Carnot, and adopted by Thompson and Clausius:—that is to say, an engine which, by reversal, or working backward, is capable of producing the same amount of heat as is consumed in its direct action. That the air engine may fulfill this condition, the following formula must be true, viz:

$$\tau'':\tau_{,}::\tau':\tau,\ \text{or}\ \frac{\tau\tau''}{\tau_{,}\tau'}=1.$$

The formula which, in the most general manner, expresses the amount of heat utilized, when H represents the entire amount drawn from the source, is the following:—

$$H\left[\frac{\gamma-1}{\gamma}\left(1-\frac{\tau}{\tau'}\,\frac{\tau''\left(\frac{\tau\tau''}{\tau_{,}\tau'}\right)^{\frac{1}{\gamma-1}}-\tau'}{\tau''-\tau'}\right)+\frac{1}{\gamma}\left(1-\frac{\tau_{,}-\tau}{\tau''-\tau'}\right)\right].$$

$$H\frac{\tau''-\tau_{\prime}}{\tau''}=H\frac{767\cdot8-671}{767\cdot8}=\cdot1267\times H,$$

as the measure of the economy of working with steam under those conditions. In like manner we obtain $\cdot0861\times H$, for steam of three atmospheres. And Joule's engine gives

$$H\frac{1220\cdot7-915\cdot5}{1220\cdot7}=\cdot433\times H.$$

By properly combining these numbers with the foregoing, we obtain these ratios, steam being in both cases taken as unity.

	Steam.	Joule.	Max. *m*.	Max. *l*.	Max. *m* & *l*.
5 atmospheres	= 1	3·436	3·631	2·839	2·502
3 atmospheres	= 1	5·031	5·321	4·153	3·665

There can therefore be no doubt of the very great economy which would attend the working of a hot-air engine, should successful ingenuity overcome the difficulties (entirely mechanical) that have hitherto, in a great measure, baffled the efforts of inventors.

I am willing to admit that the working temperature assumed by Mr. Joule is too high; or is at least higher than is to be desired: and I may add that it is higher than is necessary. But before examining what would be the effect of reducing this heat within limits to which no one could reasonably object, it may be well enough to examine one or two of the difficulties which have been mentioned earlier in this article, as having been esteemed inseparable from the use of this motive power, and insurmountable. Mr. Joule has provided against losses of heat by escape through chimneys, &c., by proposing to make the furnaces air-tight, and to pass the air through them in its way from the reservoir to the cylinder. The blast may be directed in part through the fuel; but as the entire mass of air might create unnecessarily powerful combustion if driven through the fire, the greater portion may pass over it.* It may be added that, as the difficulty hitherto has

which on supposition that the foregoing proportion holds, becomes,

$$H\left[\frac{\gamma-1}{\gamma}\left(1-\frac{\tau}{\tau'}\right)+\frac{1}{\gamma}\left(1-\frac{\tau}{\tau'}\right)\right]=H\left(\frac{\tau'-\tau}{\tau'}\right)=H\left(\frac{\tau''-\tau_{\prime}}{\tau''}\right),$$

which is Thompson's formula. The demonstration of this proposition is reserved for another place.

* By this arrangement the oxygen of the air is shortly converted into carbonic acid, but without change of volume, although with some small increase of specific heat. Should the plan be adopted of employing confined air previously compressed, some fresh air will require to be regularly supplied, to sustain the combustion. This may be introduced beneath the fuel, while the rest passes over it. The amount necessary may easily be computed. According to Dr. Prout, 100 cubic inches of air at the normal pressure and density weigh 31·0117 grs.; of which weight 23 per cent. is oxygen. According to Mr. Joule, one grain of coal produces by its combustion heat enough to raise the temperature of a pound of water 1°·634 F. According to Regnault, the specific heat of air at constant pressure is (taking a mean of his values) = ·23775. And, the chemical equivalents of carbon and oxygen being 6 and 8 respectively, the composition of carbonic acid, CO_2, requires that eight grains of oxygen be supplied for the perfect combustion of three grains of carbon. From these

been to heat the air with *any* power of furnace, since radiant heat produces no sensible impression on it, since there is little time for the process of convection, and since but a limited portion of it can come in contact with the surface of the heating chambers, it would be advantageous to fill the fire boxes above the fuel with meshes of wire similar to those of Ericsson's regenerators. These would arrest the radiant heat and impart it by contact to the passing air. It is Mr. Joule's plan to construct the fire boxes so that they may be shut off from communication with other parts of the engine, when they require replenishing with fuel.

The exterior of the cylinders may be protected in a good degree by non-conducting jackets not unlike those applied by Watt to the steam engine.

These arrangements not only afford a good security against waste of heat through chimneys, and by radiation, but they put the fire most completely under the command of the engineer, and they secure what has not yet been secured, the certainty that the air *will be heated* to the temperature desired.

There remains no serious objection to be considered except that which arises from the resistance of the supply cylinders. This difficulty is entirely of a mechanical nature, and it is to be surmounted by mechanical expedients. There is abundant force, but this force happens to be greatly in excess at one time, and in deficiency at another. I have already pointed out, in a former number of this Journal, in what manner the difficulty may be obviated in engines designed, like Ericsson's, to carry not more than two atmospheres of pressure. That method will answer, with a supply cylinder of three-fourths the capacity of the working cylinder, to an extent of more than four atmospheres, preserving a predominance of positive power at every point of the revolution. By attaching three piston rods to the same crank, the cylinders being placed at angles of 60° from each other, the method will answer for still higher pressures. But that other and still more

data it appears that the oxygen contained in 13 cubic feet, or one pound of air will, by the combustion of a due amount of carbon, raise the temperature of one pound of air 4152° F. A reference to the summaries presented further on in the text will show that the arrangements theoretically the most eligible, do not require a larger range of furnace heating than about 300°; while some demand no more than 200°, and while those which are likely to be most convenient in practice will make necessary only about 250°. One pound of air will therefore furnish oxygen enough to heat about fourteen pounds, or more than 180 cubic feet of air through the largest required range. It is consequently quite safe to say that one pound of fresh air introduced at each stroke would heat at least ten pounds to the necessary extent, besides heating itself and making good all waste. If three cylinders, therefore, work in connection with one crank, each cylinder capable of heating ten pounds of air of the ordinary temperature and density, they will hold under the pressure of five atmospheres, fifty pounds each, at the same temperature. If two of these cylinders be supplied from a reservoir into which air has been compressed to this degree, the third one, working against the atmosphere alone, will furnish oxygen enough to keep up the combustion. If the compression in the reservoir is less, the supply of oxygen will be proportionably in excess.

effectual methods of removing the evil may be devised by ingenious mechanics, I have no doubt. Upon this point I shall presently have something more to say.

As it is perhaps too much to expect that the air engine will ever be successfully employed to propel the largest ships, on account of its weight and bulk, it is obvious that the effective pressures which we have already seen are considerable, may be materially increased by resorting to the expedient which Capt. Ericsson is said to have adopted in his engines now in process of construction, of employing air which has been previously condensed in a suitable chamber to such a pressure as may be thought expedient. A double density only, given to such a confined mass, might furnish to the engine a mean effective pressure of more than twenty pounds; whereas not more than three or four at the outside are attainable in the engines of the "Ericsson," and only between two or three appear to have been attained.

It follows of course that engines worked on the principle of Mr. Joule's will require no such enormous cylinders as those of the experimental engines which Capt. Ericsson has abandoned; and also that they will be double acting. These modifications will effectually dispose of the trouble of leakage, which has been probably occasioned, as suggested by my brother, Maj. Barnard, in the July number of Appleton's Magazine, by the unequal expansion of the top and bottom of the working cylinders.*

I have observed that the working temperature assumed by Mr. Joule is higher than is necessary. It remains to consider what

* Still, I cannot but think that the leakage has been over-estimated, and that the lack of power which has been to such a degree imputed to this cause, has resulted from the impossibility of heating the air by the contrivances employed in the abandoned engines. The idea that "the regenerators were to be the principal source of heat" led to the adoption of furnace arrangements manifestly inadequate to the great demand upon them.

Capt. Ericsson says that he was never able to carry more than eight pounds of pressure; yet Prof. Norton and others assume a possibility of a leakage of 20 per cent. Now the capacity of Ericsson's supply cylinder is 614·2 cubic feet, and one-fifth of this amount is 122·84 cubic feet, which, on this supposition, must escape at every stroke. At 10 revolutions a minute, this would be 40·95 cu. ft. per second. Now the velocity with which air under pressure escapes into the atmosphere, is represented by the expression

$$v'=v\sqrt{\frac{p'-p}{p'}}$$

in which v is the velocity with which the atmospheric air rushes into a vacuum ($=1299$ ft. per sec.), p' is the elastic force of the confined air, and p, that of the atmosphere. Whence we obtain in the case in hand, $v'=765.9$ ft. $=9191$ inches. And 40·95 cu. ft. contain 70761 cubic inches, which would require an aperture equal to $\frac{70761}{9191}=7·7$ sq. inches to admit of their escape in one second. Such a leakage as this is incredible. Maj. Barnard (Appleton's Mag. July, 1853,) reduces the supposed leakage to $\frac{1}{18}$, which is still 11·373 cu. in. per sec.—equal to the escape from a free aperture of 2·139 sq. inches. It appears to me that even this estimate is largely excessive. The "hole half an inch in diameter" which Capt. Ericsson permitted to remain open in the reservoir of one of his engines, was a trifle to this.

would be the effect upon the available power, of considerably reducing this. Still retaining the relative dimensions of the cylinders proposed by him, I will first assume a compression in the supply cylinder to one-third the original bulk. This will give an absolute temperature of 813°·8, (= 354°·8 F.) which will be required to be further raised to 1085.07 (= 626°07 F.). That this temperature is admissible if it can be reached, I have no doubt. By this arrangement we shall obtain a mean available pressure of 7·33 lbs. to the sq. inch, under an absolute pressure of 70·56 lbs., and with an economical superiority to the non-condensing steam engine of five atmospheres, of nearly three to one.

But in order to show that the economical advantages belonging to this method of employing air are to a great extent attainable under much lower temperatures, I will suppose that Mr. Joule's principle is applied to an engine working under the temperature upon which all my former computations were founded, viz., 450° above the temperature of melting ice, or 482° F. We find that, under this temperatue, when $\tau_{,}$ is made equal to τ', the mean pressure becomes 4·61 lbs., and the absolute pressure 43·22 lbs. to the sq. inch, while the economy, compared with steam, as above, is more than two to one.

In the following summary statements, are exhibited at one view all the particulars relating to the several cases under each of the three temperatures which have been considered. In these tables θ stands for the temperature, Fahrenheit, produced by compression; i. e., $\theta = \tau' - 459°$; R stands for the ratio of economy as compared with steam expanding from five atmospheres to one; and R′ represents a similar ratio, steam of three atmospheres expanding to one being taken as unity.*

$$\tau'' = 1220°07 = 761°7 \text{ F.}$$

	m	l	P	p	θ	R	R′
Joule,	·750	·250	9·85	105·92	456°·5	3·436	5·031
Max. m,	·907	·250	10·45	128·00	508°·7	3·631	5·321
Max. l,	·750	·373	10·82	70·92	356°·0	2·839	4·153
Abs. max,	1·094	·436	12·07	88·52	410°·0	2·502	3·665

* In order that a comparison may be made with Capt. Ericsson's abandoned engines, in which he proposed to carry a pressure of 12 pounds (above the atmosphere) in his reservoir, while l was made $= \frac{3}{4}$, we observe, first, that these conditions require a working temperature of 592° F, which is nearly equal to the medium temperature proposed above. We shall have then for Ericsson's arrangement,

$$\tau'' = 1051° = 592° \text{ F.}$$

m	l	P	p	θ	R	R′
·667	·750	4·35	27	156°·6	1·195	1·750

These values of R and R′ suppose no regenerator to be used. The regenerators, if perfect in their action, would make R = 3·153, R′ = 4·167. Allowing that they fail to the extent of 30°, as stated by Capt. Ericsson, the values would be R = 2·668, R′ = 3·907.

$\tau'' = 1085^{\cdot}{}^{\circ}07 = 626^{\cdot}{}^{\circ}07$ F.

	m	l	P	p	θ	R	R′
Joule,	·750	·333	7·33	70·56	354°·8	2·877	4·212
Max. m,	·917	·333	7·87	86·26	403 ·7	3·087	4·530
Max. l,	·750	·420	7·63	55.98	301 ·8	2·507	3·670
Abs. max,	1·021	·478	8·76	67·00	342 ·6	2·412	3·539

$\tau'' = 941 = 482^{\circ}$ F.

Joule,	·750	·472	4·61	43·22	246°·7	2·086	3·054
Max. m,	·928	·472	5·14	53·49	291 ·9	2·325	3·404
Max. l,	·750	·488	4·65	42·00	241 ·0	2·054	3·007
Abs. max,	·960	·538	5·16	48·55	271 ·5	2·034	2·977

It appears from these summaries, that the arrangement most favorable to economy is that in which, the cut-off remaining where Mr. Joule's principle would fix it for the given supply cylinder, this same supply cylinder is enlarged to the dimensions which give the maximum of power for that cut-off. It also appears that the conditions which furnish the *absolute* maximum of power for the *temperature*, are the least economical of those which the table embraces.

Two double action cylinders fixed at right angles to each other, with their piston rods attached to the same crank, will, with the arrangements proposed in my September article, preserve a preponderance of positive power throughout the stroke, whichever of the above proportions are adopted under the temperature of 482° F.

For the higher temperatures, three cylinders, or four, would be necessary; unless some new mechanical expedient should be resorted to, to reduce the resistance at the end of the stroke. Such an expedient it was a part of the design of the present article to propose—not as the best possible, for it has not been a subject of much study—but merely for the sake of illustrating the practicability of overcoming what has been regarded as a formidable difficulty in the way of the success of the air engine. It would extend this article, however, beyond reasonable limits, to attempt a particular description of the contrivance here. It may be sufficient to say that it is a contrivance for causing the piston at the period of maximum power to encounter and overcome the maximum resistance:—that is to say, to throw the burthen of driving into the reservoir a charge of air fully condensed by a previous stroke, upon the first third of each stroke, (or first fourth, if Mr. Joule's proposed temperature be adopted,) and to leave to the last two-thirds, only the labor of bringing a new charge to the required density.* By this means, the great resistance which,

*It is obvious that this might be effected, by so adjusting the cranks that the driving piston may begin its stroke just as the air in the supply-cylinder reaches the maximum of condensation. It is desirable, however, to avoid the severe strain upon the shaft which this plan would occasion: and hence the method hinted at in the text is made to rest upon a different principle.

under the ordinary arrangements, extends over all that period when the driving power is approaching zero, is easily disposed of, and the remaining resistance only becomes serious when the companion piston possesses a powerful mechanical advantage over it. The mechanism which I would propose is simple and liable to no objection at present discoverable; yet I have no doubt that other contrivances might be devised to accomplish the same end as well or better. I shall consider, therefore, the resistance of the supply cylinder as no longer entitled to very grave consideration; and, if it were, a heavy fly wheel would be a sufficient regulator for stationary engines of moderate power. For as the engine may be *started* with a cut-off which produces *no* counter resistance, there can be no difficulty in putting the regulator in motion.

It is sufficiently obvious that, for all the ordinary purposes to which stationary engines are applied—that is, for all purposes in which immense power within limited bulk is not an indispensable condition—the air-engine may take the place of steam with an economical advantage of at least two, and probably three, to one. It remains only to examine what dimensions would be necessary to create a power equal to that employed to propel the great ocean steamers.

There can be no doubt that the effectual modes of heating which may be made to supersede the very imperfect arrangements of the "Ericsson," will render possible a material increase in the velocity of the piston. That at least twelve revolutions may be obtained with an eight foot stroke is a moderate estimate. Under the lowest of the foregoing assumed working temperatures, (482° F.) a mean pressure of more than five pounds is obtained; but we will assume one lower than the lowest, that is, 4½ lbs. per sq. inch. Now one double-action six foot cylinder with a stroke of eight feet, twelve revolutions, and a mean pressure of four and a half pounds, will give an aggregate horse power of 106¼. Let the air be drawn from a chamber in which it has been previously compressed to the density of four times that of the atmosphere, and the power will be 425. Two such cylinders will give 850 aggregate horse power. A third one working in connection with them against the pressure of the atmosphere alone, in order to feed the fires with oxygen, will raise the total to 956 horse power.* These three may more than answer to a single marine engine; and a second similar set will give an entire aggregate of 1912 horse power. The whole six, with their accompanying supply cylinders would not exceed in weight, if they would equal, the four large pairs of cylinders on the "Ericsson."

* If the air be heated without coming into actual contact with the fuel, the three cylinders may all be supplied from the reservoir, and by making them only five feet each in diameter they will give an aggregate horse power of 855. This doubled will furnish more than the average power of the large ocean steamers.

If however, we adopt the higher temperature of 626° F. to which there is as yet no well established objection, we may obtain an equal power with much smaller cylinders. The tables show that it is practicable to obtain nearly 9 pounds of available mean pressure under this temperature, when working only against the atmosphere; and this moreover with an economy which, though not absolutely the highest, is considerably higher than any that is attainable under a lower temperature, and nearly two and a half times superior to that of steam of five atmospheres. Now if we compress air into the air chamber to the extent of five atmospheres, and work two cylinders of each set against this pressure, while the third works against the atmosphere alone, we shall require no larger diameter than 45 inches for each of our cylinders to enable us to obtain 1750 horse power, which is equal to that of a first class steamer. If then we increase the diameter to 48 inches, we shall have more than 2000 aggregate horse power, which enables us to allow nearly a sixth part for friction and other drawbacks.* The weight of the cylinders themselves will no longer be an objection. That of the air-chamber, heaters and refrigerators may be a more serious matter; how far it will be so will soon be settled by the experiments of Capt. Ericsson. It may be remarked, however, that the apparatus must be a heavy one, indeed, which will materially outweigh the boilers of the ocean steamers.†

In making these estimates of power, I have not overlooked the manner in which the velocity of the piston is controlled by the increased or diminished resistance, as pointed out by Maj. Barnard in Appleton's Magazine for October. It will be found, upon the principles of estimate adopted by him, that too low rather than too high a velocity of piston is here assumed. I admit the conclusiveness of all that Maj. Barnard has said in regard to the insufficiency of the original Ericsson engines to perform the task

* If again all these cylinders work against the reservoir, 38 inches will be as large a diameter as is necessary, and 40 inches will afford nearly 200 horse power surplus.

† All the estimates of power made in this paper have been founded on suppositions of pressure to which no serious objection can be taken. But as the power is always directly as the pressure in the air chamber, there is plainly no limit to its increase except the strength of materials. The question of bulk or compactness is also dependent upon the same considerations. With a pressure of 20 atmospheres in the air chamber, a single twenty-inch cylinder (the other suppositions remaining as in the text) would give 300 horse power; and a forty-inch, 1200. Two thirty-six inch cylinders would give 1950 horse power; *more* than enough to drive the largest ocean-liner, after making every deduction for waste and unproductive expenditure of power. Two six-foot cylinders would furnish equal power with but five atmospheres of pressure in the air-chamber. But the cylinders themselves would have to endure a pressure of more than twenty atmospheres. These are the dimensions adopted by Capt. Ericsson for his working cylinders in his new engines; but as the supply-cylinders are less than our supposition, and the compression which the air undergoes in them also less, (as it must be so long as it is proposed to make the regenerators of any use,) the power which these engines will develope must be correspondingly inferior. They are considered and compared with the abandoned engines, in a note published in the last number of this Journal.

imposed upon them. The thoroughness with which he has examined that part of the subject leaves nothing further to be said. But nothing which he or any one else has asserted or proved in regard to those engines in any manner invalidates the truth of the following conclusions, viz :

1. That the elastic force of heated air is a force available for all the purposes for which stationary powers are required.

2. That it is an eminently economical source of power; being in this respect superior to steam, as usually employed, in the ratio of two or three to one.

3. That while it appears to be at present more doubtful to what extent it is applicable, if at all, to the purposes of ocean navigation, its value in that respect remains yet to be experimentally settled.

University of Alabama, Nov. 2, 1853.

Art. XIII.—*Researches on Globuliferous Rocks;* by M. Delesse, Ingenieur des Mines, Professeur Honoraire de Geologie à la Faculté de Besançon.

[This important paper by M. Delesse, is published in the Transactions of the Geological Society of France, and is one among the many contributions of its author to our knowledge of the structure of rocks and their minerals.* We give our readers an abstract of it, presenting the prominent facts and the author's conclusions. The article is illustrated by several elegant plates, only a few figures from which we have copied.—Eds.]

Under the name of Globuliferous Rocks, those rocks are designated which contain certain minerals disseminated through

* The following are the titles of some of the papers of M. Delesse on these subjects.

1. Note sur le Chrysotil des Vosges.—Ann. de la Soc. d'Emulation des Vosges, vi, 2nd Cahier, 1847.

2. Recherches sur les Verres provenant de la Fusion des Roches.—Bull. de la Soc. Geol. de France, [2], iv, 1380, 1847.

3. Notice Sur les Caractères de l'Arkose dans les Vosges.—Bib. Univ. de Geneve, March, 1848.

4. Procédé Mécanique pour déterminer la Composition Chemique des Roches.—Bib. Univ. de Geneve, July, 1848.

5. Observations sur la presence d'eau de Combinaison dans les Roches Feldspathiques.—Bull. de la Soc. Geol. de France, [2], vi, 393, 1849.

6. Sur le pouvoir Magnetique des Roches.—Ann. de Ch. et de Phys., 1849, xxv, 194, and Annales des Mines, [4], xiv, 81, 429, and xv ; xvi, 323, 1849.

7. Recherches sur le Porphyre Quartzifère.—Bull. de la Soc. Geol. de Fr. [2], vi, 629, 1849.

8. Sur le Porphyre Amygdaloide d'Oberstein.—Ann. des mines, [4], xvi, 511, 1849.

9. Recherches sur l'Euphotide.—Bull. Soc. Geol. de France, [2], vi, 547, 1849.

10. Sur la Constitution Minéralogique et Chemique des Roches des Vosges: Pegmatite avec tourmalines de St. Etienne.—Ann. des Mines, [4], xvi, 1849.

11. Sur la Constitution Minéralogique et Chemique de la Syenite du ballon d'Alsace, Mem. de la Soc. d'Emulation du Doubs. 1847.

12. Sur la Protogine des Alps.—Bull. de la Soc. Geol. de France, [2], vi, 230, 1849.

13. Serpentine des Vosges, Ann. des Mines, [4], xviii, 309, 1850.

them more or less thickly in globules.* These globules are generally feldspathic, and this memoir has especial reference to those of this kind, occurring in rocks that are rich in silica.

Granites are sometimes globuliferous, as those of Rappawiki, in Finland, which contain globules consisting of orthoclase, surrounded by oligoclase. But these researches relate particularly to porphyritic or compact rocks, especially eurite, pyromeride, trachyte, retinite, perlite, obsidian, and a large variety of porphyries. The pyromerides of Corsica and the Vosges, the porphyries of Esterel, of the country of Bade and Thuringia, the trachytes of Iceland, the retinites of Saxony, and the perlites and obsidians of Hungary, are taken as types of this structure.

The globules vary much in color, being either black, violet, green, brown, yellowish, reddish, gray or white; usually differing a little from the color of the paste. They are commonly harder than feldspar, when undecomposed, owing probably to the excess of silica; but in perlites they are less hard than feldspar, perhaps because the mineral is in a semi-vitreous state, and also, it may be, because opal penetrates them, as stated by Hausmann and Fuchs. The specific gravity is low, viz. 2·3–2·6; 2·3–2·4, in perlite. This is far less than for quartz, which has G. = 2·65. Before the blowpipe, they fuse less easily than feldspars, owing to the excess of silica and small proportion of alkalies.

Composition:—

	Si	Al	Fe	Mn	Ca	Mg	Na	K	ign.
1. Fr. Pyromeride, Wisenheim,	88·09	6·03	0·58	—	0·28	1·65	2·53		0·34 – 100, *Delesse.*
2. Fr. Perlite, Hlinick, Hung.,	79·12	12·00	2·45	—	—	1·10	3·58		1·76 – 100·01, *Fleinus.*
3. Fr. Trachyte, Baula,	74·38	13·78	1·94	1·19	0·85	0·58	3·57	2·63	2·08 – 101·00, *Forchh.*
4. Fr. Retinite, Saxony,	73·00	14·50	1·00	0·10	1·00	—	1·75		8·50 – 99·85, *Klaproth.*
5. Fr. Perlite, Saxony.	68·53	11·00	4·00	2·30	8·33	1·30	3·40		0·30 – 99·16, *Erdmann.*

The large excess of silica is the remarkable characteristic, and as a general rule in the pyromeride, the composition of the globules is the inverse of that of the enveloping rock.

Structure.—The globules have generally a well defined structure. In most of the pyromerides of Corsica and the Vosges,

14. Recherches sur l'Association des Mineraux dans les Roches qui ont un pouvoir magnétique élevé.—Bull. de la Soc. Geol. de France, [2] vii, 108, 1850.

15. Sur la Variolite de la Durance.—Ann. des Mines, [4], xvii, 116, 1850.

16. Recherches sur le Porphyre Rouge Antique et sur la Syénite Rose d'Egypte. Bull. Soc. Geol. de France, [2], vii, 484, 524, 1850.

17. Sur le Porphyre de Lessines et de Quenast (Belgique).—Bull. Soc Geol. de France, [2], vii, 310, 1850.

18. Sur la Constitution Minéralogique du Diorite, Kersantites.—Ann. des Mines, [4], xix, 149, 1851.

19. Sur la Constitution Minéralogique de la Calcaire Saccharoide du Gneiss. —Ann. des Mines, xx, 141, (1851.)

* This subject has already received some attention—see especially, von Buch, *Recueil de planches de Pétrifications remarquables*, fol.; R. C. von Leonhard, *Charakteristik der Felsarten*, p. 52; Al. Brongniart, *Essai sur les Orbicules siliceux*, Ann. Sci. Nat. [1], xxiii, 166; Naumann, *Lehrbuch der Geognosie*; Roth, *Die Kugelform im Mineralreiche und deren einfluss auf die Absonderungsgestalten der Gesteine.*

and in the porphyries of l'Esterel and Oppenau, the silicious and feldspathic parts differ in color: and the distinctions of the two are often brought out in decomposition, the silicious part resisting change, while the feldspar is kaolinised. Acids (especially hydrofluoric) develop the structure in a short time.

The globules may be either solid throughout, or they may contain interior cavities: the former I call *normal* globules: the latter *abnormal*.

Globules of both kinds are usually spherical or spheroidal, and have either a *radiated*, *concentric*, or *irregular*, structure; these three kinds of structure may occur in the same globule. They detatch themselves readily from the enclosing rock, and present usually an even surface. They undergo alteration less rapidly than the enclosing rock. These globules sometimes contain crystals of quartz or feldspar, which obviously do not concur to the formation of the globules, and which, therefore, are *independent crystals*.

The excess of silica in the feldspathic paste, is considered an excess of a solvent, as is admitted by M. Delafosse, for different silicates.

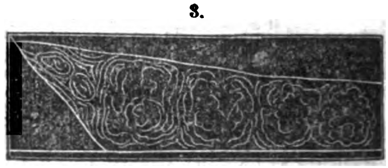
Normal globules may either contain quartz or be free from it.

Normal globules without quartz.—When globules of this kind have a crystalline structure, there are usually distinct cleavages. They are frequently observed in quartziferous, porphyrites, or eurites. Figure 1 represents one of these globules

1.

2.

3.



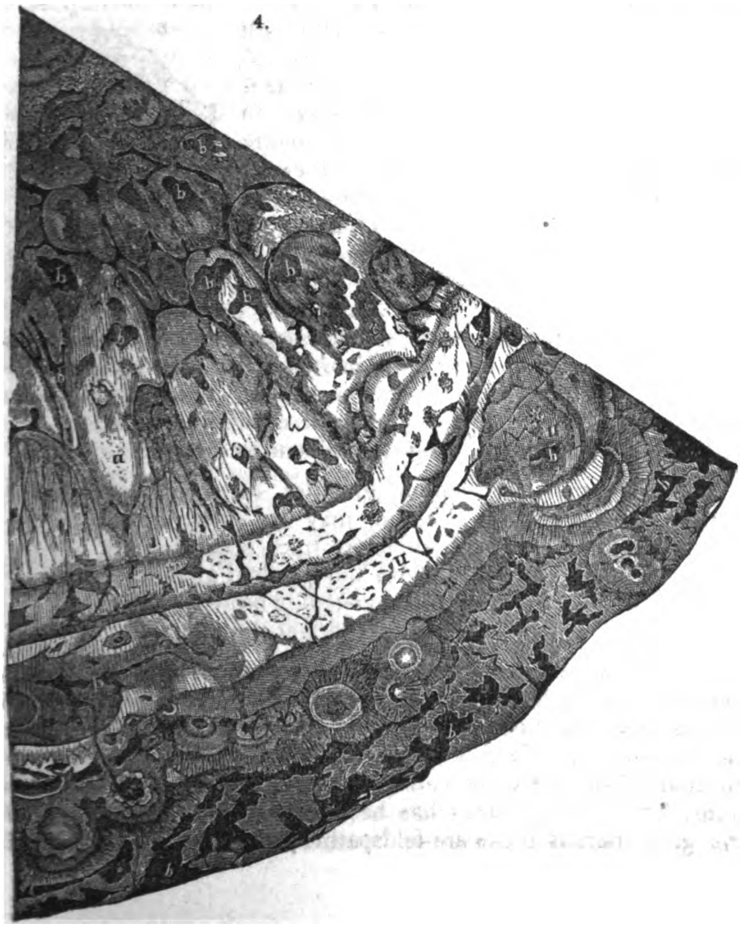
from the Eurite of Etival (Vosges), a rock consisting of orthoclase, a triclinic feldspar, a little quartz and mica; the globule approaches in form a crystal of orthoclase, and is cleavable and transparent, and it is surrounded by a thin milky or reddish compact zone of a feldspar which is probably triclinic. In certain micaceous eurites of the Vosges (Minettes), the globules are spherical and consist of feldspathic lamellæ, probably of orthoclase, irregularly aggregated, along with some mica, and they have a reddish exterior like the above. When the structure is radiated, there is but a single system of rays, diverging from the centre; the radiation is often very rude. At Oppenau, the feldspar forms but a small part of the globule, it being surrounded by chalcedony. Yet it is evident that the globule has resulted from the tendency of the feldspar to crystallize and to bring under the same influence, the silica.

The globules in obsidian are usually more or less radiated, and sometimes have an exterior coat or layer. They resemble much those found in the crucibles of a glass furnace when slowly cooled. Perlites and retinites, even when apparently compact, consist wholly of globules which may be distinguished when carefully examined; and the structure is very irregularly and sparingly concentric, with some cross fractures (fig. 3).

Normal globules with quartz.—Globules without quartz pass insensible into those with quartz. The latter appear to differ only in that the quartz has been imprisoned within, in consequence of the solidification of a crystalline crust. The structure is either radiated or concentric, and both kinds may occur together. The rays consist of feldspar needles or conoids separated by quartz; and both at the circumference and centre there are often zones of feldspar alternating with quartz.

The structure of the globules of the pyromeride of Corsica is shown in the following magnified view.

4.



There are irregular exterior zones of feldspar of different shades, *u;* needles or conoids of feldspar converging from the zones towards the centre, and others diverging from the centre outward, the two sets much interlaced; the forms and structure of these conoids are very irregular, and sometimes they become isolated globules. Quartz penetrates the whole, and is also to a considerable extent free and glassy (*b*), filling the spaces or forming zones. Sometimes the quartz is much more abundant at the centre than at the circumference. The quartz is beautifully brought out by means of hydrofluoric acid; but it should be observed that hyaline quartz is much less readily attacked than chalcedony or opaque quartz.

Abnormal globules.—These globules contain cavities, which may be regular or confused, filled or unfilled; and they have been produced either by *contraction* or *expansion.* Globules with cavities formed by contraction are common in the trachyte and other rocks of Iceland. One of these enlarged is shown in fig. 5. The cavities are of irregular form and surface. Sometimes they have been filled with quartz, and occasionally with oxyd of manganese, spathic iron, zeolites, barytes, calcite, etc. At Llanberis in Wales, the globules are sometimes a decimeter in diameter, and the cavities contain quartz crystals, and outside of the quartz a layer of ripidolite. Others in retinite contain one or more layers of chalcedony with sometimes an exterior zone of opal and an interior of quartz crystals, or a kind of jasper in layers or bands.

5.

In other globules the cavities are numerous and confused; and often they have been filled with quartz, which may be quite distinct from the feldspar or shade into it. In Corsica, the cavities sometimes contain specular iron with or without quartz; it is implanted in the quartz when this mineral is present.

The *independent* crystals of globules, occurring in the feldspathic paste constituting them, may be either quartz, orthoclase, a triclinic feldspar, hornblende, mica, specular iron, pyrites; and occasionally also, these form independent globules. In such globules the radiated structure is generally quite confused. Veins or seams of quartz also at times intersect the globules.

The most invariable characteristic of the rocks containing globules, is their holding an excess of silica; and in a given rock they are especially developed where the silica is most prevalent. By this presence of silica, connected in some cases with the penetration of the rock by veins of quartz, the occurrence or the formation of the globules has been determined. Moreover, all the globuliferous rocks are feldspathic; and the feldspar, which

would ordinarily have appeared as crystals, has taken the form of globules. The excess of silica has acted as an impurity in the mass, hindering the formation of regular crystals and leading to radiated and globular concretions. It may have acted also by causing too rapid a solidification for perfect crystallizations of the feldspar. Besides, there was a kind of repulsion between the feldspar and the excessively silicious paste, which was superior to the molecular crystallogenic forces tending to form crystals of the feldspar, and which by acting upon all sides of the agglomerations, reduced them to spheroidal globules.

In the globules of normal form in the pyromeride of Corsica,—which are characterised by exterior feldspathic layers, the outer very quartzose, the inner less so, and a semi-radiated structure within—it appears that the exterior was first consolidated, since these layers are not only very impure, but also support the convergent needles or conoids of feldspar within; the divergent needles from the centre fill up the spaces between the convergent ones, and arise from a solidification nearly cotemporaneous, beginning at the centre, where there is often a zoned feldspathic paste, as a nucleus, which is very siliceous. The solidification of the feldspar appears to have a repelling action on the silica, driving it either to the circumference or the centre. Thus, cooling from the circumference, and cooling from the centre, may occur together, or the former may alone characterize a globule, where its structure is made up of convergent needles alone. When consisting only of divergent needles, or when throughout zoned, it is considered probable that the solidification took place simultaneously throughout. The silica fills all the interstices between the feldspar needles and zones; it was solidified after the feldspar as in granite. The needles or conoids usually contain a quartz nucleus, so that they are analogous in structure to a globule; the solidification beginning at the surface, the excess of silica was driven to the interior of the conoid.

The abnormal globules, which owe the presence of cavities to *contraction*, cannot have derived these cavities from contraction after reaching the solid state. The cavities are too large to be thus accounted for by a cause which will explain only the occurrence of fissures. But in the liquid or pasty state, the circumstances are different, especially as a *process of desiccation* may have been in progress. Rocks of aqueous origin often contain concretions of concentric structure, which contain large cavities that have resulted from a desiccation of the nodule after the exterior was solidified. They may be argillaceous, silicious, calcareous, etc. Among the *argillaceous* are the *septaria* of the London clay; of the *silicious*, there are the brown zoned quartz of Egypt, which contain quartz crystals within, and also the concretions common in different deposits, especially the chalk, as the *klappersteine* or Achilleum resonans of the chalk of the Isle of Moen.

The *calcareous* include pisolites, the priapolites of Castres, the *kupstein* of the Loess, etc. There are also the *ferruginous*, consisting of argillaceous carbonate of iron, the Etites; also the *cupriferous*, such as the balls of carbonate of copper of Chessy.

The cavities of these concretions have been formed by contraction while the mass within was in a pasty state. Such cavities after they have been once formed are often filled by infiltrated material, as carbonate of lime, barytes, spathic iron, etc.

In igneous rocks, this process of contraction by desiccation may often be distinguished. In amygdaloids the cavities of the nodules of chalcedony containing crystallized quartz within, are often angular, as if formed by this means. In the Vicentine, the nodules contain angular cavities like those of the abnormal globules above described. Consequently water has performed a part in the formation of amygdaloidal nodules as well as in the formation of rocks of igneous origin. The silica of chalcedonic nodules was originally, as has been suggested, in a gelatinous state; and when owing to the dense condition of it, there was no contraction, the chalcedony entirely filled the cavity, either as a compact or a zoned mass.

But when the silica solution was very fluid, the chalcedony was left in the bottom of the cavity, as occurs at the Giant's Causeway, where the amygdala partly fill the cavities and have a nearly plane upper surface. Generally the solution was less liquid, though still as Breithaupt has observed, sufficiently so to undergo contraction on drying and afford the cavities within that are now filled with quartz crystals and other minerals. The regular form of the cavity may be owing to the compactness of the enclosing rock, allowing only of very slow drying. The zones in the chalcedony or quartz are a result of the process, showing perhaps intermissions in its progress, or in the molecular attractions in the material present affording the coloring of the layers.

In some cases the abnormal globules have probably been formed through *contraction by fusion*. It is important to observe that the *independent* crystals present in these globules, show no trace of the contraction which has been experienced by the abnormal globules; although often thin or delicate, they have not been in any way distorted, or broken. These facts authorize the conclusion that the crystals are subsequent in origin to the formation of the cavities. Hence the cavities must have been formed during the state of partial fusion, before the crystallization of these minerals took place.

From the characters of the cavities and their surfaces, it is inferred that they have probably resulted from the volatilization of water while the globules were still in a liquid state. It might be objected to this view that normal and abnormal globules often occur together. Still the presence of water is altogether proba-

ble, and we cannot conceive of any other volatile ingredient being concerned. It is hence altogether probable that these abnormal globules at first contained silica in the state of a hydrosilicate, or a jelly rich in silica; the water was at first retained by the pressure or the heat causing the spheroidal state; and afterwards it gradually escaped, and thus occasioned the contraction and the formation of the cavities.

In abnormal globules having cavities formed *by expansion*, these cavities have beyond doubt been produced by a disengagement of volatile substances, which has taken place while the rocks were still fluid. When the volatile substances were simply gaseous, they formed no deposit in the cellules; but when they carried along other substances, cooling would lead to a crystallization of different minerals on the walls of the cellules. These minerals are those composing the rocks, especially feldspar, quartz and mica: and in fact it has been found at Sangerhausen that feldspar may be deposited by sublimation in furnaces.*

In some cases, there has been an expansion by gaseous substances, producing an enveloping cellule, and afterwards a contraction forming a feldspathic globule at the centre of the cavity. In other cases, the expansion has taken place within the globule while the paste was still soft, producing a multitude of small cellules. These cellules have been afterwards filled by chalcedony.

Filling of the cavities.—The filling of the cavities has taken place either by secretion, infiltration or by both combined. In the filling of septaria, there may be a secretion of carbonate of lime or iron along the walls of the fissures, and also an infiltration filling up the rest of the cavities, the two processes producing deposits of different kinds or colors. In the filling of a mineral vein also, both processes have often operated, *secretion* introducing a part of the minerals, and *infiltration* another portion.

The infiltration of the silica into cavities, may have been either slow or rapid; the slow producing crystals, the more rapid forming chalcedony. Both processes, secretion and infiltration, may at different times have acted. Agates of the melaphyres have resulted from the penetration of the cavities with gelatinous silica, which is easily dissolved in hot water, when this water is under pressure and is charged with carbonic acid.

The same causes account for the threads or veins of quartz which intersect the globules.

The feldspar of the globules may be either orthoclase or a triclinic feldspar. The *abnormal* globules rather than the *normal*, are developed in globuliferous rocks, which are rich in soda. When orthoclase and a soda feldspar occur together, the latter is in

* Scacchi has recognized in a recent paper, (Rendiconto della R. Acad., Napoli, 1852,) that mica, quartz, garnet, hornblende, pyroxene, sodalite, nepheline and other silicates may form by sublimation.

the smaller crystals; and this inferior tendency to crystallization thus indicated, may be the reason for the formation in such rocks of *abnormal* globules which are less crystalline than normal globules.

In conclusion it may be observed that the theory here proposed is universal in its application to all globules or concretions, both those of sedimentary and igneous rocks.

One deduction of interest deserves to be noted. It is that as the vapor of water and volatile substances have acted an important part in the formation of globules, the theories for explaining the origin of granite and feldspathic rocks, ought necessarily to include this agency; for it follows that the feldspars, including orthoclase, may be formed even in the presence of water.

Art. XIV.—*Examination of some Deep Soundings from the Atlantic Ocean;* by Prof. J. W. Bailey, West Point, N. Y.

In an account of a microscopical examination of soundings made by the U. S. Coast Survey near the Atlantic coast of the United States* I made known that the soundings along the coast, from the depth of 51 fathoms S. E. of Montauk Point, to 90 fathoms S. E. of Cape Henlopen were chiefly made up of vast amounts of Foraminiferous shells, rivalling in abundance the deposits of analogous fossil species which I had proved to compose immense beds under the city of Charleston, S. C.

The facts were also mentioned that none of the species found in the soundings belong to the littoral genera of the group *Agathistegues* of D'Orbigny (*Plicatilia*, Ehr.) and that they also differed from those found in the tertiary deposits of Maryland and Virginia. These facts were confirmed and extended by the observations of F. de Pourtales in his Report to Prof. A. D. Bache, on the distribution of Foraminiferæ on the coast of New Jersey as shown by the off-shore soundings of the U. S. Coast Survey.†

In this paper Mr. Pourtales states that "the greatest depth from which specimens had been examined is two hundred and sixty-seven fathoms, and there the Globigerina are still living in immense numbers." He adds that the region of Globigerina extends to a depth not known.

I am indebted to that zealous cultivator of science, Lt. Maury of the National Observatory, for an opportunity to examine the deep sea soundings made by means of Brookes's lead on board the U. S. Dolphin by Lt. Berryman. These soundings proved to be of great interest and furnished results which have an important bearing upon Geology and Physical Geography.

* See Smithsonian Contributions to Knowledge, vol. ii, Art. 8.

† See Proceedings of American Association for the Advancement of Science, 1850, p. 84.

The soundings examined were as follows:

1080 fathoms,	Latitude	42° 04′ North,	Longitude	29° 00′ West,	July 25,	1853.
1360 "	"	44° 41′ "	"	24° 35′ "	" 18,	"
1580 "	"	49° 56′ 30″	"	13° 13′ 45″ "	Aug. 22,	"
1800 "	"	47° 38′ "	"	09° 08′ "	No date.	
2000 "	"	54° 17′ "	"	22° 33′ "	"	"

As these soundings are believed to be the deepest ever submitted to microscopic examination, and were obtained at localities far remote from those previously noticed, they were studied very carefully, and the following are the facts ascertained:

1. None of these soundings contain a particle of gravel, sand, or other recognizable unorganized mineral matter.

2. They all agree in being almost entirely made up of the calcareous shells of minute, or microscopic Foraminiferæ (*Polythalamia*, Ehr.), among which the species of *Globigerina* greatly predominate in all the specimens, while *Orbulina universa*, D'Orb., is in immense numbers in some of the soundings, and particularly abundant in that from 1800 fathoms.

3. They all contain a few species of non-parasitic or pelagic Diatoms, among which *Coscinodiscus lineatus*, *C. excentricus*, and *C. radiatus* of Ehrenberg, are much the most abundant.

4. They all contain a few siliceous skeletons of Polycistineæ, among which are several species of *Haliomma*, *Lithocampe*, &c.

5. They all contain spicules of sponges, and a few specimens of *Dictyocha fibula*, Ehr.

6. The above mentioned organic bodies constitute almost the entire mass of the soundings, being mingled only with a fine calcareous mud derived from the disintegration of the shells.

7. These soundings contain no species of Foraminifera belonging to the group of *Agathistegues* (*Plicatilia*, Ehr.), a group which appears to be confined to shallow waters, and which in the fossil state first appears in the tertiary, where it abounds.

8. These soundings agree with the deep soundings off the coast of the United States, in the presence and predominance of species of the genus *Globigerina*, and in the presence of the cosmopolite species *Orbulina universa*, D'Orb., but they contain no traces of the *Marginulina Bacheii*, B., *Textilaria Atlantica*, B., and other species characteristic of the soundings of the western Atlantic.

9. Examined by chromatic polarized light, the foraminiferous shells in these soundings showed beautiful colored crosses in their cells, and the mud accompanying them also became colored, showing that it is not an amorphous chemical precipitate. It in fact can be traced through fragments of various sizes, to the perfect shells of the Foraminiferæ.

10. In the vast amount of pelagic Foraminiferæ, and in the entire absence of sand, these soundings strikingly resemble the

chalk of England, as well as the calcareous marls of the Upper Missouri, and this would seem to indicate that these also were deep sea deposits. The cretaceous deposits of New Jersey present no resemblance to these soundings, and are doubtless littoral, as stated by Prof. H. D. Rogers (Proc. Bost. Soc. Nat. Hist. 1853, p. 297).*

11. The examination of a sounding 175 fathoms in depth, made in latitude 42° 53′ 30″ N., longitude 50° 05′ 45″ W. (near Bank of Newfoundland) by Lt. Berryman gave results singularly different from those above stated. It proved to be made up of quartzose sand, with a few particles of hornblende, and not a trace of any organic form could be detected in it. This exceptional result is important, as it proves that the distribution of the organic forms depends on something beside the depth of the water.

12. Connecting the results above mentioned with those furnished by the soundings made in the western portions of the Atlantic it appears that with the one exception above mentioned, the bottom of the North Atlantic Ocean, as far as examined, from the depth of about 60 fathoms, to that of more than two miles (2000 fathoms) is literally nothing but a mass of microscopic shells.

13. The examination of a large number of specimens of ocean water taken at different depths by Lt. Berryman at situations in close proximity to the places where the soundings were made, shows that even in the summer months when animal life is most abundant, neither the surface water, nor that of any depth collected, contained a trace of any *hard* shelled animalcules. The animals present, some of which are even now alive in the bottles, are all of a soft, perishable nature, leaving on their decay only a light flocculent matter, while the Foraminiferæ and Diatoms would have left their hard shells if they had been present.

As the species whose shells now compose the bottom of the Atlantic Ocean have not been found living in the surface waters, nor in shallow water along the shore, the question arises, Do they live on the bottom at the immense depths where they are found, or are they borne by submarine currents from their real habitat? Has the Gulf Stream any connection by means of its temperature or its current with their distribution? The determination of these and other important questions connected with this subject requires many additional observations to be made. It is hoped that the results already obtained will induce scientific commanders and travellers to spare no pains in collecting deep sea soundings. If such materials are sent either to Lt. Maury, U. S. Observatory, or to myself at West Point, N. Y., they will be thankfully received and carefully studied.

* See also this Journal, vol. xvii, p. 131.

Art. XV.—*On some New Localities of Fossil Diatomaceæ in California and Oregon;* by Prof. J. W. Bailey, West Point, New York.

Some interesting specimens of fossil Diatomaceæ from California and Oregon having come into my possession, I am induced to publish the following brief notices of them, in hopes to direct the attention of travellers in those regions to those remarkable deposits, and thus acquire more information concerning their position and extent.

1. The first specimen of fossil Diatomaceæ from California, I found among specimens of minerals collected two or three years ago in California by Washington Chilton, Esq., of New York. It was from *Suisun Bay*, 25 to 30 miles above St. Francisco, where Mr. Chilton says a large bed of similar material exists. It consists of a light white claylike substance made up entirely of fossil *marine* Diatoms, many species of which are identical with species occurring fossil in the tertiary diatomaceous deposits of Virginia and Maryland, while a number of the species found in these latter deposits do not occur in the California beds.

2. In a box of minerals collected in Oregon and California by Lt. Robt. Williamson, of the U. S. Topographical Engineers, I found four specimens of fossil diatomaceous earth, evidently from different localities, although unfortunately the precise locality is mentioned for but two of the specimens. I will designate them as specimens A, B, C, and D.

Specimen A.—This is a very light white substance, made up of the siliceous shells of *fluviatile* Diatoms. The predominant species are a small Gallionella, and a Discoplea mingled with a few species of Epithemia, Cocconema, Gomphonema and Spongiolites. This specimen was without a label, but is believed to be the specimen referred to in the following extract from a letter received from Lt. Williamson: "You will find some of the light white clay from *Pit River*, which I spoke of to you." This is, I believe, the same substance which has given rise to the newspaper accounts of cliffs in California composed of carbonate of magnesia.

Specimen B.—This is a light white chalky mass, whose locality is not given. It consists of fluviatile species, among which various species of *Biblarium* are quite abundant. The species of this genus have been found living in Siberia, and fossil in Oregon. Lt. Williamson's specimen resembles the Oregon mass found by the U. S. Exploring Expedition under Capt. Wilkes, but presents a different group of forms and therefore must be from a different locality.

Specimen C.—This is also a chalklike mass, whose precise locality is not marked. It is composed chiefly of a minute species of Gallionella, mingled with sieve-like discs which at first would be referred to the marine genus Coscinodiscus, but the entire absence of all other marine forms and the presence of several decidedly fluviatile species, makes me believe that the deposit is a fresh water one, and careful examination of these discs show that they are more nearly allied to the fresh water genus Stephanodiscus than to the marine Coscinodiscus.

Specimen D.—Is an ash-colored earth, marked as from near the Boiling Spring, Pit River. It is chiefly remarkable for containing a great number of Phytolitharia, or remains of the siliceous portions of plants, mingled however with numerous minute fluviatile Diatoms.

It is hoped that travellers in California and Oregon will keep a look out for specimens of light white clay like substances, and *carefully marking the locality at the time of collection*, send them to me for microscopic examination. Even a minute portion sent by mail will be very acceptable.

Art. XVI.—*Analysis of Beryl from Goshen, Massachusetts;* by Dr. J. W. Mallet.

This Beryl from Goshen is the variety formerly called Goshenite by Prof. C. U. Shepard, but which in his latest work, is referred to Beryl. The specimen examined was part of a broken six-sided prism, with rough faces of one inch or one and a half inches in diameter, having a very faint tinge of rose color. Sp. gr. = 2·813. It yielded on analysis,

Silica, - - - -	66·97
Alumina, - - - -	17·22
Glucina, - - - -	12·91
Peroxyd of iron, - - -	2·03
Oxyd of manganese, - -	*trace*
	99·13

which numbers are obviously those of Beryl.

Art. XVII.—*On the Silurian System of the Lake Superior Region;* by James Hall.*

Chazy, Birds-Eye, Black River, and Trenton Limestones.—These limestones are so intimately connected, one with another, in the Lake Superior district, and each is so thin, that no advantage can be derived from treating them separately. It is true, however, that each can be recognized as a distinct member of the lower Silurian series, and is characterized by fossils peculiar to itself, as has been shown in New York. Reduced as these formations are in thickness, it will, nevertheless, be necessary to study them separately, and for the geologist, or collector, to preserve the fossils distinct.

Commencing at the eastern limits of the district, these limestones are first seen upon the St. Mary's river; but they are better exposed upon the eastern side of St. Joseph's island, than upon the main land of the Michigan side. The sandstone, which is seen on Sugar island, plunges to the south, and passes beneath all these limestones, leaving, as far as observed, no trace of the calciferous; but an interval, covered by drift, occurs, where no rock is visible. In examining the shore of the island, the first rock seen, after the disappearance of the sandstone, is the Birds-eye limestone; but, further to the eastward, near a projecting point, some layers of the Chazy make their appearance, having, towards the bottom, an arenaceous character; while higher up, they assume an argillo-calcareous composition, and contain fossils characteristic of this member of the New York series. This limestone is also seen to pass directly beneath other beds, which, by their peculiar character, may be recognized as the Birds-eye. The fossils of the Chazy do not pass above the limits of the Birds-eye; but the respective limits of the two members are as well defined here as in any of their eastern localities.

The Birds-eye limestone is, for the most part, thin-bedded, the layers being separated by shaly matter, which rapidly wear away under the influence of the atmosphere and the water, while the harder parts are brittle and easily fractured. This limestone appears to be more fossiliferous here than in New York, and, in the upper layers particularly, we found a great number of Orthoceratites.

The Black-river limestone, or beds which may be regarded as the equivalent, seems to be intimately incorporated with the Birds-eye; so much so, that no line of demarcation could be detected.

* The following pages are cited from Chapters IX and X, of Messrs. Foster & Whitney's Report, on the Lake Superior Region, Part 2, and are in continuation of the part of the work from which we have cited on pages 11 to 33.

Further to the south, the Trenton limestone was observed extending in a low cliff, for some distance along the river, maintaining, to a great extent, the characters by which it is distinguished in more eastern localities. Its higher portions are made up, in a great degree, of crinoidal remains, and it preserves the same character as this portion of the series in the Mohawk and Black-river valleys. The whole mass is evidently much thinner than at any locality east of Lake Huron, and there is, also a larger proportion of shaly matter, not only between the layers, but incorporated in them.

In addition to the evidence derived from lithological characters, the fossil contents are of such a character, and so abundant, as to leave no doubt in this respect. The aspect not only of this member, but of the others, was such as to impress one with the belief that, though identical in age and in composition, and a continuation of their eastern equivalents, they were deposited under circumstances less favorable to organic life, resulting from the nature of the materials deposited, or from varying conditions in the ocean. The quantity of shaly materials mingled with this limestone, and distributed in layers between the beds, would seem to indicate that a shallow and turbid state of the water prevailed during its deposition.

The observations made by Messrs. Whittlesey and Desor, on the Manistee river, tend to confirm these views.

On the Escanāba river, for more than seventy miles along its meanders, above its mouth, these limestones are almost constantly exposed, and present these features. The river, for the distance of a mile before it enters the lake, flows over limestone strata, which are nearly horizontal, or dip very slightly in the direction of the current, but more gently than the descent of the water; consequently, the strata are cut through and their edges exposed, to the thickness of only a few feet. The first rock met with, in ascending the stream, is a tough limestone, in thin layers, separated by bands of shale. It has the lithological characters of the Birds-eye, and contains Orthoceratites and other fossils, characteristic of that member of the series, as well as of the Black-river limestone. The succeeding layers, which are well-exposed about a mile above this point, consist of thin, irregular, or wedge-shaped layers of light ash-colored limestone, verging to a dark-blue color, and contain many species of fossils, characteristic of the Trenton series; leaving no doubt of their identity. Many of the thin layers are composed of crinoidal remains, and the weathered surfaces are often completely studded with the detached joints or fragments of columns, standing in relief. This character of the weathered surfaces is unlike that exhibited in any part of this limestone noticed within the limits of New York; but an examination of these crinoidal fragments' shows that they belong to genera, and, perhaps, species, known in this series elsewhere.

The thin and even-bedded layers at the mouth of the river, may be quarried to a considerable extent, whenever there shall exist a demand for them, and they will form a durable building material. The succeeding layers are too irregular to be of any value, except for burning into lime.

At the lower mill-dam, a mile above the first exposure, the banks of the stream, as well as the base of a small island, consist of beds of limestone, which form an escarpment fifteen feet thick. These also belong to the Trenton group, as indicated by the following fossils, which were collected at this point: *Isotelus gigas*, *Calymene Blumenbachii*, var. *senaria*, *Orthis testudinaria*, *Leptæna sericea*, and *L. alternata*.

In ascending the river, this limestone, and even the identical beds, continue as far as the Lower falls, distant two miles from the upper mill-dam. The stream for the most part is very rapid, and the dip of the strata very nearly corresponds with the descent.

At the Lower falls, the thin and irregularly stratified portions are succeeded by regularly-bedded strata of limestone, attaining a thickness of fifteen or twenty feet. It is impure from an admixture of arenaceous and argillaceous matter, while layers of these materials occur beneath. A few fossils were found at this place, among which were Brachiopods of the Trenton and Hudson river groups, with *Chætetes lycoperdon*.

After passing the falls, these last described beds very soon disappear, and there occurs an interval of several miles, where few traces of rock are observed; but, at several points, the limestone is seen by the margin of the river,—the descent of which is here very gentle,—in no instance exposed to a greater thickness than two feet. The layers are thin, of a light-grey color, of a granular structure, and contain numerous cavities, some of which are partially lined with crystals. Although these few beds of themselves are of little importance; yet, when studied in connection with the range of the series, they present a feature of considerable interest, noticed here for the first time. These compact, grey beds lie above the limestone seen at the Lower falls, since they clearly pass beneath the channel of the river, before the others emerge. Between the Lower falls and the Meadow,* distant

* This is a somewhat remarkable feature in a river of this character, where the general descent is very rapid, being some seven or eight hundred feet in the distance of thirty miles. The principal meadow is found at a bend in the river, the ordinary channel just above it turning abruptly to the east, and making a broad curve, where it has cut through the drift; while to the west is another channel, through which a part of the water is discharged during freshets, uniting with the main channel half a mile below; thus leaving on the west side a large tract of bottom-land, the upper extremity of which consists of a wooded island. In looking from the eastern bank, a broad meadow bounds this temporary channel, flanked on the right by the wooded island, and on the left by the unbroken forest. In this meadow are several elms of magnificent size, the nearest ones standing out in bold relief, while the farthest blend with the forest growth of maples, beaches, &c. Their mode of growth shows that,

nine miles from the lake, the surface is mainly covered by an accumulation of boulders, apparently derived from the drift, which forms a steep escarpment on the eastern side of the river. Below this place, the river curves to the eastward, cutting deeply into an accumulation of this character; but, above, it again bends to the westward, and lays bare, on the east side, a ledge, some twelve feet thick, of grey limestone, thin-bedded, and containing small cavities, sometimes lined with crystals of magnesian carbonate of lime, and at others empty. These layers are a continuation of those before described as occurring at the water's edge, several miles below.

From this point, onward, for a considerable distance, the dip of the strata is more rapid than the descent of the stream, though the latter is quite rapid; consequently, there is a succession of the lower strata presented to view in its banks. I had, therefore, an opportunity of verifying my first observations, that the grey, granular limestone rested upon some shaly and arenaceous beds with thicker calcareous strata, like those seen at the Lower falls, succeeded by the irregularly stratified beds observed at the upper mill-dam. We had an opportunity of tracing these two last divisions for several miles along the river, where these escarpments were exposed from twenty-five to thirty feet in thickness, disclosing, in the ascending order, the Birds-eye limestone, succeeded by the Trenton, with scarcely a trace of the Black river limestone; for, while the *Ormoceras*, characteristic of the latter rock, occurs at the junction of the other two, in the same connection is found the *Orthoceras multicameratum*. Notwithstanding, therefore, the extreme tenuity of these different members, and the great admixture of arenaceous and shaly matter, there is no difficulty in recognizing, at this remote point, the important subdivisions which have been made in New York, and of determining them by their characteristic fessils.

From the first exposure of the rocks above the meadow, they are almost constantly in view to the Upper falls, and from thence, onward to the forks of the river. About two miles from the upper end of the meadow, the Birds-eye limestone is seen at the level of the river and continues, with some slight undulations, to occupy the surface, as far as the first rapids below the Upper falls. At Indian creek, a short distance below the foot of the rapids, the Birds-eye limestone is very distinctly defined, and rests upon some heavy beds, which clearly represent the Chazy limestone. One of these beds is remarkable for weathering in a peculiar manner, and an examination shows that it is filled with a kind of

from their commencement, which is very ancient—since two or three were observed which had fallen from age—they had stood in the open areas which they now occupy. In such wooded bottom-lands, in this latitude, we often find plants and trees flourishing luxuriantly, which, under ordinary circumstances, are found only in more southern situations.

tough, silicious and irregularly-shaped concretions, or segregations, which stand out in relief, while the calcareous part wastes away.

At the Upper falls, the strata rise more rapidly to the northward, and though the ascent of the stream is considerable, yet, on arriving at the foot, the calciferous sandstone is exposed, forming the base of the escarpment over which the water is precipitated; while, above it, there are two layers which represent the Chazy and Birds-eye limestones and the lower part of the Trenton group.

Here, the following section is exposed in an ascending order.

1. Birds-eye limestone, fine-grained and compact, of a bluish-drab color.

2. Calcareous layers, of a grey color, with crinoidal joints and other fossils,—4 feet.

3. A heavy-bedded, variegated limestone, with much silicious matter, the surface weathering very unevenly,—2 feet.

4. A thick, silico-calcareous bed, with fossils in the upper part, —2 feet.

5. Silico-calcareous layers at the foot of the falls, thin and even-bedded,—1 foot.

The whole exposure at the falls is about fifteen feet, and in the bank above, about ten feet more. The beds forming the top of the falls disappear below the river, near the point where Indian creek comes in from the east. The layers having the character of the Birds-eye limestone disappear a short distance below, and are succeeded by thin beds containing an abundance of *Orthis testudinaria*, *Leptæna sericea*, and *L. alternata*, with other fossils of the Trenton limestone, and have the same character as some of the layers about midway in this group, as developed in the Mohawk and Black river valleys, in New York. Above the falls, the low cliff of fossiliferous limestone continues for some distance, gradually declining to two or three feet in height above the river. The same changes take place, with the recurrence of the same layers, in ascending the stream, the strata rising more rapidly than the ascent of its bed. From this cause, we find the calciferous sandstone coming out from beneath the fossiliferous limestones, at a point some three miles above the falls, and continuing thence up the stream for the distance of two miles above its forks.

These limestones, which have a combined thickness of less than seventy-five feet, are exposed almost continuously in the bed and banks of the stream, for the distance of more than thirty miles. following its meanders, and a distance of about twenty miles in a direct line. The difference between the dip of the rock and the descent of the river, is less than sixty feet. There is, however, very little parallelism between the two; for, within

the first eight miles after leaving the lake, we meet with the highest beds of limestone in the series which occur on this river. Although its descent is rapid, yet it is unequal in its rate, and these inequalities appear to be due to undulations in the strata, which can be detected at several points. For the most part, the stream is very shallow, and its bed rocky.*

Along the whole extent of this exposure of the rocks, whether examined continuously, or at intervals, there is no difficulty in identifying different portions with their New York equivalents. When taken as a whole, and all of the beds examined in connection, the principal subdivisions, such as the Chazy, Birds-eye, and Trenton limestones, are readily identified, not only by their lithological characters, but by their organic remains. Even in the arenaceous layers, which form some twenty feet of the whole thickness, we not only detect numerous species of fossils peculiar to the Trenton limestone, but many peculiarities of bedding, and other characteristics, which, though not easily described, are readily understood and comprehended by the geologist.

It is deemed unnecessary to give farther details of the variable features of these limestones along the Escanāba river; in all the localities examined, they offer little of economical value, aside from their application to building purposes.

At the mouth of Rapid river and along its borders, and also at the mouth of the White-fish and the head of Little Bay des Noquets, the Trenton limestone is exposed, exhibiting the same lithological characters, as at the upper dam on the Escanāba.

Farther explorations made by Mr. Whitney, along the White-fish river, in crossing from Little Bay des Noquets to Lake Superior, proved the occurrence of the Trenton group, for the distance of fifteen miles, or more, from the head of the bay. The specimens procured are filled with fossils, principally *Leptæna sericea*, and occasionally with *L. alternata* and *Orthis testudinaria*, while, in lithological characters, they agree with those observed on the Escanāba and Manistee. The country along the White-fish river is low, rising little above the river margin; consequently, there are no cliffs, or escarpments, where the strata are exposed to any extent, and specimens can be procured only at the water's edge. This river then, like the Manistee, affords evidence of the existence of certain groups, but does not admit of continuous observation as to their succession, thickness, and importance.

* The name Escanāba signifies *Smooth Rock*, given for the reason that the stream often flows for considerable distances, over the smooth surfaces of the slightly inclined rocks. The inequalities caused by the offset of particular beds, give rise to numerous rapids, and render the navigation so difficult that even a small skiff cannot be impelled against the stream, when the water is low, except by using setting-poles armed with steel points.

In the present state of the northern peninsula, being almost an unbroken wilderness, and the elevation of the country occupied by the Silurian series but a few hundred feet above the lake, with no abrupt curves in the strata, by which they are brought to the surface, it is impossible to determine anything more than the limits of the more important groups: for all practical purposes, however, the determinations already made are sufficient; and it was only with the hope of acquiring more detailed information, that we desired more time and better opportunities of investigation.

Leaving the Escanāba, and following along the coast of the bay in a southeasterly direction, we soon find the limestones, just described, coming out to the surface at the lake-level and scarcely rising above it, the character of the beds being the same as those seen within a mile of the mouth of the river.

At the mouth of Ford river, the country is low and there are no rocks visible. The banks consist of alluvial, or drift materials, for four miles, when there occurs a long rapid over the thin-bedded limestone, the same in character as that at the upper mill-dam on the Escanāba. Proceeding towards Cedar river, we passed several reefs of rock and observed large slabs of limestone in the shallow water, which appeared identical with those at the mouth of the Escanāba, and lower than the thin-bedded portion of the Trenton group. Upon Cedar river, there are no rocks visible for two miles after leaving its mouth; but here, at the end of a mill-dam, the arenaceous and shaly bands, which occur at the Lower falls of the Escanāba and opposite "Wood's Camp," are visible. These occupy a higher position in the series than those seen at Ford river. Few fossils were here observed, crinoidal joints being the most numerous.

From the mouth of Cedar river to the mouth of the Menomonee, the more thickly-bedded portions of the Trenton, with layers of Birds-eye, occur along the margin of the bay. Although nearly in situ, the beds have been raised and broken by the water, and, in some places, piled up in walls, or barriers, which have a very artificial appearance.

At the lower dam, on the Menomonee, a little above the water, limestone is observed, in places, on the right bank, its surface being ground down, and grooved with drift-scratches. Its character is very similar to that of the Birds-eye, and the only fossils observed were crinoidal joints.

The west side of Green bay, from the mouth of the Menomonee to its head, was explored by Messrs. Whittlesey and Desor. The specimens collected showed a continuation of the same limestones which had been observed from Little Bay des Noquets to the Menomonee. The fossils collected were two species of *Murchisonia*, *Pleurotomaria lenticularis*, and several *Brachiopoda*.

After examining the eastern shore of Green Bay, I took up my observations upon the same limestones at Depere, tracing them along the Fox river to the outlet of Lake Winnebago. Subsequently, in order to connect the geology of the Lake Superior district with that of the Chippewa district, I continued my examinations across the state of Wisconsin to the Mississippi river, and thence, at intervals, along that river to the falls of St. Anthony. At Mineral Point, the lower members of the series have become very argillaceous, weathering into a light drab color, and are characterized by numerous fossils. At Plattsville, the Birds-eye layers are pretty well defined, though associated with much shaly matter and some layers of dark shale. The rock, on being freshly fractured, is very dark-colored, but weathers to an ashen hue. The Trenton limestone, which succeeds, is thin-bedded and light-colored, and all that remains is scarcely more than twenty-five feet in thickness. The same features are observed at Galena, and again at Dubuque. At the latter place, the connection of this group with the succeeding limestone is very obvious. In all these localities, the entire thickness of these lower limestones, which can clearly be identified with the Trenton and associated limestones of the east, is less than fifty feet; but it is possible that some better exposures would give a greater thickness.

I conceive that there can be no longer a doubt in reference to the age of the limestones under consideration. Their identity with those of New York and of Canada has been established, not only by a comparison of the fossils, but also by tracing almost continuously their range from the Mohawk, Champlain, and Black-river valleys, through Canada, to the eastern limits of this district, and thence westward, continuously, to the Mississippi river.

These remarks also apply to other groups, concerning which some difference of opinion has heretofore prevailed. Feeling the necessity of adopting some recognized standard, we have referred these subdivisions, so well marked, to those which have already been made by the New York geologists.

Before leaving the subject of these limestones, it will be necessary to recall to mind some observations made on the Escanāba river, above the Lower falls, and at the bend above the Meadow. The upper layers of grey limestone, at the last named point, have a thickness of some fifteen feet, are meagre in fossils, and do not appear to be identical with any portion of the series, which I have observed farther east. In going westward, I had not an opportunity of observing the overlying deposits of the Trenton limestone until I arrived in Wisconsin. Here, in numerous localities, as well as in Illinois and Iowa, the deposit above that which is marked by an abundance of fossils characteristic of the Trenton, is a grey, or drab-colored limestone, and very friable, forming

a part of the "cliff limestone" of the Ohio and Indiana reports, and is called by Dr. Owen, in his report on the Lead region, the "upper magnesian limestone." From its position, as well as its lithological characters, it appears that this limestone, which is the principal lead-bearing rock in these states, is a continuation of that noticed on the Escanāba, lying above the fossiliferous beds of the Trenton limestone; but that it has increased in thickness, as traced westwardly, and becomes an important member of the series; and hence, we have designated it in the classification of the rocks, as the "Galena limestone."

In the neighborhood of Galena, Dubuque, Mineral Point, and other places, there are numerous localities where a direct succession in the beds may be traced. It is very evident that this limestone diminishes in thickness eastwardly from these points, and becomes a very subordinate member of the series, losing, at the same time, its metalliferous character. From the general absence of fossils, and from its resemblance to the next succeeding limestone in lithological character, no distinction has usually been made between them. In the localities cited, particularly in the neighborhood of Dubuque, the higher grounds are occupied by a limestone containing an abundance of *Catenipora*, *Heliolites*, and other corals marking it as of the age of the Niagara. From the relative position of these coral-bearing rocks to the lead-bearing strata, it has been inferred that they were but parts of the same group, and they have heretofore been described as such.

This was the state of our knowledge, when I examined this series in 1841, and having satisfied myself that the coral-bearing limestone of Wisconsin and Iowa could be clearly identified with the Niagara group of New York, I expressed the opinion that the lower part of the cliff limestone was of the same age. Up to the present time (1850) I am not aware of any published evidence from an examination of the rock in place, to prove that the lead-bearing rock is of lower Silurian age.* The principal fossil resembles a *Coscinopora*, but is probably a *Receptaculites*. I have found, however, in the same rock, the head of an *Illænus*, a *Leptæna*, not unlike *L. alternata*, *Spirifer lynx*, and *Atrypa increbescens*. The still higher, thin-bedded, argillaceous limestone contains a species of *Lingula*, undistinguishable from *L. subquadrata*, *Spirifer lynx*, *Pleurotomaria lenticularis*, *Murchi-*

* Mr. Conrad, in the proceedings of the Academy of Natural Sciences of Philadelphia, 1843, expressed his opinion that the lead-bearing limestone was of the age of the Trenton, from certain fossils obtained at Mineral Point. These fossils are from the Trenton limestone proper, but no productive veins are known to occur in that rock so far as I can learn. Dr. Owen in his [earlier] report upon that region remarked that the veins die out on reaching the "blue limestone." This blue limestone, in Wisconsin, is no other than the Trenton limestone containing large numbers of the fossils peculiar to that rock, and underlying the blue marl and limestone of Cincinnati and other western localities, and which are not recognized in central and western Wisconsin.

sonia bellicincta, and another species with angular volutions, a large *Orthoceras*, and fragments of *Illænus*. Up to this point, I found no corals of the Niagara period, and though the fossils are not numerous, they are all of lower Silurian forms, and furnish the best evidence we have of the age of this limestone.

This lead-bearing rock, as before observed, rests upon fossiliferous strata of the Trenton age, which can be recognized as the identical group traced over several hundred miles. The galena sometimes penetrates the Trenton series, in films or sheets, but does not form veins, as in the grey, heavy-bedded limestone above.

From all the evidence, therefore, the lead-bearing, or Galena limestone, must be regarded as a distinct member of the lower Silurian system, which is not recognized at the east. From its gradual diminution in thickness, its source appears to have been towards the west. The conditions of the ocean, though favorable to the deposition of this immense mass of calcareous materials, were unfavorable to the development of organic life; for, although there remain a few species which continue through the period of its deposition, the greater number known in the group below did not survive beyond the commencement of this. The occurrence of more highly fossiliferous strata above, still of lower Silurian age, would show that the Galena limestone does not form a series of transition beds between the upper and lower Silurian.

To the south and southwest, this rock is of limited extent, probably nearly coincident with the lead region; the universal testimony showing that no productive lead mines exist in the western states, out of the range of this rock.

This fact sets at rest all speculations as to the probable metalliferous character of certain rocks which have been supposed to be identical with the Galena limestone. The lead-bearing rock is a peculiar one, holding a certain place in the series, and of limited geographical extent. It is metalliferous throughout the greater part of its known limits, where it has considerable thickness. The lead veins are almost wholly confined to it, and evidently have their source in it. The small quantities sometimes found below its base are in seams that die out, as they penetrate the inferior rock, and it often takes the form of interlaminations among the strata, having little connection with the lower masses.

Regarding the higher beds before alluded to as identical with the Galena limestone, it is not probable that the rock contains any valuable deposits of ore within the Lake Superior district; but, on the other hand, the small thickness of the mass would preclude the possibility of the occurrence of valuable veins. The fact of the existence of this rock, as a distinct member of the series, is interesting in a geological point of view, and opens the question as to the completeness of the series, which has been studied in New York, Canada, Pennsylvania and Virginia.

There is another question which may arise, and that is, as to the relations of the lead-bearing rock to the Hudson river group; for there appears to be little probability of identifying this group, at any of the localities west of Lake Michigan. If the upper, ash-colored beds on the Escanāba are identical with the Galena limestone, there is then no difficulty in determining the question; for the Hudson river group is superior to this limestone. Although I am not aware that the shales of this group have been recognized in Wisconsin, or Iowa, I am disposed to believe that the numerous fossils found in the drift,—among which are a small *Nucula*, *a Cleidophorus*, *Pleurotomaria*, and others,—are derived from the destruction of beds of that age, which lie above the Galena limestone. This opinion is further strengthened from having found on Little Bay des Noquets some beds filled with small fossils similar to those noticed; and I have received a specimen of similar character, said to have been procured, in place, near Galena.

[Mr. Hall next describes the Upper Silurian and Devonian Series, showing that the region presents strata of the Clinton Group, Niagara Group, Onondaga Salt Group and upper Helderberg Series. The Medina Sandstone which occurs below the Clinton Group was not recognized. The Clinton Group occurs at Drummond's Island, along the eastern shore of Green Bay, on Sturgeon Bay, and probably about Lake Winnebago. To the southwest of Green Bay near Hartford, there is a band of oolitic argillaceous iron ore, similar to that of the Clinton Group in New York. Beyond the entrance to the Bay des Noquets, fossils of a species of Cytherina, an Avicula and Murchisonia subulata were observed; and at Sturgeon Bay, the Pentamerus oblongus. The Clinton Group becomes calcareous to the west; while in central New York it consists mainly of shaly and arenaceous beds, with very subordinate layers of calcareous matter, at the Niagara river it is made up of two members of limestone and one of shale.

In closing his remarks on the Niagara Group, Mr. Hall observes]:—

Notwithstanding all the changes which have taken place in the Niagara group, as developed at different points, and its intimate blending, as in this district, with the limestone of the Clinton group below and the Onondaga salt group above, we find no serious difficulty in recognizing it as a whole, both by lithological characters and fossil remains. We have been able, by these characters combined, to trace its continuation throughout the entire district from east to west. We have seen it, in this extent, assuming different aspects, dependent on causes before adverted to; but we have never failed to find a greater or less number of known characteristic fossils, even where the strata were explored only to a limited extent. Their occurrence, under such circum-

stances, and their persistence over so great an area, where physical characters have in a great degree failed, only serve to demonstrate the value of these means of studying and identifying the stratified deposits.

I have, also, had an opportunity of tracing this group, at intervals, across the country between Lake Michigan and the Mississippi river, and of recognizing it by its fossils, particularly its corals, even beyond that point, to the southwest. It exists in Ohio, Indiana, Kentucky, and Tennessee, and may be recognized, not only by its lithological characters, but by its numerous fossils, identical with those first described in the New York Geological Reports, as occurring in this group. Among these, we have, in addition to the corals, which are the more common fossils, several species of crinoids, the *Caryocrinus*, *Eucalyptocrinus*, *Ichthyocrinus*, and others which are identical with species known to occur in New York.

In a southwesterly direction, and without the limits of this district, this group has been examined by Mr. I. A. Lapham, of Milwaukee, who has had no difficulty in recognizing it by its characteristic fossils. His observations will be found incorporated in the subsequent pages of this report.

Thickness of the Niagara and Clinton Groups.—As already remarked in the commencement of this chapter, the lowest beds are seen at the lake-level on the eastern side of Drummond's island; while on the northern side, we find the higher beds of the Hudson-river group. Again, on the St. Mary's river, the lowest beds of the Clinton group come to the water-level just above Lime island. Passing westward, along the northern shore of Lake Michigan, although numerous undulations are visible, often enabling the observer to see a considerable thickness of strata, yet the lower beds are nowhere exposed, until we arrive at the bluffs on Big Bay des Noquets. I have already mentioned the exposure of the same beds here which we had seen on Drummond's island. The entire height of the cliffs does not exceed two hundred and fifty feet, and we have nowhere evidence of the existence of superior beds of more than one hundred feet in thickness belonging to these groups. The entire thickness, therefore, of the calcareous beds of the Niagara and Clinton groups does not exceed three hundred and fifty feet. This is, I am aware, somewhat less than the estimate of Mr. Murray, in his section across the Grand Manitoulin islands, which, including both groups, amounts to five hundred and sixty feet.* Although there are, at intervals, exposures which appear along the coast, where the rate of dip is such as to give a greater estimated thickness than three hundred and fifty feet, yet we have no positive proof of a greater observed thickness.

* Geological Survey of Canada, Report of Progress, 1847–48.

In this estimate, it must be understood that the elevated portions of the island and peninsula of Mackinac are not included, for they are occupied only to a small extent, if at all, by strata of this age.

[The Onondaga Salt Group occurs at Mackinac and St. Ignace, but only in thin beds, having diminished in thickness to the west. North of St. Ignace Mr. Whittlesey found a marly bed about 50 feet thick, containing gypsum in isolated masses occurring under the same forms as in New York and Canada West. This marly bed with some higher and more calcareous ones, represent the Onondaga salt group. This bed is not recognized along Green Bay or at Milwaukee, and has probably entirely thinned out.

The Upper Helderberg series, including the Schoharie grit, the Onondaga and Corniferous limestone, is largely represented in the west, though those subordinate groups cannot always be made out. The series, like others, is marked by an increase of calcareous matter on passing westward. The limestones have been greatly worn or denuded, and upright or overhanging cliffs, tower rocks, and domes, are of frequent occurrence about the islands and peninsula of Mackinac. Mr. Hall closes with the following observations on the Fossils of the bed]:—

So far as I observed, the island of Mackinac and the adjoining coast, furnish few of the larger corals, though Favosites and many species of Cyathophillidæ are common. Farther south, however, in the neighborhood of Presq'isle, on the western shore of Lake Huron, where the upper beds of this group come to the level of the water, the corals largely predominate, and the conditions of the ocean appear to have been highly favorable to their growth and development.

In collecting fossils about this island, one is liable to be deceived in regard to the character of the rocks, since it often happens that those of the Niagara group have found their way among the loose materials which have been transported from the northern shore. Thus, the *Catenipora*, *Heliolites*, and other corals of the Niagara group may be picked up at the base of these cliffs, associated with those which have fallen from the cliffs above. As a general remark, however, it may be stated that the fossils of the Niagara are much oftener silicified than those of the group under consideration. Owing to this fact, it might readily be inferred, without careful comparison, that the Niagara group existed on the island of Mackinac and on the peninsula to the northwest. Notwithstanding, however, the great similarity in aspect and color, they are very different in their chemical composition and their associated organic remains.

Most of the fossils collected at Mackinac prove to be undescribed, but are identical, however, with species which occur in the same rocks in New York. Some of the smaller bryozöoid

corals belong to the genus *Tremátopora* and *Cladopora*, while two or more species of *Favosites* were observed. A few shells only were collected, and these, with a single exception, belong to the *Brachiopoda*. Among the trilobites was a *Phacops*, resembling *P. bufo* and a species of *Proetus*, both apparently identical with species which occur in the upper Helderberg range. One of the most characteristic species, however, is the *Phacops anchiops*, characteristic of the Schoharie grit. The specimens of the latter fossil, though consisting only of portions of two bucklers, are so peculiarly characteristic, that I cannot doubt the identity in age of these widely-separated localities. Although it would have been desirable to identify a larger number of fossils with those occurring in the corresponding series elsewhere, yet the evidence is sufficient to remove this group from any below the Oriskany sandstone.

ART. XVIII.—*On the Theoretical Relations of Water and Hydrogen;* by T. S. HUNT, Chemist to the Geological Commission of Canada.

IN carrying out his theory of types, M. Laurent proposed to consider water H_2O_2, having its equivalent represented by four volumes of vapor, as the type of the oxyds like M_2O_2, of the hydroxyds $(MH)O_2$, and of the sulphurets corresponding to these two classes. By his system of compound radicals, Liebig had extended to organic chemistry the nomenclature of Lavoisier, and he looked upon spirit of wine $C_4H_6O_2$, as the hydrated oxyd of a radical ethyl ($C_4H_5 = Et$), while hydric ether C_4H_5O, was the simple oxyd of the same radical. But as ether-vapor contains in the same volume, twice as much carbon as the vapor of alcohol, Gerhardt had already proposed to double the formula of ether, and Laurent now showed that while alcohol is to be regarded as the hydroxyd of ethyl $(Et\,H)O_2$, or water in which ethyl replaces an equivalent of hydrogen, ether is the anhydrous oxyd, in which the second equivalent of hydrogen is replaced, and should be written Et_2O_2. Hence while ether is neutral, alcohol is monobasic, having an equivalent of hydrogen replaceable by a metal, and is the type of monobasic vinic acids, while water is the type of bibasic acids. (*Laurent, Récherches sur les Combinaisons azotées. Ann. de Chim. et de Phys.*, *Nov.* 1846.)

In a review of that remarkable essay, published in this Journal for Sept., 1848 (vol. vi, p. 173), I suggested that this view was "susceptible of still farther extension, and that we may include in the same type all those saline combinations (acids) *which contain oxygen.*" I referred to the hypochlorites $Cl\,O$, $M\,O$, as derivatives of the type H_2O_2, in which Cl replaces H; being $(ClH)O_2$,

and (Cl M)O_2, while anhydrous hypochlorous acid is Cl_2O_2, the result of a complete substitution. "In the same manner nitric acid, N HO_3, is a monobasic salt (i. e. acid), corresponding to water in which NO_2 is substituted for H, as in many organic compounds; we have then (NO_2, H)O and (NO_2, M)O;" or (NO_4, H) O_2 in the notation adopted above. "As an adaptation of this idea to bibasic compounds, sulphuric acid, S H_2O_4, is to be regarded as water in which S HO_3 replaces H; thus (S HO_3, H)O. As the replacing elements contain an equivalent of hydrogen which is saline (i. e. replaceable by a metal), the acid is bibasic. When the hydrogen in S HO_3 is replaced by a metal, we have a class of acid sulphates like (S KO_3, H)O. The complete replacement of hydrogen in the original type yields (S HO_3)$_2$O, which is the Nordhausen acid commonly represented by $2SO_3$, H_2O. This latter compound as Gerhardt has shown, corresponds to the anhydrous bisulphate of potash."

"The tribasic acids may equally be reduced to the same type, if we conceive the elements which replace one equivalent of hydrogen, to be bibasic instead of neutral or monobasic; phosphoric acid, P H_3O_4 is (P H_2O_3, H)O."

"The primitive saline type is then essentially bibasic, and is presented in its most elemental form in water, while the simplest type of the monobasic salt, which is a derivative of the last, is found in hypochlorous acid." p. 174. This view of the derivation of polybasic acids is illustrated by the bibasic sulphacetic, and the tribasic sulphosuccinic acid.

On page 177 we further remark, that "the binary molecule of the metals, hydrogen, chlorine, bromine, etc. will be seen to be the type of an immense number of combinations, embracing the various alloys and amalgams, the hydracids like hydrochloric acid, with their corresponding salts, and such compounds as Cl Br and Cl I, while I Cl_5 is referable to a triple molecule of these elements, represented by H_6; to this type belong the perchlorids of antimony, arsenic and phosphorus, while the corresponding trichlorids form a double molecule."

In a subsequent Essay on Chemical Classification read before the American Association for the Advancement of Science, at Philadelphia, in September, 1848, and published in this Journal for May and July, 1849, (vols. vii and viii,) we observed that the relation between alcohol and acetene is that which subsists between the two types H_2O_2, and H_2, acetene being hydrogen in which ethyle replaces H, thus C_4H_5, H $= C_4H_6$, while hydrochloric ether is a chlorinized hydrocarbon corresponding to hydrochloric acid, so that having repeated what has been already cited as to the type H_2, we add, "moreover it follows from the relations of H Cl to the chlorinized hydrocarbons, that it (H_2) is the type of all the hydrocarbons, as well as of the alkaloids

which may be described as amidized species of them, and are equally susceptible of substitutions by chlorine." It was also remarked that "as many neutral oxygenized compounds, which do not possess the saline character, are still derivatives of acids which are referable to the type H_2O_2, we may regard all oxygenized bodies as belonging to this type." "While nitric acid is $\text{Ɲ}HO_3$, or $(NO_2,H)O$, the result of the complete replacement of H by NO_2 will be $(NO_2)_2O$, or the unknown dry nitric acid, homologue of the so-called anhydrous phosphoric and arsenic acids, which are equally $(PO_2)_2O$, etc." Vol. viii, p. 92.

One of the objects proposed in the essay just quoted, was a comparison of the views of Gerhardt and Liebig with regard to the formation of ethers, amids, and allied bodies. Gerhardt in accordance with the electro-chemical theory of Berzelius, had considered the acids in these reactions to be electro-negative by their oxygen, while the alcohols, ammonia, and the hydrocarbons were electro-positive by their hydrogen, so that these bodies *minus* H_2, replaced O_2 in the acid.* To this view we objected that it leaves unexplained that change in the basic relations of the acid, which Liebig rightly understood when he compared the ethers to salts, and represented the acid as losing H, which is replaced by the elements of the alcohol *minus* HO_2. This theory, unlike that of Gerhardt, made the ethers of the hydracids enter into the samé class with those of the oxacids; at the same time it did not include those bodies which are produced with the elimination of H_2O_2, by the action of oxygen acids upon ammonia and hydrocarbons, and which were recognized in Gerhardt's system, as completely analogous to the ethers in the mode of their formation. Here the compound radical theory is found to be defective, although the analogy which forms its point of departure is correct. In concluding this comparison we remarked that "we are led to recognize the view of Liebig, *apart from his ideas of dualism, and his theory of compound radicals*, as the one fundamentally true." (Vol. vii, p. 405).

In this Journal for March, 1848, (vol. v, p. 265,) we observed that the relation of wood-spirit to acetonitryl is the same as that of water to hydrocyanic acid, and that water differs from wood-spirit, precisely as this last differs from spirit of wine, so that the relation of homology, recognized by Gerhardt in the compounds of carbon, is extended to water and hydrogen; for from the relations which we have asserted between H_2O_2, and H_2, it follows that while water is the homologue of the alcohols, hydrogen H_2 is the homologue of acetene C_4H_6, (Et H,) and of formene C_2H_4, (Me H) which Frankland calls hydrids of ethyl and methyl, as well as of his zinco-methyl C_2H_3, Zn. The bodies which he regards as the alcohol radicals are still homologues of

* Précis de Chimie Organique, tom. ii, p. 495.

hydrogen, the result of a complete substitution, and are $(C_2H_3)_2$ etc., like benzile $C_{28}H_{12}O_4$, which is Bz_2, while bitter-almond oil is Bz H.

In the Journal for January, 1850 (vol. ix, p. 65), this is again referred to, and we remark that as water is to be regarded as the homologue of the alcohols, it follows from the principles already laid down "that the ethers are homologous with their parent acids," a point which was illustrated by the action of the cyanic ethers with ammonia; while the volatile bases of Wurtz "sustain to their corresponding alcohols, the same relation that ammonia does to water." In volume xiii. p. 206, we repeat, "water is not only the analogue, but the strict homologue of the alcohols, so that the molecule H_2 is the equivalent (homologue) of C_4H_6, and its homologues, and H of ethyl and methyl; (the hypothetical radicals.)"

The question whether these homologues of hydrogen H_2 are to be regarded as the radicals of the alcohols and ethers, has been discussed by Gerhardt, Hofmann, and others, but resolves itself into this; Frankland's ethyl is to Et Cl what Zn_2 is to Zn Cl, and H_2 to H Cl; the metals, hydrogen and chlorine always present a dualism in their reactions, as marked as ethyl, kakodyl and cyanogen.

Williamson *(Philos. Mag., Nov.*, 1850,) has made a beautiful application of Laurent's theory of the alcohols; by the action of potassic alcohol $(Et\,K)O_2$ upon hydriodic ether, Et I, he obtained K I, and Et_2O_2, and by a similar process mixed ethers, such as $(Et\,Me)O_2$. He at the same time explained the theory of the ordinary ether process, as the reaction between sulphovinic acid, $S_2(Et\,H)O_8$, and $(Et\,H)O_2$, giving $S_2H_2O_8$, and Et_2O_2. Meanwhile Chancel, following out the same idea, announced almost simultaneously with Williamson, the production of hydric ether by the distillation of sulphovinate of potash with potassic alcohol; by the reaction of oxalovinate with sulphomethylate of potash, he also obtained a mixed oxalic ether $C_4(Et\,Me)O_8$, oxalic acid being $C_4H_2O_8$.

Still more recently Gerhardt has announced the production of a class of bodies, which he describes as the anhydrids of the monobasic organic acids. He had formerly regarded the production of anhydrids as characteristic of polybasic acids, because he would not admit the theoretical derivation of monobasic acids from a bibasic type H_2O_2, the complete replacement of whose hydrogen should, as I have long ago shown, yield neutral anhydrids, (like the anhydrous nitric acid since discovered,) sustaining to the corresponding acids the relation of hydric ether to alcohol. According to the view which I advanced in 1848, acetic acid, $C_4H_4O_4$, was represented by $(C_4H_3O_2, H)O_2$, and the anhydrid would be $(C_4H_3O_2)_2O_2=C_8H_6O_6$, corresponding to four vol-

umes of vapor. This body has been obtained by Gerhardt as well as the butyric, valeric, benzoic, and cinnamic anhydrids, besides mixed species analogous to the mixed ethers, such as the aceto-benzoic anhydrid, which contains the elements of one equivalent each of acetic and benzoic acids, *minus* H_2O_2. These bodies are of course neutral, and regenerate acids by assimilating the elements of water.

The process by which these bodies are obtained, is very instructive: when the perchlorid of phosphorus PCl_5, or the oxychlorid PO_2Cl_3 acts upon a salt like the acetate of potash, a body represented by $C_4H_3Cl\,O_2$ is obtained, which bears the same relation to acetic acid that hydrochloric ether does to alcohol; by the action of this acetic chlorid upon acetate of potash, chlorid of potassium and anhydrous acetic acid are produced. Alcohol $C_4H_6O_2$ being represented as $(Et\,H)O_2$, we may write the formula of acetic acid $(Ac\,H)O_2$, $(C_4H_3O_2 = Ac)$, while the chlorid is $Ac\,Cl$. This corresponds to hydrochloric or hydriodic ether, while acetate of potash $(Ac\,K)O_2$, is analogous to potassic alcohol. The process is then similar to that by which Williamson obtained hydric ether; $Ac\,Cl + (Ac\,K)O_2 = K\,Cl + Ac_2O_2$, or the anhydrous acid.

The reaction in all these cases is, as I have pointed out in the paper before quoted, (vol. viii, p. 93) identical in essence with that between $H\,Cl$ and $(K\,H)O_2$, yielding an alkaline chlorid KCl, and water H_2O_2, the prototype of all the above acids, ethers, alcohols, and anhydrids. We have there also remarked that H_2O_2 is to be regarded as a derivative of hydrogen, H_2, and that it is often difficult to distinguish between the types. Thus, for example, the acetic chlorid might be regarded as a chlorinized aldehyde, $(C_4H_3, Cl)O_2$, belonging to the second type, while its reactions permit us to compare it with the hydrochloric ethers of the type H_2. It must be kept in mind that although the apparent dualism deduced from the results of chemical change, is subject to but very simple variations in the elements, it is exhibited in so many different ways in the higher species, that we cannot assign an absolute value to any hypotheses based upon their changes.

I have been particular in again bringing forward these views, because they now belong to the history of chemical theory, and because after having maintained them alone since 1848, and having insisted upon them in various ways in my communications to this Journal, I now find them brought forward by Williamson, Brodie and Gerhardt. This latter chemist in a paper presented to the French Academy in June, 1852, and published in the *Annales de Chimie et de Physique* for March, 1853, abandons those theories to which I long since objected, and brings forward, with a similarity of thought and expression not to be mistaken, the views upon which I have here insisted. (See as above pp. 336—342.

Williamson in a paper read before the British Association in July, in 1851, has expressed the same ideas with regard to the typical relations of water, and is recognized by the English editor of Gmelin's Handbook (vol. vii, pp. 17 and 201) as the author of the theory. See also Brodie's lecture before the Royal Institution in May, 1853, (Chemical Gazette, Aug., 1st) "On the formation of hydrogen and its homologues."

It is gratifying to find that the views which I have so long maintained, are at last recognized by chemists, and are found productive of beautiful and important results; but it would be only just in these chemists, to have admitted the priority, by three or four years, of my own published views, anticipating the brilliant series of discoveries which have served them as the basis of their generalizations.

Montreal, Dec. 20, 1853.

ART. XIX.—*Notice of a Geological Map of the United States and the British Provinces of North America, with Explanatory Text, Geological Sections and Plates of the Fossils which characterize the formations,* by J. Marcou.*

A GEOLOGICAL map of the United States by a member of the Geological Society of France is likely to command attention both in this country and Europe; and it is therefore important to know how far it is a correct exposition of the present state of American Geological Science. We have therefore examined the map of Mr. Marcou and the accompanying text with much interest, and with no less disappointment. We may briefly run over a few of the earlier pages, and take a cursory glance at the map, for the benefit of the science, mentioning some of the errors, omissions, or objectionable points that have struck us.

In his introduction, after mentioning Maclure, Mr. Marcou speaks of Conrad and Lea, (p. 14) celebrated conchologists of Philadelphia, and leaves out Morton altogether. He then observes that Jackson and Alger published in 1828 a geological description of Nova Scotia, etc., and adds "Such nearly was the condition of geology in America when Murchison published his celebrated book entitled the Silurian System,"—leaving it to be inferred that from 1828 to 1839, when Murchison's work appeared, nothing had been done by geologists in America:—notwithstanding the publication in this period of the Final Report on the geology of New Jersey, the first Final Report on Massachusetts, the An-

* A Geological Map of the United States and the British Provinces of North America, with explanatory text, geological sections and plates of the fossils which characterize the formations. By Jules Marcou, United States Geologist, member of the Geological Society of France, etc., etc. *Boston*, Gould & Lincoln.

nual Reports of Pennsylvania, New York, Ohio, Michigan, Tennessee, and Maine, which gave altogether a very good general view of the Geology of a great part of the United States.

On page 15, the author says, "Troost, Vanuxem and Eaton were also among the first to compare the American formations with those of Europe, and *laid the true foundations on which all the geological maps and memoirs published on this side of the Atlantic for sixteen years, have been constructed.*" Now as one example in point, Troost identified the Silurian of Tennessee with the carboniferous of Europe, and Eaton the Silurian of New York with the New Red Sandstone. Giving every due credit to these named geologists for their labors, which in many respects had important results, nothing is more entirely unfounded than the above quoted assertion; what they did could not by any possibility have served as a foundation for maps and memoirs subsequently published. Mr. Vanuxem did indeed, in the division of the Cretaceous, identify that formation of New Jersey with the same in Europe; and had from the beginning a clearer idea of the age of our geological formations, as compared with Europe, than any other geologist.

The map of Byrem Lawrence, which is next mentioned, in terms of high praise, is essentially copied from a map made by Dr. D. D. Owen, and published in the Transactions of the Geological Society of London.

We proceed to the body of the work.

The *Lingula antiqua*, our author says, "is found in New York, Michigan and Wisconsin." We do not know of it in Michigan;—perhaps in the same locality with *Lingula prima* (Foster and Whitney's Report) in the Lake Superior region, where it occurs in a sandstone which Mr. Marcou further on calls the New Red Sandstone. The last sentence in the same paragraph reads thus: "Its thickness varies with the different localities in which it is found, and depends on the more or less horizontal position of the bed; nevertheless it may be said to vary from 500 to 2000 feet." Prof. Rogers estimates this rock in Pennsylvania with doubt at 1000 feet, and in many parts of the United States it varies from 150 to 300 feet. Dr. Owen estimates the thickness on the Mississippi at much less than 1000 feet, and we do not know any authority for believing it greater than this. Why the thickness should depend on the more or less horizontal position of the bed, is what we do not comprehend.

Under the Trenton Limestone, page 22, we find, "It is in this division that for the first time is presented a complete fauna, representing the first degree of the biologic development of our planet. The first division of the Lower Silurian (i. e. Potsdam) offers only a few species of animals, rarely to be found, and mostly in a bad state of preservation."

The writer appears to be ignorant of what Dr. Owen has discovered in these strata on the upper Mississippi, viz.: *Trilobites, Corals, Crinoids, Orthis, Lingula, etc.* The Lingulæ are in a most perfect state of preservation; and in numbers of individuals unsurpassed in any formation of any period. The author speaks of Barrande's labors in Bohemia, but does not seem to know that Barrande too has described a distinct fauna in these lower beds, or their equivalent in age, and places there very properly, as do all American geologists, the first degree or stage in the "biologic development of our planet."

The author gives, among his characteristic fossils of the Trenton formation, *Orthoceratites communis*, of Wahlenberg, and refers the name to a figure of *Cameroceras trentonense*, evidently copied from Palæontology of New York, vol. i, pl. 56, fig. 4; a fossil very unlike the *O. communis* in every respect. Still further, he cites it as common in New York, Pennsylvania, Canada, the Mingan Islands, on the coast of Labrador, and in Newfoundland, near the straits of Belle Isle. Now the form figured is a rare fossil, known only in a few specimens found in New York.

The author cites *Orthis testudinaria* and *Verneuili*, Dalm., as "species of Brachiopods related to Spirifer, which are found equally in Europe and America." If the author has been as careless of his localities as he has been in his citations of authorities, we can judge very well of his accuracy in this case. The O. Verneuili is one of Eichwald's species, and the figure in Marcou's book is copied directly, reduced in size, from Murchison and de Verneuil's Russia and the Ural Mountains, vol. ii, pl. 12, fig. 1; and is there cited from two localities in Russia, viz., Reval and the island of Dago. This is the first intimation of its having been found in America; and as no locality is given, we may be permitted to discredit it altogether. Orthis testudinaria is an abundant species in Europe and America.

On page 23, it is asserted that most of the blue limestone in the neighborhood of Cincinnati is of the Trenton formation. This was believed ten years since. The author appears not to be aware of what has been published during the last three or four years, or he would not have made the mistake.

On page 24, speaking of the Hudson river group and Utica slate, he says: "Fossils are rare in this division." Perhaps no portion of the palæozoic rocks is more densely crowded with fossils than this group when in an unaltered condition. In central and north New York, in "Upper Canada and Bay des Noquets," the two latter localities cited by our author,—the fossils are extremely abundant. While Mr. Marcou says the only fossils of these rocks are Graptolites and fragments of Trilobites, it is shown in the Palæontology of New York that more than sixty

species of fossils are restricted to these beds, and more than thirty others are common to this formation and the Trenton limestone.

Under the term Upper Silurian our author includes numerous rocks and groups, and among them the Pentamerus limestone of New York. This rock was so named from its containing great numbers of *Pentamerus galeatus*, this being, with its associated shaly limestone, the only position of that fossil. Now our author makes the *rock Silurian*, (p. 25) and describes and figures the *fossil as Devonian* (p. 31). He asserts that it is "common to Upper Silurian and Devonian of Europe, and is in the same geological position in America, but on this side of the Atlantic, it is especially found in the Devonian division." Now this fossil is never found in any rocks of America included by this or any other author under Devonian; nor is it true that it occurs in the Devonian of Europe. Had our author read what was published on the other side of the Atlantic as long ago as 1848, he would have known this.

The author cites Upper Silurian rocks as forming "the upper part of the Falls of the Mississippi at Fort Snelling." The explorations of Dr. Owen and others have shown that the rock is the Trenton limestone; and it is underlaid by a soft, shaly and fucoidal mass representing the Birdseye limestone; and that this rests on a sandstone belonging to the lower formations. It will surprise the ardent collectors of Ohio to learn that they have upper silurian rocks "in the environs of Cincinnati."*

We much incline to doubt whether Fremont, Stansbury, or Wislizenius have brought unequivocal Silurian rocks or fossils from "several points in the Rocky Mountains," and we equally question whether *Pentamerus oblongus* has been found in North Carolina and Minnesota. Lockport, N. Y., Chicago and Minnesota are given as localities of *Favosites gothlandica*, which is not known in either of these places. On page 28, we learn for the first time that "*the Upper Silurian in America often contains beds of rock salt.*" The italicising is ours, the statement is the author's.

Leaving the Upper Silurian, we find under the Devonian, a heterogeneous assemblage of rocks, as one or two citations will show.

On page 29, our author says, "very fossiliferous sandstones form the first devonian beds in Pennsylvania and New York; then comes a great extent of marl and clay, presenting in certain localities quite numerous fossils; and lastly, the whole is crowned by very deep red sandstone, especially at the Catskill mountains, N. Y., at the base of the Alleghany mountains, Pa., and at Gaspé, Lower Canada."

* We know something of the rapid growth of western towns, but we cannot suppose that the environs of Cincinnati extend to forty or fifty miles.

Our author here takes no notice of one of the most extensive limestone formations in the United States, which extends from the Helderberg in New York to the Niagara river at Black Rock, and thence through Canada West, and forms the higher part of Mackinac and the northern part of Michigan proper, or the lower peninsular of Michigan. The same limestone extends from northern Ohio to Tennessee; and though elsewhere noticed, it is here omitted in the description of the successive beds or formations which constitute the author's Devonian System.

On page 30, he says, "Since Mr. Agassiz has recognized carboniferous fishes, and Goniatites in the black slate of Ohio, this group ought to be placed in the lower carboniferous of which it forms the base." Our author has evidently some confused idea that the position of the shales and sandstones of Ohio had been a mooted question, and that it has been settled as above. The truth is that Prof. Agassiz has not recognized carboniferous fishes in the black slate of Ohio; no fishes at all have been found in that slate: neither have any Goniatites been found in the black slate of Ohio; and those found in the black shale of Indiana were not recognized by Prof. Agassiz.

We will cite another sentence following the last noted: "The Island of Mackinac, the Ohio falls near Louisville, and Perry county, Tennessee, have become classic points for the American Devonian, on account of the great number of fossils which are found there, and their identity in species, for the most part, with those found in the Eifel, in the Hartz mountains, and in Devonshire."

Now Mackinac is a locality of Devonian limestone in part, and yielding very few fossils. The falls of the Ohio consist of Silurian and Devonian rocks, each yielding a good number of species. In Perry county nine-tenths of the fossils are Silurian, and their identity with the Eifel species may or may not be true, to some extent. The only fossil cited by the author from Perry county, as far as we observe, is *Hypanthocrinus decorus*, a decidedly Silurian fossil (see page 27, and figure). Our author cites *Spirifer mucronatus*, and applies the name to two figures, both copied from Reports of New York, one of which is the true species, while the other is *Spirifer macronotus*, a widely distinct species, the former having a narrow area and strong plications, the latter a wide area and numerous fine plications. He is equally unfortunate in his localities, citing New York, Ohio, and Tennessee. We suspect that no other person has seen either of these from the two last named localities.

Spirifer heteroclitus, Def. pl. iii, fig. 7, 7*a*.—For this species our author has reduced in size two figures of *Spirifer congestus*, from Report 4th, Geol. District of New York, which species differs so widely from *Spirifer heteroclitus*, that we cannot comprehend

how he could have made the mistake. To make the matter worse, he cites the following localities in the United States: "It is very frequent at the Ohio fall, and at Charlestown Road, Indiana; in Ohio, New York, Pennsylvania and Tennessee." Now the species figured is not *Spirifer heteroclitus*, it has not been found at either of the localities named except New York; the species found at the Ohio Fall and Charlestown Road is neither the species figured, nor is it the *Spirifer heteroclitus;* though not only it, but one other species of that vicinity, resembles the Sp. heteroclitus still more strongly than the one figured: and finally, we venture to say, that neither the species figured, nor either of those at the Ohio Fall or Charlestown Road, have been found in the other localities cited.

As an offset to the above, *Chonetes nana*, which is abundant and almost universal in rocks of this age, is cited as found only in the environs of Louisville.

Our author commences his description of our carboniferous rocks by insisting upon the existence of "vast beds of gypsum and rock salt." The former is true of a few localities; as to the latter, one well authenticated locality only, so far as appears, furnishes rock salt.

It is very remarkable that our author should have repeated the same rocks and groups under carboniferous, which have been described under Devonian, viz.: "The bituminous shale, (or black slate) and Waverly sandstone series, and the fine-grained sandstones of Ohio, Illinois, Indiana, the black slate of Tennessee," &c. These names, it is true, are not used, since he has omitted reference to all the western states in his Synonyma of Devonian sedimentary rocks; but as will be seen, he cannot extricate himself from this difficulty. On page 30, he thus describes the Devonian: "To the west it extends through the southern part of the state of New York, forms the whole contour of Lakes Erie and St. Clair," &c. Now the extension of those rocks occupying southern New York, and along the shore of Lake Erie, to form its "whole contour," comes thence into Ohio, not by any identification or parallelism, by lithological or fossil affinities, but by absolute continuity. Yet our author describes first his Devonian as forming the contour of Lake Erie, and afterwards represents the same beds, the bituminous shale and Waverly sandstones of Ohio, as carboniferous,—these very rocks themselves forming the southern contour of Lake Erie.

We have thus run over the work to the 33d page, and here leave it. One point only we will notice. The sandstone of Lake Superior is classed as the *New Red*, notwithstanding all the labors of Logan, Owen, Foster and Whitney, and others, who have agreed in considering it lower Silurian.

We will say nothing of our author's attempts to systematize our mountain chains; if we needed a parody on Elie de Beaumont and his systems of mountains, we have it here.

Mr. Marcou laments the want of accurate topographical maps, and because he has not such guides, he does not lay down on his map any mountain chains, but has "written *near* the places occupied by the different chains of mountains, the names of those chains." For the same reason we suppose, or some other equally cogent, he has given no lines of States, but has written the names of the various states *somewhere near* the places occupied by them respectively. This allows the limits of the various geological formations to be laid down *any where near* the places they occupy, without giving the pupil or the critic the means of determining their accuracy within many miles, a device perhaps convenient for the author, but not so for the student.

The map in its geology is little more than a reproduction of that published by Lyell in 1845, and in many respects it is inferior to that. As we have remarked respecting the text, the sandstone of Lake Superior is represented as the New Red, or of the age of that of the Connecticut, New Jersey, &c. But besides this, he continues a belt of the same formation across from the head of Lake Superior, by the sources of Red river to the head of the Coteau des Prairies. In that direction he again takes up the same formation in the Wind River chain of mountains. In both these instances, there is not the slightest authority for supposing such belts of sandstone formation, and particularly any of the age of the New Red.

Without the slightest reason or authority, and in the face of facts, he runs a belt of lower carboniferous from the coal field of Michigan to the northeastern part of the Illinois coal field, and another from the eastern side of the Illinois coal field to the northwestern side of the Alleghany coal field. The latter is a worse error than the former, for he positively traces it across a broad belt of Silurian rocks which are a prolongation of those to the north of Cincinnati, and which are clearly followed northeastward to the lake shore and the islands west of Sandusky.

In like manner he traces another similar belt of carboniferous rocks to the southward of the Cincinnati axis, connecting the east and west coal field. The idea many years since promulgated that the Waverly sandstone of Ohio did pass around to the south side of this elevation, was long since proved otherwise; and no geologist for the past seven years would have ventured to republish an exploded error.

The union of the coal field of Missouri and Iowa with that of Arkansas is without authority; and so also the extension of lower carboniferous over so great a breadth of territory to the westward.

The Devonian formations, which on the map are colored in a broad belt through New York and thence narrowing westward to Cleveland and Sandusky, are there represented as cut off by the belt of lower carboniferous, before mentioned, which runs westward to the Illinois coal field. Now the fact is, that the formations traced through southern New York and thence to Cleveland, are absolutely and unmistakeably continuous along the western margin of the great Alleghany coal field, through Ohio and Kentucky, and even into Tennessee and Alabama.

Our author has recognized the formation about Richmond, Va. as Liasic, while the same formation in North Carolina is colored as New Red Sandstone. We do not discuss the question of the age of the New Red or Triassic of Connecticut Valley and further south: we only say here that there is no reason whatever for regarding that of North Carolina as differing in age from that of the Richmond basin.

There are other errors with regard to regions less known, as, for example, that of extending the cretaceous area to the east side of the Missouri for several hundred miles above Council Bluffs; and of terminating the same formation on the northwest more than a hundred miles short of its known limits. But these are excusable, compared with many points we have passed in review.

Art. XX.—*On the Chemical Composition of the minerals Algerite and Apatite;* by J. D. Whitney.

1. *Algerite.*

In the Journal of the Boston Natural History Society (vol. vi, p. 118) an account of the analysis by T. S. Hunt of a supposed new mineral, to which he has given the name of *Algerite*, will be found. Another analysis of the same substance by Mr. R. Crossley has been published (see Am. Jour. Science, [2], x, 77).

Having been furnished by F. Alger, Esq. with specimens of this mineral, I have made an examination and analysis of it, the results of which appear to be of interest in their relation to an important branch of mineralogical chemistry, hitherto much neglected, but which has received a new impulse from the laborious and interesting researches of G. Bischof.

The first light thrown upon the real nature of this supposed new mineral species, was by J. D. Dana, who was struck by the evident altered appearance of the specimens examined by him, and published the following remarks (Am. Jour. Sci., [2], xv, 440): "I am satisfied that the form of the crystals is a *square prism.* In external appearance they would not be distinguished

from scapolite, and this naturally suggests some relation to this species." The results which I have obtained in my examination of Algerite seem to sustain the opinion of Dana, and it appears highly probable that this is an altered mineral, and also, as suggested above by Dana, a scapolite of the Wernerite variety.

On the first examination of this substance it seems evident that it is a mineral in the progress of decomposition. In some of the specimens the crystals have been wholly removed from the matrix, or only a trace of a brownish yellow powder left remaining. Their hardness varies from 2·5 or 3 to 3·5 or 4.

A comparison of the results of the analyses of Messrs. Hunt and Crossley will show that the discordances are too great to be explained on the ground of errors of analysis. My own analysis differs still more from theirs, than they do from each other, and though time and material were wanting to enable me to make as accurate a determination of some of the ingredients as I could have wished, yet it seems, at least, sufficient to settle the question of the claim of this substance to rank as a distinct species.

In noticing Hunt's analysis it will be seen at once that the ratio of the oxygen of the silica and bases cannot be expressed by any approximate simple numbers; so, also, in Mr. Crossley's, the ratio of the oxygen of $\dddot{Si}$, $\dddot{\bar{A}l}$, $\dot{R}$ & $\dot{\bar{H}}$ is given as 7 : 3 : 1 : 1; while it is really 7·05 : 3·22 : 1·22 : 1.

For convenience of comparison, the results of these two analyses are here given, side by side:

	Hunt.		Crossley.
Silica, - - - -	49·82	-	49·96
Alumina, - - - -	24·91	-	24·41
Peroxyd of iron, -	1·85	-	1·48
Magnesia, - - - -	1·15	-	5·18
Potash and traces of soda,	10·21	potash,	9·97
Carbonate of lime, -	3·94	-	4·21
Water, - - - -	7·57	-	5·06
	99·45		100·27

The results of my examinations are as follows:*

Before the blowpipe it blackens a little and soon fuses, intumescing considerably, giving a colorless glass, and glowing with a vivid light.

A small quantity of the mineral, selected with care as being the least altered in character, was ignited in small fragments, and lost 6·20 per cent. of water; another portion, which had the appearance of being more decomposed, lost, under the same treatment 6·68 per cent. The pulverized mineral was found to be

* It is proper to remark that these investigations were made two years since, and that the publication of them has been delayed in the hope of an opportunity to make still farther examinations.

very little acted on by chlorohydric acid, even when digested with it for a long time. The acid however took up a small portion of lime which was intimately disseminated through the mineral in the form of a carbonate. After the ignition it was noticed that portions of the ignited mineral remained nearly unaltered in appearance, while the larger part acquired a brick-red color, and on examination with the microscope was seen to contain silvery white scales, apparently of mica.

As only a small quantity could be used for analysis, the results can be relied on only as approximately correct. They are as follows:

Silica,	52·09
Alumina, and a little $\ddot{F}e$,	18·63
Phosphate of lime,	8·22
Carbonate of lime,	4·41
Water,	6·68
Loss, potash and soda?	9·97
	100·00

The difficulty of reconciling this analysis with either of those cited above, on the hypothesis of the algerite being a mineral of a fixed composition, will be apparent. Several specimens were examined and all were found to contain phosphate of lime.

Scapolite is, of all others, a mineral which seems most liable to metamorphosis. Numerous examples of this will be found in Bischof's "Lehrbuch der chemischen und physikalischen Geologie" (ii, 403, 1433, &c.). It is proved by the analysis of a mica in the form of scapolite (from Arendal) that all the lime of this mineral may be removed and a part replaced by potash. It is proved by numerous analyses that a portion of the lime may remain in combination with carbonic acid. It is also demonstrated that the lime may be exchanged for magnesia so as to give rise to a magnesian mica; also that the entire mass may be converted into steatite, or a hydrous silicate of magnesia. My analysis shows in addition to these facts, that in the process of metamorphosis a part of the lime may enter into combination with *phosphoric acid*. It is interesting to notice, in this connection, the analyses by Messrs. Brewer and Garrett of a substance very similar in composition to the Algerite, and also evidently an altered scapolite, which are published in Dana's Mineralogy, third edition, p. 680.

So much is evident, that the Algerite is not a homogeneous mineral and cannot claim to rank as a distinct species; and it is highly probable that it adds one to the already numerous list of the products of the transformation of the scapolite family. Further analyses of specimens from this locality, if such can be obtained, will throw more light on this interesting subject.

2. *Apatite.*

The theoretical composition of apatite according to the latest atomic weights (Ca=250, Cl=443·3, P=400, F=235·4) is as follows:

	Ca	Cl	$\ddot{\bar{P}}$	$\dot{C}a$		Ca Cl	$\dot{C}a^3\ddot{\bar{P}}$
Chlorine Apatite,	3·82	6·77	41·27	48·14;	or	10·59	89·41
	Ca	Fl	$\ddot{\bar{P}}$	$\dot{C}a$		Ca Fl	$\dot{C}a^3\ddot{\bar{P}}$
Fluorine Apatite,	3·94	3·72	42·62	49·72;	or	7·66	92·34

There has been a good deal of discussion as to whether the formula proposed by G. Rose was borne out by the analyses. Rammelsberg analyzed the fine crystallized apatite from the Zillerthal,* and endeavored to make a direct determination of the fluorine. He only succeeded in obtaining about one-fourth of the amount required by the formula, and remarks that he leaves it undecided whether this result is due to the imperfection of the analytical processes, or to the fact that the formula needs revision. At the request of G. Rose, R. Weber made several analyses of the Snarum apatite,† the results of which are regarded by Rose as decisive in favor of his formula. C. A. Joy‡ also analyzed a specimen from Faldigl in Tyrol, which gave a pretty near accordance with the amount required by Rose's formula. No one has succeeded in obtaining the required amount of fluorine by direct determination, and the analyses have been made up from the amount of phosphoric acid, lime and chlorine by calculation. It will be noticed that all the analyses give an *excess* of phosphate of lime. There is invariably a deficiency of chlorine or fluorine combined with calcium, and the analyses seem to indicate that the greater the degree of purity of the substance, the nearer the approach to the requirements of the formula. Almost all the analyses of the uncrystallized varieties show a very considerable deficiency in the fluorine and chlorine.

In order to throw some light on this question, I have analyzed a specimen of apatite from Hurdstown, N. J., furnished me by F. Alger, Esq.; the substance examined was broken from a large crystal of a greenish-yellow color, and was translucent on the edges. Two analyses were made, in one of which the separation of the phosphoric acid was effected by mercury, according to Rose's process; in the other the lime was separated by means of sulphuric acid and alcohol. The results were:

	I.	II.
Insoluble,	0·29	0·25
Lime,	53·50	53·37
Phosphoric acid,	43·28	43·17
Chlorine,	——	1·02
Iron,	*trace*	*trace*

* Ramm. 2d Supp., p. 15. † Pogg. 84, 303. ‡ Inaugural Dissertation, p. 44.

These results give on calculation:

	$\dot{C}a^3\dddot{P}$	Ca Cl	Ca Fl
I.	93·77	1·59	3·04
II.	93·54	1·59	3·02

The analyses agree well together, but give too small a quantity of chlorid and fluorid of calcium.

Art. XXI.—*Contributions to Chemical Mineralogy;* by James D. Dana.—Part II.

1. *Relations of Anhydrous Carbonates and Sulphates.*

The Carbonates that come under the general formula $\dot{R}\ddot{C}$, and the Sulphates, $\dot{R}\dddot{S}$, present each a case of trimorphism: and a third example of similar character is found in the combinations of Sulphates and Carbonates ($\dot{R}\dddot{S}+n\dot{R}\ddot{C}$). These cases are as follows; the angles given are R : R in the rhombodedral forms, and I : I (∞ P) in the prismatic:—

Rhombohedral.	*Trimetric.*	*Monoclinic* (basal cleavage).
Calcite, $\dot{C}a\ddot{C}$; 105° 05′	Arragonite, $\dot{C}a\ddot{C}$; 116° 10′	Barytocalcite, $(\dot{C}a, \dot{B}a)\ddot{C}$; 95° 8′
Dreelite, $(\dot{C}a,\dot{B}a)\dddot{S}$; 93–94°	Anglesite, $\dot{P}b\dddot{S}$; 103° 38′ Anhydrite, $\dot{C}a\dddot{S}$; 102° 56′ Barytes, $\dot{B}a\dddot{S}$; 101° 42′	Glauberite, $(\dot{C}a, \dot{N}a)\dddot{S}$; 83°
Susannite, $\dot{P}b\dddot{S}+3\dot{P}b\ddot{C}$; 94°	Leadhillite, $\dot{P}b\dddot{S}+3\dot{P}b\ddot{C}$; 103° 16′	Lanarkite, $\dot{P}b\dddot{S}+\dot{P}b\ddot{C}$ 85° 48′

There is a remarkable parallelism between the sulphates and carbonates. The difference between the angle of Arragonite and Calcite is about 11 degrees; between Calcite and Barytocalcite 10 degrees. Nearly parallel with this, the angle in Anglesite or Anhydrite is about 10 degrees larger than that in Dreelite, and that of Dreelite is 10 degrees larger than that of Glauberite. The sulphatocarbonates correspond with the sulphates nearly in angle, and confirm this parallelism. Susannite and Dreelite are nearly the same in angle; so also Leadhillite and Anglesite, and Lanarkite and Glauberite.*

It hence follows that the *homologous* prisms in Arragonite and Anglesite are the prisms, above mentioned, of 116° 10′ and

* The angle of Susannite mentioned by Haidinger is that of 2R. Brooke and Miller give Leadhillite another position which makes the angle I : I = 120° 20′. It is probable that this is not the correct position, from the fact that the angle instead of being between that of the carbonate and sulphate of lead, or identical with one or the other, is much larger than that of the carbonate. In another position, taking 1ĭ as I, we have I : I = 103° 16′, or near Anglesite. This view is sustained by the relations of Susannite (which is identical with Leadhillite in composition) to Dreelite.

In Lanarkite, according to measurements by R. P. Greg, Esq., recently communicated to the writer, the occurring vertical prism has the front angle 49° 50′; and this being taken as the prism $i\bar{2}$ (∞ P′2), makes I : I = 85° 48′, as above stated.

103° 38′; and the carbonate and sulphate series differ fundamentally by 10 to 15 degrees. Hausmann therefore cannot be right in making the carbonates and sulphates homœomorphous, by assuming a different prism in Anglesite (one usually taken as a horizontal prism) as the fundamental vertical prism; the alleged homœomorphism was based on a comparison of parts *not homologous*, and does not exist. This conclusion also follows from the fact that the common hexagonal and 6-rayed twins of Arragonite and Cerusite, arising from the nearness of the angle of the prism to 120°, never occur in the sulphates, showing that the angle of the fundamental prism is not near 120°.

2. *Homœomorphism of Beryl, Pyrosmalite, Dioptase, and Eudialyte.*

	O : 1	O : 2*
Beryl, $(\bar{Be}, \bar{Al})\bar{\bar{Si}}^2$ - - - -	150° 3′	130° 57′
Pyrosmalite, $4(\dot{Fe}, \dot{Mn})^3 \bar{\bar{Si}}^2 + Fe\,Cl^3\ [+\bar{Fe}\,\dot{H}^6$ -	148° 30′	129° 3′
Eudialyte, $\dot{R}^3 \bar{\bar{Si}}^2 + \bar{Zr}\,\bar{\bar{Si}}^2$ - - -	148° 38′	129° 21′
Dioptase, $\dot{Cu}^3 \bar{\bar{Si}}^2 + 3\dot{H}$ - - - -	148° 38′	129° 21′

O is the basal or terminal plane in each, and 1 is 1P. In Dioptase it is a cleavage plane (1R), and in Eudialyte, although usually designated ¼R it is also a direction of cleavage, though imperfect cleavage, and should be taken as 1R.

With the above group, the following may also be compared:

Cinnabar, Hg S, (R=92° 36′)	O : ½=146° 32′	O : 1=127° 06′
Quartz, $\bar{\bar{Si}}$, (R=94° 15′)	O : ½=147° 35′	O : 1=128° 18′
Dreelite, $(\dot{Ca}, \dot{Ba})\bar{\bar{S}}$, (R=93°–94°)		
Susannite, $\dot{Pb}\,\bar{\bar{S}} + 3\dot{Pb}\,\bar{C}$, (R=94°)	O : ½=147° 26′	O : 1=128° 03′

It is possible that the plane ½ in Cinnabar and Quartz may be homologous with that of 1 in Dioptase or Beryl. With regard to Quartz or Dreelite, there is the objection to this view that they do not correspond with Dioptase in cleavage. Taking them as here designated, the vertical axis in each is twice that of Beryl. *Chabazite* also is near quartz in angle.

3. *Homœomorphism of Pyrrhotine (Magnetic Pyrites), Greenockite, Breithauptite, Copper Nickel, Nepheline, Cancrinite.*

Pyrrhotine, $Fe^7 S^8$; Fe S?	O : 1=135° 16′	O : ½=153° 39′
Greenockite, Ca S,	O : 1=136° 24′	O : ½=154° 32′
Breithauptite, Ni Sb,	O : 1=135° 15′	O : ½=153° 38′
Copper Nickel, Ni As,	O : 1=136° 35′	O : ½=154° 41′
Nepheline, $(\dot{Na}, \dot{K})^2 \bar{\bar{Si}} + 2\bar{Al}\,\bar{\bar{Si}}$	O : 1=136°	O : ½=154° 13½′

Cancrinite and Nepheline are nearly identical in angle. Millerite (Ni S) diverges from the allied species; O : R = 159° 10′, being 4½° beyond O : ½ in either of the above.

* The expressions here used for the planes are essentially those of Naumann, only I (or I) is written for ∞, and the P is dropped. In the monoclinic system the clinodiagonal forms are distinguished by a grave accent (\`).

These species have O : 1 nearly coincident with O : 2-2 in beryl (2-2, or 2P2 being a plane on the basal angle of the hexagonal prism). Hence if the series of planes on the angles in Nepheline and others of this group is homologous with that on the edges in Beryl, the two groups would be properly united. But there is no good reason for this supposition. Taking the planes as presented, the vertical axis in the Beryl group is to that in the Nepheline as $\sqrt{5} : 4$.

4. *Homœomorphism of Willemite, Phenacite, and the species of the Corundum Group.*

Willemite, $\dot{Z}n^3 \dddot{S}i$, - - - - - -	R=115°
Phenacite, $\bar{\bar{B}}e \dddot{S}i$ - - - - - -	R=116° 40′

These angles are the angles of $\frac{1}{2}$R in the Corundum Group, and may be reasonably taken as of $\frac{1}{2}$R in Willemite and Phenacite. $\frac{1}{2} : \frac{1}{2}$ in Specular Iron = 115° 22′. Oxyd of Zinc ($\dot{Z}n$) is shown by G. Rose (Kryst.-Chem., p. 64) to have the form essentially of Corundum; and water ($\dot{H}$) is probably of this group. Corundum has for O : R, 122° 26′, being 4° 40′ less than in Cinnabar, and nearly 6° less than in Quartz.

5. *Homœomorphism of Apophyllite, Nagyagite, Uranite, Anatase, Matlockite.*

These species have a more or less perfect basal cleavage.

	O : 1
Apophyllite, $(\dot{C}a, \dot{K}) \dddot{S}i+2\dot{H}$ - - -	119° 30′
Nagyagite, $(Pb, Au)(Te, S)^2$ - - -	118° 37′
Uranite, $\dot{C}a \dddot{\bar{P}}+\dddot{\bar{U}}^2 \dddot{\bar{P}}+16\dot{H}$ - - -	118° 35′
Anatase, $\ddot{T}i$ - - - - -	119° 22′
Matlockite, Pb Cl+Pb O - - -	119° 34′

Hausmannite ($\dot{M}n \dddot{\bar{M}}n$) is stated to be homœomorphous with Anatase; its angles diverge more widely than the others from the type, O : 1 giving 121° 3′. It is intermediate between this group and the Idocrase series. *Calomel* has O : 1 = 119° 51′. But its cleavage is not basal.

The *Zircon* group, which includes *Zircon*, *Rutile*, *Cassiterite*, *Œrstedite*, have the vertical axis twice that of the group above. O : 1 in Zircon = 137° 50′, and O : 2 = 118° 54′. Hence if the plane 1 in the Apophyllite series may be taken as 2, the groups come together.

The *Scapolite* species, *Scapolite*, *Meionite*, *Mellilite*, *Sarcolite*, may be of the Zircon type. O : 1 in Scapolite is 148° 20′; and in Zircon O : 1*i* (the intermediate octahedron, or, P ∞) = 147° 22′. So that if the prism of most perfect cleavage in Scapolite be taken as the fundamental prism, and $\frac{1}{2}$P ∞ becomes $\frac{1}{2}$P, the two species correspond in form and angles.

6. *Homœomorphism of Romeine, Idocrase, Cerasine, Chiolite, Braunite.*

The species have no basal cleavage.

Romeine, $(\dot{R}^3, \bar{S}b)\ddot{\bar{S}}b$	$O : 1 = 124^\circ\ 05' - 124^\circ\ 35'$
Idocrase, $\dot{R}^3\ \bar{S}i + \bar{R}\ \bar{S}i$	$O : 1(2) = 123^\circ\ 27'$
Cerasine, Pb Cl+$\dot{P}b\ \ddot{C}$	$O : 1 = 123^\circ\ 06'$
Chiolite, 3Na F+2Al^3 F^3	$O : 1 = 123^\circ\ 17'$
Braunite, $\bar{M}n$*	$O : 1 = 125^\circ\ 40'$

The plane in Idocrase here taken as 1 is usually made the plane 2.

The *Scheelite* series may be related both to the Apophyllite and Idocrase series. In Scheelite ($\dot{C}a\ \ddot{W}$), $O: 1i = 123^\circ\ 59'$; in Scheeletine ($\dot{P}b\ \ddot{W}$), $122^\circ\ 33'$; in Wulfenite ($\dot{P}b\ \ddot{M}o$), $122^\circ\ 26'$; in Fergusonite ($(\dot{Y}, \dot{C}e)_6\ \ddot{T}a$), $124^\circ\ 20'$. But if the planes $1i$ are not homologous with 1 in Apophyllite, and the planes usually given as 1 should be retained so, then the angles $O: 1$ are between 114° and 116°. In Scheelite, both 1 and $1i$ correspond to cleavages, and the former is most distinct, while the latter is a plane of twin-composition.

7. *Homœomorphism of Sylvanite, Mispickel.*

Sylvanite, (Ag, Au)Te^3	$I = 110^\circ\ 48'$ (B. & M.)	$O : \frac{1}{2}\bar{\imath} = 148^\circ\ 34'$
Mispickel, Fe (S, As)^2	$I = 111^\circ\ 53'$	$O : \frac{1}{2}\bar{\imath} = 148^\circ\ 56'$

* Volger in his Studien zur Entwickelungsgeschicte der Mineralien, Zurich, 1854, p. 421, in an important chapter on manganese ores, makes Braunite an altered mineral with the original composition $\ddot{M}n$, on the ground of a supposed homœomorphism with tin ore ($\ddot{S}n$), and rutile ($\ddot{T}i$). The angles however are rather remote for such a conclusion. They are much nearer those of idocrase. We have evidence in the dimorphism of idocrase and garnet in connection with the dimorphism of some of the elements and the protoxyds and peroxyds, that a monometric form, a rhombohedral of 85° to 88°, and a dimetric with the form of idocrase, may be the three forms of a trimorph, and hence it seems possible that a peroxyd ($\bar{M}n$) may have an idocrase form. Idocrase, as explained beyond, may be viewed as consisting of peroxyds $\dot{R}^3$, $\bar{R}$, $\bar{S}i$, each containing 3 of oxygen.

Hausmannite is supposed by Volger (p. 414) to be homologous in composition with anatase; and *Polianite* (p. 403) which approaches Göthite in its angles and affords on analysis the composition of Pyrolusite, $\ddot{M}n$, is supposed to be an altered $\bar{M}n\ \dot{H}$, rendered anhydrous by the alteration. *Manganite*, which is generally considered $\bar{M}n\ \dot{H}$, is regarded by him as probably $\bar{M}n\ \dot{H}^3$ and homœomorphous with anthosiderite ($\bar{F}e\ \dot{H}^3$).

The fact is undoubted that most of the manganese ores are liable to alterations by oxydation and gaining or losing water, so that the analyses which are yet few in number, may not give the true composition of the original species.

Volger also makes *Brookite* homœomorphous with *Göthite* (loc. cit, p. 483 and 503). But the resemblance is not close. In Brookite $I : I = 99^\circ\ 50' - 100^\circ\ 30'$, and $O : \frac{3}{2} = 147^\circ\ 48'$; in Göthite $I : I = 94^\circ\ 52' - 95^\circ$, and $O : 1\bar{\imath} = 148^\circ\ 48'$. He infers, from the alleged approximation, that Brookite is a hydrated oxyd, and therefore $\ddot{T}i$ is *not trimorphous*. Sagenite, or acicular rutile in Quartz, he endeavors to make out either Brookite or pseudomorphs of Rutile after Brookite. Brookite is closely homœomorphous with Columbite, as shown by the writer in this volume, page 86.

8. *Homœomorphism of Andalusite, Topaz, Staurotide.*

I : I in Staurotide is 128° 42′, in Topaz 124° 19′, and in Andalusite (taking $i\bar{2}$ as I), 127° 28′. Compared with Topaz, the plane $1\bar{i}$ ($P\bar{\infty}$) of Staurotide is $\frac{3}{2}\bar{i}$, and the plane $1\bar{i}$ in Andalusite is $\frac{4}{3}\bar{i}$. The axes in this view are as follows:

Topaz, ($\ddot{A}l^3\ \dddot{S}i^2$, F replacing some O)	$a:b:c=0{\cdot}8989:1:1{\cdot}8931$
Andalusite, ($\ddot{A}l^3\ \dddot{S}i^2$)	$a:b:c=0{\cdot}9487:1:2{\cdot}0263$
Staurotide, ($\ddot{A}l^2\ \dddot{S}i$)	$a:b:c=0{\cdot}9652:1:2{\cdot}0825$

The species *Lievrite* has some relations to this group: the prism I, referred to the above type, would be $i\bar{\tfrac{4}{3}}$.

9. *Discrasite, Witherite, Aragonite, etc.*

Discrasite, Ag^2 Sb	I=119° 59′	O : $1\bar{i}$=130° 41′
Witherite, $\dot{B}a\ \ddot{C}$	I=118° 30′	
Aragonite, $\dot{C}a\ \ddot{C}$	I=116° 10′	O : $1\bar{i}$=130° 50′

10. *Augite and Wollastonite.*

In volume xv, [2], page 449 of this Journal, the author pointed out the fact that Wollastonite, by a change of position from that ordinarily taken, and one fully authorized, had nearly the angles of Augite, as follows:

Augite, ($\dot{M}g$, $\dot{C}a$)^3 $\dddot{S}i^2$	I : I=87° 5′	O : $2\grave{i}$=131° 17′	C=73° 59′
Wollastonite, $\dot{C}a^3\ \dddot{S}i^2$	I : I=87° 28′.	O : $2\grave{i}$=137° 48′	C=69° 48′

The main difference is in the less length and greater obliquity of the vertical axis in Wollastonite, and in the cleavage. The axes are:

Augite, - - - -	$a:b:c=0{\cdot}5412:1:0{\cdot}91346$
Wollastonite, - - -	$a:b:c=0{\cdot}4338:1:0{\cdot}89789$

It may be of some interest to observe, that the angle I : I in the Augite series is near the rhombohedral angle of the Corundum series, which varies from 84° to 88°.

11. *Pyrolusite and Göthite not closely homœomorphous.*

The angle I : I of Pyrolusite = 93° 40′, and is near the corresponding angle of Göthite and Diaspore. But O : $\frac{1}{2}\bar{i}$ in Pyrolusite is 160°, which gives 143° 57′ for O : $1\bar{i}$, and this differs 5° from the same angle in Göthite.

12. *Homœomorphism of Monazite, Lazulite.*

Monazite ($\dot{C}e$, $\dot{L}a$, $\dot{T}h$)$\dddot{P}$ (I); Wagnerite $\dot{M}g^3\dddot{P}$+MgF (II); Lazulite ($\dot{R}$+$\ddot{A}l$)$\dddot{P}$+$\dot{H}$? (III).

I. I : I = 93° 10′; C = 76° 14′; O : $1\grave{i}$ = 138° 8′ ∴ $a:b:c=0{\cdot}94715:1:1{\cdot}0265$
II. I : I = 95° 25′; C = 63° 25′; O : $1\grave{i}$ = 146° 3′ ∴ $a:b:c=0{\cdot}7401:1:0{\cdot}9831$
III. I : I = 91° 30′; C = 88° 15′; O : $1\grave{i}$ = 139° 45′ ∴ $a:b:c=0{\cdot}86904:1:1{\cdot}0260$

The inclination of the vertical axis (angle C) varies widely; and consequently O on the clinodiagonal prism $1\grave{i}$($P'\infty$) differs

much. But excepting Wagnerite, the relations between the axes are close. In Wagnerite the plane *w* (see Brooke and Miller) is taken as the basal plane O.

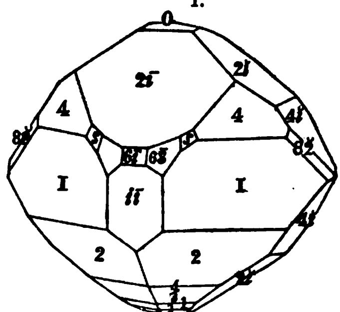
In *Triphyline* I : I = 94°; but the approximation to the others in the value of the *vertical* axis is not very close, even if the prism be oblique. The author has before shown that *Crocoisite* is homœomorphous with monazite.

The Monazite and Apatite series are *mutually dimorphous.*

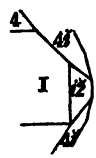
13. *Datholite and Euclase.*

The following figures of crystals of Datholite present some planes hitherto unobserved. Figs. 1 and 2 are from Roaring Brook, Connecticut, and 3 and 4 from Isle Royale, Lake Superior. Fig. 2 presents only a lateral portion of a crystal which in most respects resembles figure 1. A transparent crystal from this locality afforded for I : I, 115° 12′ giving by calculation $i\breve{2} : i\breve{2}$ 76° 28′. The plane O and all the others are highly polished, excepting sometimes $8\breve{2}$, $i\breve{2}$, and usually *s*; *s* replaces edge I : $2\bar{\imath}$; and as its intersections with $6\bar{3}$ and 4 are not parallel, nor is the intersection with $6\bar{3}$ parallel to that of $6\bar{3}$ and $6\bar{\imath}$, it is probable the plane is $\frac{16}{3}$ - $\bar{\frac{8}{5}}$. The angles of $\frac{16}{3}$ $\bar{\frac{8}{5}}$ are X = 139° 25′, Y = 57° 8′, Z = 141° 32′. The minute plane *t*, figure 4, gives

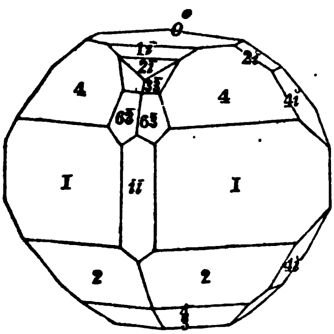
1.



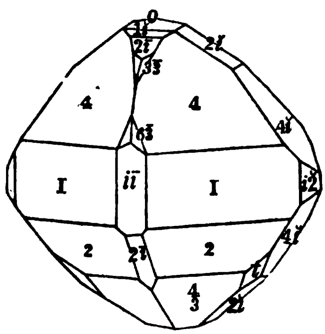
2.



3.



4.



approximately O : *t* = 140°—142°, $i\bar{\imath} : t$ = 109° 30′, whence *t* : *t* = 141°, which are near the angles of plane $2\breve{2}$; but the intersections of *t* with 2 and $2\bar{\imath}$ are not parallel, although those with $\frac{4}{3}$ and $4\breve{\imath}$ are parallel. From the last parallelism $m = 4n \div (2+n)$.

Datholite, as Brooke and Miller show, is trimetric and hemihedral, instead of monoclinic, its hemihedral character giving it a monoclinic aspect. Still it is homœomorphous with Euclase.

In Euclase, I : I = 114° 50′; and $a : b : c = 0{\cdot}4894 : 1 : 1{\cdot}477$.

In Datholite, I : I = 115° 26′(−12′); $a : b : c = 0{\cdot}5 : 1 : 1{\cdot}5829$.

In each, the axes are very nearly as 1 : 2 : 3.

The homœomorphism of *Sphene* and Euclase is shown by the author in this Journal, vol. xvi, [2], p. 96.

14. *Isodimorphism of Tourmaline and Calcite.*

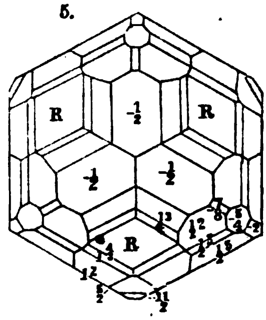
In the last number of this Journal, the author has written for the formula of Tourmaline, $(\dot{R}^3, \bar{\bar{R}}, \dddot{B})^4 \dddot{Si}^3$, (which is equivalent to $(\dot{R}^3, \bar{\bar{R}}, \dddot{B})\dddot{Si}^{\frac{3}{4}}$.)

The species Euclase is known to have the formula $(\bar{\bar{Al}}, \bar{\bar{Be}})^4 \dddot{Si}^3$.

The analogy in these formulas of Tourmaline and Euclase will be observed. And if an analogy in crystallization existed, we should have thereby good evidence that the formula of Tourmaline was right in fact and also in principle.

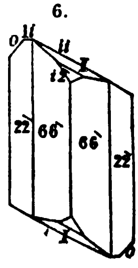
Tourmaline and Calcite are closely homœomorphous, as was sometime since suggested to the writer by Mr. T. S. Hunt. The rhombohedron 2R of Tourmaline has the angle 103°, near 105° 5′ of Calcite. The planes 2R are as highly polished as R, and sometimes more highly so; and there is no reason in cleavage or otherwise for assuming R to be the fundamental form rather than 2R: under this view the planes usually called R (giving the angle 133° 8′) become ½R, analogous to the common nail-head form in Calcite. No objection therefore exists to the homœomorphism of these species. The annexed figure presents a new form of Tourmaline, from Hunterstown, Canada East, for the privilege of studying which the writer is indebted to Mr. Hunt. It is of a rich dark-brown color and transparent, measuring two-thirds of an inch across. It is lettered to correspond with the above views, the plane usually called 2R being made R.

5.

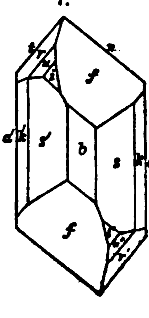


Calcite and Barytocalcite are well known to be mutual dimorphs, and the latter is monoclinic like Euclase. Moreover Euclase and Barytocalcite are approximately homœomorphous. We have copied the annexed figures of Barytocalcite (6) and Euclase (7) from Brooke and Miller, only

6.



7.



changing the lettering of the former. They represent crystals of the species, seen in profile. A resemblance is apparent at a glance; besides, $f:f$ (over edge n) of Euclase $=106°$; I : I (over $\ddot{i}$) Barytocalcite $=106° \ 54'$. In Barytocalcite I, I are cleavage planes, and so also the plane O; and making I, I the sides of the fundamental prism and O, the base, the planes are as lettered in the figure. The plane in Euclase corresponding to O of Barytocalcite, would be t. Taking t as O, and f for the comparison as I, the axes of the two species are as follows:

Euclase,	I=106°	$a:b:c$=0·83892 : 1 : 1·32674	C=106° 8′
Barytocalcite,	I=106° 54′	$a:b:c$=0·81035 : 1 : 1·29583	C=88° 46′

In this view, r, u, i are the clinodiagonal prisms $\frac{1}{3}\grave{i}$, $\frac{2}{3}\grave{i}$, $\frac{4}{3}\grave{i}$; the plane $s=\frac{4}{3}\text{-}\grave{\frac{4}{3}}$, $k=1\text{-}\frac{3}{2}$, q, a plane between s and k, mentioned by Brooke and Miller, is the plane 1, or belongs to the fundamental octahedron, having for the angle X, 128° 48′.

Remembering how Anhydrite diverges from the other sulphates $\dot{R}\dddot{S}$ in angles and *cleavage*, and Wollastonite, another lime species from Augite in the same particulars, and also noting the difference in angle between Tourmaline and Calcite, it will be admitted that the homœomorphism is close.

Since then, calcite and tourmaline are homœomorphous, and also barytocalcite and euclase, tourmaline and euclase are mutual dimorphs as well as calcite and barytocalcite; and, moreover, *Tourmaline and Calcite are isodimorphous.*

Hence the formula of Tourmaline, analogous with that of Euclase is the right one; and condensation in writing formulas is apparently the correct method, in place of the hypothetical subdivision adopted by Hermann, and others.

The Laurent School in France is obviously right in making the protoxyds and peroxyds replace one another, the parts equivalent being those having the same number of atoms of oxygen. The principle is sustained by the homœomorphism of Willemite and Phenacite, $\dot{Z}n^3 \dddot{S}i$ and $\dddot{B}e \dddot{S}i$; or if $\dddot{B}e$ be written $\dot{B}e^3$, other cases show the mutual replacement of $\dot{B}e^3$ and $\dddot{A}l$. It is exemplified in Augite and Spodumene, the former $\dot{R}^3 \dddot{S}i^2$, the latter $(\dot{R}^3, \dddot{R}) \dddot{S}i^2$; and also in many other species.

15. *Observations on the formulas and relations of some species.*

Euclase, Datholite, and Sphene.—The formulas are as follows, in accordance with the above principles.

Euclase, - - - - - -	$(\frac{1}{3}\dddot{A}l+\frac{1}{3}\dddot{B}e)\dddot{S}i^{\frac{4}{4}}=(\dddot{R})\dddot{S}i^{\frac{4}{4}}$
Datholite, adopting the above view, and making 3Ḣ replace 1Ċa as in Scheerer's theory, has the fomula,	$(\frac{1}{3}\dot{R}^3+\frac{1}{3}\dddot{B})\dddot{S}i^{\frac{4}{4}}=(\dot{R}^3,\dddot{B})\dddot{S}i^{\frac{4}{4}}$
Or if 3Ḣ replace 3Ċa, it becomes, - -	$(\frac{4}{5}\dot{R}^3+\frac{3}{5}\dddot{B})\dddot{S}i^{\frac{4}{5}}=(\dot{R}^3,\dddot{B})\dddot{S}i^{\frac{4}{5}}$
Sphene, - - - - - -	$(\dot{C}a+\ddot{T}i)\dddot{S}i^{\frac{4}{5}}=(\dddot{R})\dddot{S}i^{\frac{4}{5}}$

In Sphene there are 3 of oxygen within the brackets as in the other species. The usual formula is $2\dot{C}a\dddot{S}i+\dot{C}a\ddot{T}i^3$. But the relation in form to Euclase sustains the above mode of writing it. $(\dot{C}a+\ddot{T}i)\dddot{S}i^{\frac{2}{3}}$ is equivalent to $(\ddot{R})\dddot{S}i^{\frac{2}{3}}$, since $\dddot{R}=RO+RO^2$; so that sphene is essentially a silicate of the common form, or of $\dddot{T}i$ in which part of $\dddot{T}i$ is replaced by $\dot{C}a$. Some chemists write for the sesquioxyd Braunite $\dot{M}n+\ddot{M}n$, and others adopt the same form for other sesquioxyds, denying the existence of a proper sesquioxyd.

There are but few anhydrous silicates, in which the bases exceed the silica in oxygen. These are *Sillimanite*, *Kyanite*, *Andalusite*, *Topaz*, *Staurotide*, with *Tourmaline*, *Euclase* and *Sphene*. Andalusite, Topaz and Kyanite, have the same formula $\dddot{A}l\dddot{S}i^{\frac{2}{3}}$; in Sillimanite we have both $\dddot{A}l\dddot{S}i^{\frac{3}{4}}$, and $\dddot{A}l\dddot{S}i^{\frac{2}{3}}$; and in Staurotide, homœomorphous with topaz and andalusite, $\dddot{A}l\dddot{S}i^{\frac{1}{2}}$. Hence the ratio of silica varies in the same species from $\frac{2}{3}$ to $\frac{3}{4}$ (and perhaps to 1) and in the same homœomorphous group, from $\frac{1}{2}$ to $\frac{2}{3}$. The formulas of Euclase, Datholite and Sphene, are therefore essentially of one type. And if this be true, then $\dddot{R}\dddot{S}i^{\frac{2}{3}}$ (under which we include $(\dot{R}^3.\dddot{R}$ or $\dddot{B})\dddot{S}i^{\frac{2}{3}}$) is *trimorphous*. (1.) *Triclinic* in Kyanite and Sillimanite; (2.) *monoclinic* in Sphene, etc.; (3.) *trimetric* in Andalusite, Topaz. G. Rose considers Kyanite and Sillimanite distinct in form. But the angles of Sillimanite observed are too doubtful to enable us to decide upon this point.

Beryl and Eudialyte.—These species are shown to be closely homœomorphous on page 211.

The formula of Beryl is $(\frac{1}{2}\dddot{B}e+\frac{1}{2}\dddot{A}l)\dddot{S}i^2$; that of Eudialyte, $(\frac{2}{3}\dot{R}^3+\frac{1}{3}\dddot{Z}r)\dddot{S}i^2$. We also add that Pyrosmalite and Dioptase have the ratio 1 : 2, if the water and also the chlorid in the former be excluded.

Groups of Anhydrous Silicates among minerals.—Making the ratio between the oxygen of all the bases, and that of the silica of fundamental value, the anhydrous silicates among minerals mostly fall into five groups, presenting the ratios 1 : 3, 1 : 2, 1 : 1½, 1 : 1, 1 : ($\frac{2}{3}$ – $\frac{1}{2}$ – $\frac{3}{4}$.)

I. Ratio 1 : 3. This includes Edelforsite, $\dot{C}a\dddot{S}i$, and Mancinite, $\dot{Z}n\dddot{S}i$, both species of somewhat doubtful existence.

II. Ratio 1 : 2. Includes Wollastonite, Augite, Spodumene, Wichtyne, Beryl, Eudialyte.

III. Ratio 1 : 1½. Includes Eulytine, $\dddot{B}i^2\dddot{S}i^3$.

IV. Ratio 1 : 1. Includes—

1. *Trimetric.*	Chrysolite, etc.	$\dot{R}^3\dddot{S}i$	Tephroite,	$\dot{M}n^3\dddot{S}i$
2. *Hexagonal.*	Phenacite,	$\dddot{B}e\dddot{S}i$	Willemite,	$\dot{Z}n^3\dddot{S}i$

3. *Monometric.*	Garnet,	$(\frac{1}{3}\dot{R}^3+\frac{1}{3}\dddot{\bar{R}})\dddot{\text{Si}}$	Helvin,	$(\frac{2}{3}\dot{R}^3+\frac{1}{3}\dot{\text{Be}})\dddot{\text{Si}}$*
	Pyrope,	$(\frac{1}{4}\dot{R}^3+\frac{3}{4}\dddot{\bar{R}})\dddot{\text{Si}}$		in which $\dot{R}=\dot{\text{Mn}}, \dot{\text{Fe}}, \text{Mn S}$.
4. *Dimetric.*	Zircon,	$\ddot{\text{Zr}}\,\dddot{\text{Si}}$	Meionite,	$(\frac{1}{3}\dot{\text{Ca}}^3+\frac{2}{3}\dddot{\bar{\text{Al}}})\dddot{\text{Si}}$
	Idocrase,	$(\frac{1}{2}\dot{R}^3+\frac{1}{2}\dddot{\bar{R}})\dddot{\text{Si}}$	Scapolite,	$(\frac{1}{4}\dot{R}^3+\frac{3}{4}\dddot{\bar{\text{Al}}})\dddot{\text{Si}}$
	"	$(\frac{3}{5}\dot{R}^3+\frac{2}{5}\dddot{\bar{R}})\dddot{\text{Si}}$	Mellilite,	$(\frac{2}{3}\dot{R}^3+\frac{1}{3}\dddot{\bar{R}})\dddot{\text{Si}}$
	Sarcolite,	$(\frac{1}{2}\dot{R}^3+\frac{1}{2}\dddot{\bar{\text{Al}}})\dddot{\text{Si}}$		
5. *Monoclinic.*	Epidote,	$(\frac{1}{3}\dot{R}^3+\frac{2}{3}\dddot{\bar{R}})\dddot{\text{Si}}$	Zoisite,	$(\frac{1}{4}\dot{R}^3+\frac{3}{4}\dddot{\bar{R}})\dddot{\text{Si}}$
	"	$(\frac{3}{5}\dot{R}^3+\frac{2}{5}\dddot{\bar{R}})\dddot{\text{Si}}$	Gadolinite,	$\dot{R}^3\,\dddot{\text{Si}}$
	Allanite,	$(\frac{1}{2}\dot{R}^2+\frac{1}{2}\dddot{\bar{R}})\dddot{\text{Si}}$		
6. *Triclinic.*	Danburite,	$(\frac{1}{4}\dot{\text{Ca}}^3+\frac{3}{4}\dddot{\text{B}})\dddot{\text{Si}}$	Axinite,	$(\dot{R}^3, \dddot{\bar{R}}, \dddot{\text{B}})\dddot{\text{Si}}$

V. Ratio $1:\frac{2}{3}$(to $\frac{3}{4}-\frac{1}{2}$). Includes—*Triclinic*, Kyanite, Sillimanite; *Monoclinic*, Euclase and Sphene; *Trimetric*, Andalusite, Topaz, Staurotide, and perhaps Lievrite; *Dimetric*, Gehlenite; *Hexagonal*, Tourmaline.

From the Formulas of Datholite, Tourmaline, Axinite, Danburite, it follows that these are not *borosilicates*, the boracic acid being a *base*. It is a general fact that all mineral species containing boracic acid are either hemihedral or oblique in crystallization.

The *Feldspars* and some other species do not appear at first to come into this system, unless the alumina and silica are considered as replacing one another, and on this ground, any ratio between 1:1 and 1:3 may be made out. The constancy of the oxygen ratio of $\dot{R}$ to $\dddot{\bar{R}}$ (=1:3) in the feldspars, seems to preclude our taking such a liberty with the $\dddot{\bar{\text{Al}}}$ and $\dddot{\text{Si}}$. The following are the species, their oxygen ratios, old formulas, and the formulas proposed.

		Oxygen ratio.	Old formula.	Formulas.
1. *Monoclinic.*	Orthoclase,	1:3:12	$\dot{K}\,\dddot{\text{Si}}+\dddot{\bar{\text{Al}}}\,\dddot{\text{Si}}^3$	$(\dot{K}+\dddot{\bar{\text{Al}}})\dddot{\text{Si}}^4$
2. *Triclinic.*	Albite,	1:3:12	$\dot{\text{Na}}\,\dddot{\text{Si}}+\dddot{\bar{\text{Al}}}\,\dddot{\text{Si}}^3$	$(\dot{\text{Na}}+\dddot{\bar{\text{Al}}})\dddot{\text{Si}}^4$
	Oligoclase,	1:3: 9	$\dot{R}\,\dddot{\text{Si}}+\dddot{\bar{\text{Al}}}\,\dddot{\text{Si}}^2$	$(\dot{R}+\dddot{\bar{\text{Al}}})\dddot{\text{Si}}^3$
	Andesine,	1:3: 8	$\dot{R}^3\,\dddot{\text{Si}}^2+3\dddot{\bar{\text{Al}}}\,\dddot{\text{Si}}^2$	$(\dot{R}+\dddot{\bar{\text{Al}}})\dddot{\text{Si}}^{\frac{8}{3}}$
	Labradorite,	1:3: 6	$\dot{R}\,\dddot{\text{Si}}+\dddot{\bar{\text{Al}}}\,\dddot{\text{Si}}$	$(\dot{R}+\dddot{\bar{\text{Al}}})\dddot{\text{Si}}^2$
	Anorthite,	1:3: 4	$\dot{R}^3\,\dddot{\text{Si}}+3\dddot{\bar{\text{Al}}}\,\dddot{\text{Si}}$	$(\dot{R}+\dddot{\bar{\text{Al}}})\dddot{\text{Si}}^{\frac{4}{3}}$

The following species also belong to the section, as they have the same ratio 1:3 for the protoxyds and peroxyds; Leucite has the composition of andesite; and sodalite, etc., that of anorthite.

* The formula of Helvin, as written by G. Rose, is $(\dot{\text{Mn}}, \dot{\text{Fe}})^8\,\dddot{\text{Si}}^2+\dddot{\bar{\text{Be}}}\,\dddot{\text{Si}}+\text{MnS MnO}$. But we conform as closely to the analyses, if we add $\frac{1}{2}$MnO$+\frac{1}{2}$MnS, when it becomes as above written, or $(\frac{2}{3}(\frac{4}{5}\dot{R}+\frac{1}{5}\text{MnS})^3+\frac{1}{3}\dddot{\bar{\text{Be}}})\dddot{\text{Si}}$ = Silica 34·5, glucina 9·4, protoxyd of manganese (part Fe in the analysis) 39·8, sulphuret of manganese 16·3.

		Oxygen ratio.	Old formula.	Formulas.
8. *Monometric.*	Leucite,	1:3:8	$\dot{K}^3\dddot{Si}^2+3\dddot{\bar{Al}}\dddot{Si}^2$	$(\dot{K}+\dddot{\bar{Al}})\dddot{Si}^{\frac{8}{3}}$
	Sodalite,	1:3:4	$\dot{Na}^3\dddot{Si}+3\dddot{\bar{Al}}\dddot{Si}+Na\,Cl$	$(\dot{Na}+\dddot{\bar{Al}})\dddot{Si}^{\frac{4}{3}}[+\frac{1}{3}Na\,Cl$
	Hauyne,	1:3:4	$\dot{Na}^3\dddot{Si}+3\dddot{\bar{A}}\dddot{Si}+2\dot{Ca}\dddot{S}$	$(\dot{Na}+\dddot{\bar{Al}})\dddot{Si}^{\frac{4}{3}}[+\frac{2}{3}\dot{Ca}\dddot{S}$
	Nosean,	1:3:4	$\dot{Na}^3\dddot{Si}+3\dddot{\bar{Al}}\dddot{Si}+\dot{Na}\dddot{S}$	$(\dot{Na}+\dddot{\bar{Al}})\dddot{Si}^{\frac{4}{3}}[+\frac{1}{3}\dot{Na}\dddot{S}$
	Lazulite.			
4. *Dimetric.*	Nepheline,	1:3:4	$\dot{R}^2\dddot{Si}+2\dddot{\bar{Al}}\dddot{Si}$	$(\dot{Na}+\dddot{\bar{Al}})\dddot{Si}^{\frac{3}{2}}$
	Cancrinite,	1:3:4½	$\dot{R}^2\dddot{Si}+2\dddot{\bar{Al}}\dddot{Si}+\dot{R}\ddot{C}$	$(\dot{Na}+\dddot{\bar{Al}})\dddot{Si}^{\frac{3}{2}}[+\frac{1}{2}\dot{R}\ddot{C}$

The formulas of the feldspars in the last column, show what is equally plain in the oxygen ratios, that the species differ in the amount of silica, and this is the great essential difference. It is least in anorthite; and from this species it increases to $4\dddot{Si}$ in albite or orthoclase. This increase takes place without any change of crystallization, there being only very small variations in the angles. In anorthite, the oxygen ratio for the bases and silica is 1 : 1; and as anorthite is common in good crystallizations, and is every way a well characterized species, it shows us that 1 of oxygen of the silica to 1 for the bases is all that the *feldspar type* requires. Moreover, the relation of the species to Scapolite, which has the ratio 1 : 1, also favors this view. Hence 1 : 1 may be considered the *type-ratio*, upon which, variations take place according to definite proportions. When silica abounds in the rock material in process of crystallization, and the other ingredients are at hand, the species holding the largest proportions of silica would be formed.

The isomorphism of Sodalite, Hauyne, and Nosean, and dimorphism with Anorthite, parallel with the dimorphism of Leucite and Andesine, show that the ratio 1 : 3 : 4, is their type ratio. The very unlike substances $Na\,Cl$, $\dot{Ca}$, $\dddot{S}$, $\dot{Na}\dddot{S}$, are added without modifying the form, and although chemically included, are unessential to the type. The same is true of $\dot{R}\ddot{C}$ in Cancrinite, which has the crystallization of Nepheline.

The facts above exhibited appear to show that a type admits of some variation in the amount of silica without changing the character of the species. In Meionite, having the ratio of 1 : 2 : 3, the scapolite type is exhibited; and Bischof and Rose take this as the only ratio of the species. But the ratio 1 : 2 : 4 appears to be required by most analyses of Scapolite, in which there is an addition to the silica. In the same manner $\dot{R}^3\dddot{Si}^2$, with an addition of $\frac{1}{3}\dddot{Si}$, becomes Hornblende.*

Among the feldspars, Andesine and Leucite have essentially the oxygen ratio of Augite, 1+3 : 8 = 1 : 2, and the formula might

* The amount of silica present may be one cause leading to the formation of Hornblende in place of Augite. But in pseudomorphic changes, the same proportions may result, by a removal of part of the bases, or an addition of magnesia as Blum and Bischof have suggested.

be written $(\frac{1}{4}\dot{R}^3+\frac{3}{4}\ddot{R})\dddot{Si}^3$. Moreover, oligoclase has similarly the oxygen ratio of Hornblende $= (\frac{1}{4}\dot{R}^3+\frac{3}{4}\ddot{R})\dddot{Si}^{\frac{9}{4}}$. Hence we may look upon Leucite, and Andesine, with Pyroxene, as in a certain sense trimorphous. Still, their relation to the feldspar series is such that they are naturally classed with the other feldspars.

The zeolites, if the water be excluded, have the oxygen ratios of the feldspar-section, as shown in the following table; the oxygen of the water in the zeolites is annexed to the name of the species:—

Oxygen ratio. $\dot{R}$ $\ddot{R}$ $\dddot{Si}$	*Feldspars.*	*Zeolites.*
1 : 3 : 4	Sodalite, Anorthite,	Ittnerite (2), Thomsonite ($2\frac{1}{2}$).
1 : 3 : $4\frac{1}{2}$	Nepheline,	Zeagonite ($4\frac{1}{2}$).
1 : 3 : 6	Labradorite,	Levyne (4), Natrolite (2), Scolecite (3).
1 : 3 : 8	Leucite, Andesine,	Analcime (2), Chabazite (6 or 5). Philippsite (5). Laumonite (4).
1 : 3 : 9	Oligoclase,	Harmotome (5), Chabazite (6).
1 : 3 : 12	Orthoclase, Albite,	Heulandite (5), Brewsterite (5). Stilbite (6 or 5), Epistilbite (5).

Some of the species are correspondingly isomorphous with feldspar species, as Analcime with Leucite, Ittnerite with Sodalite; and the ratio 1 : 3 : 12 produces oblique forms in both series. But we do not intend to draw a general parallelism, as the water whatever its relations, must in some cases modify the ratios. But as regards the origin of the species, the table is an interesting one. Bischof remarks on the identity in the ratio between the oxygen of the bases and silica in chabazite and that of Hornblende, and thereby explains the occurrence of pseudomorphs of chabazite after hornblende.

Pyrrhotine (Fe^7S^8) *and Greenockite* (Cd S).—As Pyrrhotine and Greenockite are homœomorphous, they are naturally arranged in the same group, although the former has a little too much sulphur. The formula $5Fe\,S+Fe^2\,S^3$, may perhaps be written $Fe\,S\,[+\frac{1}{5}Fe^2\,S^3]$, the latter term being unessential to the type.

Art. XXII.—*On Microscopes with large Angles of Aperture;* by Dr. E. D. North.

We have no fuller or more careful statement of the mode in which an enlarged aperture increases the efficiency of a Microscope than that of Mr. Pritchard in his "Microscopic Illustrations," which is copied by Mr. Quekett. After explaining that since the whole diameter of the front lens receives a pencil of rays from each minutest point of the object, and that, consequently, when these pencils from each point are large, more light is received from the points separately as well as from the entire object, he adds:—

"But it may perhaps be supposed from this reasoning, that if we throw a greater quantity of light upon an object, so that more may be collected by the object-glass, we shall be better able to define a structure; which would probably be the case if the additional light could be thrown only upon those minute parts of the object which we wish to examine, and not upon the whole object. But as we cannot do this, as the increase of illumination cannot be made to increase the *relative* proportions of light which proceed from those minute parts, the intended advantage will not be derived."

This paragraph involves implications directly opposite to some of the most important facts in regard to using Microscopes successfully. The second sentence says in effect, that when, with an object-glass of small aperture, and in a faint light, we discover a certain degree of minuteness of structure, we shall, under a strong illumination, discover no more, which is directly contrary to the fact, unless the objective is miserably deficient in correction. Indeed, a most important quality of first rate objectives, is their ability to bear a strong illumination.

It is also taken for granted that an increase of the *relative* proportions of light upon minute striæ and other markings, would enable us to see them better. On the contrary, the true requisite is that the objective shall be so perfectly corrected as to *preserve* the relative light, shade, and variations of color on adjoining minute portions, thus exhibiting the object precisely as it would appear were its smallest parts *large enough* to be visible to the naked eye. If some parts could be illuminated more than others, a false instead of a true appearance would be the result. By destroying the natural shading, we should get the appearance of a different object.

The *limit* of beneficial intensity of light depends rather on physiological than on optical conditions, being different with different individuals; one person's eye being pained and dazzled, when another's is aided. The direct light of the sun enables the aged to read without spectacles. It will be well, however, to notice that, although intrinsic brightness may be the same and very intense, yet if all the light entering the pupil comes from a minute surface, the eye is less affected on the whole. Place a card with a pin-hole at each end of a tube, and hold the latter near the eye, and we can gaze steadily at the small portion of the sun thus visible. But in general, the dazzling and blinding effect of intense light is owing to contrast, as when one comes from a dark room into sunshine. The contrast may be of surrounding objects, as in viewing in a darkened room the light of a galvanic battery.* In reference to extreme intensity of illumination, the

* By means of this contrast of light on a black ground, fine displays may sometimes be made of lines on test objects: thus, by using sunlight with Mr. Wenham's admirable condenser, the transverse lines of the Grammatophora subtilissima, (Greenport, in balsam,) are shown at regular intervals and sharply defined, the objective being Smith & Beck's $\frac{1}{16}$, of largest angle.

following observation is most important. Artists, such as painters, engravers, sculptors, never employ a strong light on their work. The most important discriminations for them, are delicate gradations of color, of shadows, or of the mingling of the two. Their eyes, according to our observation, are by natural constitution very easily pained and dazzled. The finest artistic effects are possible only in a subdued light. An able writer on painting, remarks: "It is the property of (strong) light to convert objects into its own whiteness, and to take away color." Dimness of light and indefiniteness of outline, assist an active imagination. Faint illumination in the microscope may gratify an artist's eye by making the representation more pictorial, and especially by assisting the imagination in regard to depth. Such pictorial effects are suited to the popular eye, and it is allowable for the most accurate and reliable observer to gratify himself and friends with them as exhibitions of a peculiar kind. The object of the common Stereoscope, is to produce a pictorial, rather than a geometric impression on the mind: yet the brief reports as yet published respecting stereoscopic vision applied to the microscope, have not often discriminated these two kinds of effect.

The most minute objects now examined under the microscope, are colorless and transparent, and consequently do not need a diminished illumination for bringing out delicate variations of color, and, as is the case with the naked eye, it may be said in general that the more the light, the more distinctly will the minutest portions cast a shadow or reveal themselves by their refractive and dispersive effect, up to that degree of general brightness of the image which a given eye can bear.

But what constitutes the *extremest minuteness* in regard to visibility? Simple and obvious as the answer is to this question, yet writers on the microscope speak of an increased angle as if it could in all cases compensate for a higher magnifying power.

As is the case with the telescope, or with the naked eye, an area is visible when it subtends an angle at the eye of at least "more than half a minute of a degree." Or, it may "be assumed that one minute of angle is a good general measure for the *visibility* of areas"—"therefore, that areas are visible at a distance of about 3,000 or 4,000 times as great as their diameter." "But though such a spot can *be seen*, it cannot be *defined* as square, circular, &c." "To be thus clearly defined to the naked eye, black spots on a white ground, must have a diameter of about $\frac{1}{1500}$ of radius." But black stripes can be separated when areas cannot be defined, and the whole is greatly dependent on illumination. As no two persons' eyes have precisely the same power of minute vision, the limitation in question must be to some extent indefinite; it deserves, however, and will doubtless receive further investigation.

Yet it is important to keep in mind that there *is* a limit which no illumination or enlargement of aperture will overcome without increase of magnifying power. It may lead to serious error to be accustomed to strain the eye with a low eye-piece or objective, when a more extended amplification of image ought to be employed for more decidedly separating lines or points. M. Robin argues for employing high powers in anatomical investigations, with an earnestness which proves a then prevalent error. Probably such mistakes have contributed to establish an impression of there being often a danger of using too much light. When endeavoring to look into minute structure with a low or medium power, when a high one is what is needed, one will unconsciously strain and prolong his attention, until, if the light be strong, he brings on confusion, dazzling and pain, which a weaker light may alleviate, and at the same time be sufficient for what the given amplification can reveal. As long as an increase of light assists the mind it can be borne by the eye, as is indeed proved by the present fashion of employing sun-light. Not only high powers, but well corrected medium ones, and even rather low ones, may thus be used, and that too with no diminution of light by obliquity. Fatigue and dazzling of the eye result also from aberration, as we shall presently show.

It may be well to mention a curious effect which is quite liable to result from attempting to accomplish too much with a given objective. When an object-glass is thus *overstrained*, a test may exhibit a set of lines, or even two sets, crossing each other, and forming dots, which are not the true lines, *because two or three of them are united into one.* The case is not similar to the *doubling* of lines, which is so often noticed. Although not well defined, those now in question are seen *in focus*, and although thickened and badly defined, are yet plainly visible and easy to count in micrometrically measuring their distances. Among other experiments in proof, the following is selected: The ribs of Pinnularia major were viewed with a low objective and intense lamp light, (sunlight is still better,) at an obliquity, very near the degree which produces a dark back-ground. The ribs were strongly visible, though not entirely across the semi-breadth of the valve. Being easily counted, the micrometer gave their distances $\frac{1}{8000}$ and $\frac{1}{6000}$ of an inch in different circumstances, with sufficient accuracy for the experiment. But when with the same light and illumination, a $\frac{1}{4}$th objective was employed, the ribs were visible in their whole length, and with a perfect definition. Their distance was, in the specimens examined, beyond all room for doubt, found to be very accurately equal to only $\frac{1}{14000}$ inch. Two acute microscopists and practiced observers, one of them an able microscope maker, witnessed, and themselves repeated this demonstration, and confirmed its reliability. The experiment

will be found instructive and also easy by microscopists in general. A friend also mentions that he had noticed the same optical effect; and with sun-light had shown to two persons, one being a distinguished microscopist, that even when a power so low as one inch (a remarkably good one, however, by Mr. Grunow) was employed on the Navicula Angulata, lines of the *true* obliquity in reference to the sides were brought out, although so far apart and few in number, as manifestly to consist of but one in a space which really contains as many as three or four. The finest scales of the Lepisma Saccharina, with an objective of 1½ inch, or probably the coarser scales with a power even considerably lower, are convenient for this purpose. Undoubtedly, tests of very close markings are often supposed to be resolved, when in fact only half the number of lines is seen, and these thickened, flattened, and badly defined. We do not see how the performance of an objective can be stated scientifically, except on Nobert's lines or when it is confirmed by the micrometer.

It is obvious that as the *image* of a faint star in the telescope, so is an indivisible point or line in the image formed by the objective of a microscope. In both instruments the necessary condition of visibility is that light enough come from a point in the image to stimulate the optic nerve sufficiently. Hence when the desideratum is not to *separate two points*, but to perceive the existence of a solitary one, to magnify in a higher degree without at the same time admitting more light, may cause the point to vanish from sight. Low eye-pieces are appropriate for *discovering* faint markings, higher ones for separating those whose obscurity comes from being extremely near to each other.*

The demand for light in using high magnifying powers is enormous. A minute anatomist when dissecting under a low magnifier, or with the unaided eye, illuminates a tissue with as much light as possible, often employing two condensers of artificial light, yet as the brightness of an image is diminished in inverse proportion to the square of the linear magnifying power, an ultimate fibrilla of voluntary muscle, when magnified 1000 diameters (and as much or more amplification is needed), would have only $\frac{1}{1,000,000}$th part the light on its image, were it not for the large aperture of the objective.

With a microscope just as with a single eye, we judge of depth and form, by means of geometrical foreshortening and infinitely delicate gradations of light, shade, and sometimes color; assisting our conclusions by change of focus and directing attention to different parts and presentations. Nothing shows more strikingly

* The President of the London Microscopical Society remarks that although Mr. Wenham thought the apertures which he had gained were of no utility beyond 150°, yet to himself they seemed to have greater power of discovering or indicating the existence of lines, though they were insufficient for defining them. In this remark the same principle is recognized.

the perfection of the human eye than this power of looking at points in the plane of the exact focal distance, and yet at the same time receiving impressions *appropriately indistinct* from points within or beyond the focus. It is the same, though in less pefection, with a well corrected object glass of large angle; in proportion to the length of its focus, do points beyond the focal distance form *suitable* images. But upon this requisite more will be said presently. The power of exhibiting minute variations of light and shade thus being essential in reference to solid form and also being that chiefly by which lines are separated on a plane surface, becomes obviously the most valuable quality of an objective.

No object glass can be wholly freed from the two kinds of aberration. Of these the spheroidal is the most difficult to correct, particularly when the residuum of error is to be magnified by an eye-piece.

Mr. Pritchard should therefore have explained that when an objective has a small aperture, and is at same time not well corrected, the more light is sent through it by increasing the illumination, the greater is the amount of uncorrected aberration, which has its confusing effect subsequently magnified with the rest of the image. But if additional brightness of the image and greater amount of light entering the eye is gained by enlarging the angular aperture, the lens must be corrected for this aperture or no definition is gained. The benefit from large aperture, then is as before, simply that of an increase of light. If the spherical and chromatic aberrations could be *perfectly* corrected, a stronger illumination would assist an objective of small aperture, precisely as it does the naked eye, up to a surprising degree of intensity, while injurious extremes would be farther removed by success in discrimination, and the pleasure of the exhibition.

But images are formed more by the marginal than the central rays, as these are the most numerous; yet as spherical aberration increases in proportion as rays are further from the center, the outer ones are most difficult of correction; and the residuum of error increases from the center towards the margin. Objectives with large apertures, therefore need to be more skillfully corrected than those with small ones, or they cannot be used at all; a fact which should be called to mind by any one disposed to sneer at Mr. Spencer in regard to his enormous apertures. It is this better correction needed for large angles which explains the peculiarity of the most perfect object glasses being as much superior for a stronger as for a weaker illumination. Dr. Goring reported that only the coarsest tests could be resolved by sun-light; at present some rely upon it solely for those of extreme difficulty.*

* A striking illustration of the effect of large apertures, and of that degree of correction which is indispensable, was afforded by the surprise which some micro-

The following experiment is interesting:

Let any one who has an objective with a large angle, try it in a room nearly dark, no light being had except from a small lamp or a candle in an adjoining one. Let the rays from the distant candle shine on the stage of the microscope, which yet receives so little light that with the naked eye the smaller letters cannot be read upon an engraved bank bill which is laid on the stage, and receives no condensation of light from a lens or a concave mirror. Then looking through the microscope at the bill as an opake object, he will not only be able to read the characters, but distinguish the finest lines of the engraving and make out the texture of the paper. Employing transmitted light and the concave mirror, he may determine the fibres, whether cotton or linen, of which the paper is composed, and even do this, with some objectives, when the amplification is the excessive one of 2000 diameters. So, also, we may examine as an opake object, a scale of a fish, the wing of a moth, a feather, a leaf or a petal, by diffused daylight, without Lieberkuhn or other condensing apparatus, and, under a low magnifying power, we shall perceive, not only more clearly than with a hand lens, but also more plainly than with the naked eye, those coarser details which need either no amplification or but a slight one. The instrument has the same effect as if it not only magnified, but illuminated the object, and with a light surprisingly clear, uniform, and pleasant. It is not useless to mention such facts, for there are probably many who do not know, and little suspect the perfection of the best instruments as now made, and their superiority not only in pleasantness, but trustworthiness over the single microscope.

Large aperture being so important, it has become customary simply to mention its extent in commending an objective. Dr. Goring seldom mentioned the magnifying power of an object glass needed for a particular test; but in regard to the tests now most used, which require to be magnified so much more, no accurate account can be given except such as specifies the degrees of the two qualities, aperture and magnifying power, which are needed for their resolution. Indeed, expansion and definition sufficient for counting with a micrometer, is the only standard which can enable one investigator to compare his observations with those of another.

Of late, the superior correction which opticians can accomplish on medium powers, makes them more efficient than higher ones even on tests for which the greater expansion of image of the latter is an important advantage. A smaller angle now will often accomplish more than one larger by a half. The anticipation of

scopists felt, on reading the report of the Jury of the Great Exhibition, pronouncing the objectives of M. Nachet to be *uncorrected* for *spherical* aberration. But this artist's angles fall much short of Mr. Spencer's. Persons accustomed to the effect of refined spherical correction were less surprised.

Mr. Pritchard* that large apertures by permitting a longer conjugate focus, may ultimately enable all organized structure to be investigated with ¼ inch glasses, has been fulfilled sooner than he probably could have anticipated. Objectives of this grade now resolve every known test.

Of the two unavoidable remainders of error, the spherical becomes the most important for farther reduction; we thus gain not only in what is called definition, that is fineness and delicacy of lines, but in light and shade, in depth, and even in light. Take a ¼ inch object-glass of very fine definition, but an aperture no larger than reported by Mr. Quekett (between 60° and 70°) and one of the Naviculæ whose lines it will resolve, though not too readily—one with diagonal lines being best for this purpose. Adjust the screw collar for an *uncovered* object, and let the cover on the slider be a thick one, or interpose another piece of thin glass; better also for the object to be mounted in balsam. Turn down the wick of a lamp and remove the light till there is barely light enough to see the lines yet only on a part of the valve; fixing the eye on a portion of the surface where the lines seem to be hidden by a dark shade, adjust the front lens for the thickness of cover, and this shade will disappear; the lines will come out and the whole object *appear* better illuminated. Compare this effect of correcting the spherical aberration by the screw collar, with that of suffering it to remain uncorrected, and bringing out the additional lines by bringing the lamp nearer. The two effects may be made the same.

An objective may bring out a single set of lines on a Diatomaceous test, especially if sun-light be employed, in a strong and bold manner (though thickened and perhaps highly prismatic) when its deficiency of spherical correction shows them through a mist and does not show a perspective definition of general form. Among the Diatomaceæ, the genus Nitschia was established by Hassall who yet makes no mention of its most distinctive characteristic, the prominent keel, which strikingly distinguishes it from Synedra *(Exilaria)*, as is shown by the Rev. Wm. Smith: yet the founder of the genus, though he expressly notices the connection of the two genera, failed to point out this mark of difference which at once attracts the attention if a first-rate objective be used.

If angular aperture be *unnecessarily* large, we have the disadvantage of a useless or injurious limitation of focus—even supposing the *working distance* to be not too inconvenient. Taking the semi-diameter of front lens as radius, the working distance is the tangent of the inclination of the extreme rays: when this inclination, as in large apertures, is small, the tangent varies more nearly as the angle, and any error of focus causes the outer rays

* Micrographia, Essay on Solar Microscopes, 1839.

to have more aberration. View of depth is therefore necessarily more limited. Still, for extremely slight elevations and depressions, and for very thin objects, some restriction will afford a more exquisite discrimination, provided the spherical correction is carried to a degree of perfection in due proportion to the angle. Indeed, whatever tends to bring the performance of an object-glass nearer to mathematical exactness, must limit its focus. Stop off aperture and we see farther in depth, because we see more indistinctly in regard to minute details. Again, we may gain the same advantage by so setting the screw collar as to increase the spherical aberration. Since the longer the focus of a glass, the more power will it have over depth, the ordinary resource is, to employ an objective of lower magnifying power. By whatever means the image is enlarged, the points of the object which form it must lie more nearly in the same plane; higher eye-pieces or longer tubes have the same effect as higher objectives. The question of "pleasantness" in regard to degrees of aperture, will therefore depend upon the investigations in which individual observers are employed, and their tastes and acquired habits. The most interesting suggestion of possible evils from very large angles, is that lately put forth by Mr. Wenham: that the projection of a solid upon a front lens near its margin being almost that of a side view, when extremely oblique, a number of different projections are thus made to overlap each other in the image. It is very instructive to repeat this gentleman's experiment. Looking at an insect's egg shell with a circular opening, the plane of the circle lying oblique to the horizon, a right hand and left hand view ot the circle gives different elliptical projections. These different ellipses may be produced by interposing a slip of card and cutting off alternately the right and left sides of the front lens of the objective. We thus gain two different forms of the object, both of which are received together in ordinary microscopes. The same change of form may be effected by covering one-half of the eye lens of the instrument. Mr. Wenham's figures were taken by a $\frac{2}{3}$rd object-glass. The effect becomes much more striking when we experiment with a higher power and an angular aperture three times as wide. But whatever be the power or angle, if the definition be good the upper and lower sides of the circle *are not in precise focus at the same time.* It is not necessary for producing these changes of form to cut off a part of the pencil at all; as we carry the focus from the upper to the lower edge, the ellipticity of the figure is plainly seen to change, as evidently by the laws of geometry it must do, from the change of angular direction of its different points to the lens. Instead of the right and left, the upper and lower sides of the lens may be cut off, a requisition for which stereoscopy does not provide, although it may be thought of importance in regard to large apertures.

Again, if we bring into focus the under side, say, of the circle, we may notice a slight mistiness about its general outline, which may be removed by cutting off half the objective front lens, or of the eye lens. But on more careful examination, we shall perceive that we are bringing no one point into perfectly accurate focus, and at the same time concentrating our attention upon it. If these two things be attended to, all mistiness, thickening, and uncertainty, will disappear from such objectives as this gentleman employed. Such facts are no real obstacles to microscopic accuracy. They occur equally in unaided vision, whether of small or of large objects. If possible, we hold a solid body—a crystal for instance—in the hand, and turn it about, yet each change of position projects a different geometrical figure. The laws of vision are the same with a microscope as without, and no perfection of instruments can supersede the necessity of comparing different views and of arriving at a knowledge of the third dimension in space, through operations of the mind.

In some cases a restricted focal distance is both convenient and advantageous: one is when we look through a set of markings. The Stauroneis pulchella of Rev. Wm. Smith, is described by him: "striæ very distinct, 30 in ·001″, punctate; puncta hexagonal." In his introduction, Mr. Smith remarks: "The experiments and authority of Professor Bailey place the existence of an internal membrane (in the Diatomaceæ) beyond all doubt."—"In Stauroneis pulchella the membrane in question possesses an unusual degree of firmness, the siliceous valves, after a slight maceration in acid, may be seen to fall away from the internal membrane and to leave the latter unaltered in form."

Specimens in balsam from London, prepared by the author of these remarks, exhibit the following appearances: under a $\frac{1}{4}$ of between 60° and 70°, with a working distance sufficient for a Lieberkuhn, and having (to quote the term applied by the Jury of the Great Exhibition) the "exquisite" correction of Smith and Beck, they show by an achromatic condenser, the hexagonal puncta in three sets of lines, yet obscured in definition, and confused by the mingling of what seem like shadows.

Intense illumination of a suitable obliquity and direction, exhibits a set of fine striæ in a plane *above* these puncta, and running in directions which coincide with neither of the lines of puncta. So far, some would make the objection of "optical appearances." But when the focus is limited by the joint influence of the still more perfect correction, and the increased angles of the $\frac{4}{10}$ of 100°, the $\frac{1}{5}$ of still larger angle, and the $\frac{1}{8}$ of 120° to 128°, these two distinct sets of markings may be seen with either perpendicular or oblique light, simply by carrying the focal distance from the one to the other, and without changing the illumination. The upper striæ are at the distance quoted; the hexa-

gons below are twice as distant; each set in turn may be seen in in its own plane, and without confusion from the other. We do not refer to these hexagons as an instance of the interior membrane referred to in the passage just now extracted, which seems to indicate a different one. The appearances are the same on every specimen, and fragment, whether lying flat or edgewise. The striæ are similar, only more difficult to distinguish with accuracy, to the *fine* diagonal markings on the surfaces of Triceratium, Actinocyclus, Coscinodiscus, and other genera which have large hexagonal cells.

An able optician from Berlin says that the importance of spherical correction is but imperfectly understood in Germany: we thus find accounted for the mistakes of Ehrenberg, which have caused so much surprise, and Dr. Hannover's omitting all mention of a screw collar, in a work of such merit as to be selected for translation into English.

Art. XXIII.—*Further Notes on Cereus giganteus of Southeastern California, with a short account of another allied species in Sonora;* by Dr. George Engelmann, St. Louis, Missouri.

Specimens of flowers and fruit, together with interesting notes and drawings communicated by Mr. George Thurber, and specimens of ribs of the plant with spines presented by Dr. Parry, enable me to perfect the history of this giant Cactus.* Mr. Thurber travelled through the Gila country and Sonora, as one of Mr. Commissioner Bartlett's party, in the summer of 1851, and is believed to be the only scientific gentleman who has seen the plant in question in flower. These materials enable me to furnish the following detailed character.

Cereus giganteus, *Engelm.*: erectus, elatus, simplex, s. ramis paucis erectis caule cylindrico versus apicem sensim attenuato brevioribus candelabriformis; vertice applanato tomentoso; costis ad basin caulis sub-12 versus apicem 18–20 rectis obtusis (vetustioribus ad caulis basin obtusissimis) subrepandis; sinubus ad basin caulis latissimis versus apicem profundis angustioribus angustissimisque; areolis prominentibus ovato-orbiculatis junioribus albido-tomentosis; aculeis rectis basi valde bulbosis tenuiter sulcatis angulatisque albidis demum cinereis, radialibus 12–16 imo summisque brevioribus, lateralibus (præcipue inferioribus) longioribus robustioribus subinde cum aculeis adventitiis paucis setaceis summo areolæ margini adjectis; aculeis centralibus 6 robustis albidis basi nigris apice rubellis demum totis cinereis, 4 inferiori-

* See this Journal, New Series, vol. xiv, page 335, Nov., 1852.

bus decussatis quorum infimus longissimus robustissimus deflexus, 2 superioribus lateralibus brevioribus; floribus versus apicem caulis ramorumque sparsis, tubo ampliato breviusculo petalisque patulis; ovario ovato sepalis 25–30 squamiformibus triangulatis acutis in axilla fulvo-villosis stipato; sepalis tubi sub-30 orbiculato-subtriangularibus mucronatis, inferoribus in axilla lanigeris, superioribus nudis, sepalis intimis 10–15 spathulatis obtusis carnosis (pallide viridibus albescentibus); petalis sub-25 obovato-spathulatis obtusis integris crispatis coriaceo-carnosis crassis (flavescenti-albidis); staminibus numerosissimis, filamentis superiori tubi parti adnatis (inferiore nudo); stylo stamina paulo superante; stigmate multifido; bacca obovata squamis sepaloideis triangularibus carnosis minutis ad axillam fulvo-lanatis stipata, pericarpio duriusculo carnoso, demum valvis 3–4 patentibus reflexisve dehiscente; seminibus numerosissimis in pulpa saccharina nidulantibus oblique obovatis lævibus lucidis exalbuminosis; embryone cotyledonibus foliaceis incumbentibus hamato.

This species ranges from north of the Gila river southwardly into Sonora, to within 20 miles of Guaymas on the Californian Gulf. It doubtless also occurs on the Peninsula of California; where, according to Vanegas in his history, published about 100 years ago, the fruit of a great Cactus forms an important article of food to the natives of the eastern coast, the harvest time of which was a season of great festivity. The flowers are produced in May and June, and the fruit ripens in July and August. Mr. Thurber collected the last flowers and the first ripe fruit in the beginning of July. He has collected abundance of seed, and will be pleased to communicate it to those who take an interest in the cultivation of Cacti. The youngest plants Mr. Thurber noticed were three or four feet high, with narrow furrows and long spines; the smallest flowering plants were about 12 feet high, and the tallest specimens observed appeared to reach the elevation of 45 or 50 feet.

The ligneous fascicles correspond with the intervals between the ribs, and not with the ribs themselves; of which Dr. Parry has fully satisfied himself, and which indeed is the case in all ribbed Cacti. From between these bundles ligneous fibres radiate horizontally towards the ribs, and especially to the areolæ.

At the base of the stem the ribs are broad and obtuse, with wide and shallow intervals; upwards the ribs are somewhat triangular, rounded or obtuse, with deep and acutish grooves between them; towards the top of the plant the ribs are equally obtuse, but quite compressed, and the grooves are deep and narrow.

The elevated areolæ are 7 lines long, nearly 6 lines in diameter, about an inch distant from one another, sometimes more closely approximated.

Lowest and upper radial spines 6 to 12 lines long, sometimes the upper ones with a few additional, shorter, flexuous, setaceous spines: lateral ones 12–18 lines long, the lower ones longest; the four lower central spines straight or very slightly curved downwards, 20–30 lines long; the two upper central spines 15 to 18 lines long. The stoutest spines are one line in diameter, their bulbous base being fully twice as thick. The old spines together with the whole areola readily come off in one bunch, but generally the 6 central spines fall off first, leaving the radiating ones appressed to the stem, till finally they also fall away.

The flowers are produced near the summit of the plant, but not on it, and the fruit is usually 6–12 inches from it.

The dried flower communicated by Mr. Thurber is 3 inches long; but the drawing represents the flowers as fully 4 inches in length and diameter. The ovary in the dried specimen is ⅝ths of an inch long; the lower naked part of the tube 1 inch, the upper staminiferous much widened part ⅝ths of an inch long. Upper sepals fleshy, greenish white, ⅝ths of an inch long, below 2, above 4 lines wide. Petals of a light cream color, an inch long, 6–7 lines wide above, very thick and fleshy, and very much curled. Filaments light yellow, adnate to the upper half of the tube: anthers 0·8 to 0·9 of a line long, linear, emarginate at the base and apex. Style not seen; the drawing represents the numerous (15–20?) stigmata as half an inch long, suberect, of a green color. The flowers appear to be open night and day, and probably for several days in succession.

The fruit sent by Mr. Thurber (in alcohol) is obovate 2½ inches long, by 1½ in diameter, beset with about thirty scales, having short brownish wool in their axils, but entirely destitute of spines. Mr. Thurber informs me that this specimen is unusually long: the fruit, he says, is usually 2 or 3 inches long by 1½ to 2 in diameter; the color is green, reddish towards the summit; the remains of the flower fall off, leaving a broad and convex scar. The pericarp has the hardness of a green cucumber, somewhat softer towards the apex, and is about 2 lines thick: it bursts open on the plant with 3 or mostly 4 irregular, interiorly red valves, which spread horizontally, and appear like a red flower when seen at a distance, which accounts for the report of this species having red flowers. The crimson-colored and rather insipid pulp has the consistency of a fresh fig; it completely separates from the rind, and drying up from the heat of the sun, falls to the ground, or is beaten down, when it is collected by the natives and rolled into balls, which keep several months, or is pressed for the thick molasses-like sacharine juice which it contains. The innumerable seeds are 0·7 to 0·8 lines long.

Another, apparently nearly allied species, was collected in Northern Sonora. From the half of a flower before me, together

with Mr. Thurber's meagre notes, (other specimens unfortunately having been lost,) I have ventured to make out the following description :

CEREUS THURBERI (n. sp.): erectus, elatior, e basi ramosus sub-14-costatus, sulcis parum profundis, aculeis brevibus nigricantibus; floribus tubuloso-campanulatis virescenti-albidis; ovario globoso sepalis 80–100 carnosis squamiformibus triangularibus acutis imbricatis ad axillam villosis stipato; sepalis tubi inferioribus 24 lanceolatis acutiusculis axilla nudis, superioribus 20–25 orbiculato-obovatis obtusis; petalis 16–20 obovato-spathulatis obtusis crassis.

Collected in June 1851, in a rocky cañon near the mountain pass of Bacuachi, a small town on the road to Arispe, in Sonora; afterwards found with *Cereus giganteus*, near Santa Cruz: it abounds also near Magdalena and Ures. Santa Cruz appears to be the northern limit of this species, which does not extend to the Gila river. Stems 4 to 12 feet high, many from the same base, 6 to 10 inches in diameter, sometimes articulated, occasionally branching above, with about 14 ribs and shallow grooves. Flowers greenish white, borne about a foot below the summit of the stem. Dried flower 2¾ inches long; the tube narrower, and more elongated than in *C. giganteus;* the globose ovary and the naked and staminiferous part of the tube each about ¾ inch long; free part of petals of the same length, and 4 lines wide. Anthers much larger than in the foregoing species, 1·3 to 1·4 lines long. Style not seen.

I have dedicated this to the collector, Mr. George Thurber, of Rhode Island, an excellent botanist, who has kindly furnished me with the materials for this article.

Cereus Thurberi and *C. giganteus* appear to be closely allied species. They have high and erect stems, flowers with a short tube, half of which is naked, the filaments occupying only the upper half of the tube; both have short and fleshy sepals on the ovary, with short wool in their axils, unaccompanied by any bristles or spines; in both the petals are whitish, obtuse, and fleshy.

Both, and especially *C. giganteus*, stand very near the *Pilocerei* on account of the great height of the stem, the short ventricose tube of the flower, and the thick petals; but they have not the least indication of a *cephalium* (or woolly head) nor of any particular development of wool; their flowers spring from the axils of the ordinary and unaltered areolæ; and the seed is quite different, at least from that of *Pilocereus senilis*, the only species of that genus, I believe, which has been well examined; these seeds are said to be obliquely thimble shaped, densely dotted, and to have an embryo with thick globose cotyledons. It is also said that the filaments cover the whole inside of the tube of the flower,

and even the free upper part of the ovary. In all the *Cerei* and *Echinocacti* examined by me, I find the lowest part of the tube free, the filaments being aduate to some distance above the ovary. It is not improbable that the Chilian velvety *Cerei* (*Velutini*, Pr. Salm.) are to be classed near our species. The flower of what appears to be *Cereus Chilensis*, Pfr., obtained near Valparaiso, and figured by the artist of the U. S. Exploring Expedition, greatly resembles that of *C. Thurberi:* it is a little larger, but has the same shape, and the same closely imbricated sepals on the ovary; the tube has about 100 sepals, and the white petals are acute; whether fleshy or not is uncertain.

ART. XXIV.—*On the Chemical Composition of Recent and Fossil Lingulæ, and some other Shells;* by W. E. LOGAN, F.R.S., and T. S. HUNT.

IN the Report of Progress of the Geological Survey of Canada for 1851–52, we have mentioned the existence of small masses containing phosphate of lime, and having the characters of coprolites, which occur in several parts of the Lower Silurian rocks. In a bed of silicious conglomerate towards the top of the calciferous sandstone, at the Lac des Allumettes, on the Ottawa, they are abundant in cylindrical and imitative shapes, sometimes an inch in diameter. The same material forms casts of the interior of a species of Holopea or Pleurotomaria, and often fills or completely incases the separated valves of a large species of Lingula, which Salter has referred to *L. parallela* of Phillips. The phosphatic matter is porous, friable, and of a chocolate brown color; it contains intermixed a large quantity of sand; and small pebbles of quartz are sometimes partly imbedded in it. The analysis of one specimen gave 36 per cent. of phosphate of lime, with 5 p. c. of carbonate and fluorid, besides some magnesia and oxyd of iron, and 50 p. c. of silicious sand.

Similar masses occur in the same formation at Grenville, and in the lower part of the Chazy limestone at Hawkesbury, in both cases containing fragments of Lingula. Those from the latter place, are rounded in shape, and from one-fourth to one-half of an inch in diameter, blackish without, but yellowish-brown within, and having an earthy fracture; the analysis of one of them gave:

Phosphate of lime, (PO_5, 3CaO),	44·70
Carbonate of lime,	6·60
Carbonate of magnesia,	4·76
Peroxyd of iron, and a trace of Alumina,	8·60
Insoluble silicious residue,	27·90
Volatile matter,	5·00
	97·56

From the color it is probable that the iron exists as a carbonate. When heated in a tube, a strong odor like burning horn is perceived, accompanied by ammonia which reddens turmeric paper and gives white fumes with acetic acid, showing that a part at least of the volatile matter is of an animal nature. The specimens from Lac des Allumettes lose 1·7 p. c. by gentle ignition, with a like production of ammonia, and an odor of animal matter; the same thing was observed with those from Grenville.

The existence in Lower Silurian rocks, of these masses, whose characters leave no doubt that they are coprolites, and whose chemical composition is like that of the excrements of creatures feeding upon vertebrate animals, led us to examine the shells of the Lingulæ always associated with these phosphatic bodies. The result has been that all the specimens yet examined consist chiefly of phosphate of lime; they dissolve readily with slight effervescence in hydrochloric acid, and the solution gives with ammonia a copious precipitate readily soluble in acetic acid, from which oxalic acid throws down lime. With a solution of molybdate of ammonia there is obtained a quantity of the characteristic yellow molydo-phosphate, many times greater than the bulk of the shell.

We have thus examined *Lingula prima*, and *L. antiqua*, from the Potsdam sandstone, *L. parallela* from the calciferous, and a species somewhat resembling *L. quadrqta*, from the Trenton limestone. It was desirable to compare with these, the shell of a recent species, and for this purpose, fine specimens of the *Lingula ovalis*, of Reeve, from the Sandwich Islands, were furnished us by J. H. Redfield, Esq. of New York. The shell of this species had the same composition as the fossil ones, and the thick green epidermis, which swelled up like horn when heated, gave a bulky white ash of phosphate of lime.

For a further analysis the shell was boiled in water to remove all soluble matters, the soft parts still adherent were carefully detached, and the shell with its epidermis weighing ·186 grammes, was calcined over a spirit lamp. The brownish residue weighing ·114 grammes, readily dissolved with slight effervescence, in dilute hydrochloric acid, leaving but a few light flakes of carbonaceous matter. Acetate of soda and perchlorid of iron were added to the solution, which was boiled, and the precipitated basic salt separated by filtration, and decomposed by hydrosulphuret of ammonia. The filtrate from the sulphuret of iron having been concentrated, the phosphoric acid was thrown down by ammonia with a magnesian salt; there was obtained ·070 grms. of pyrophosphate of magnesia, equal to ·044 of phosphoric acid, or ·0978 of phosphate of lime, $PO_5, 3CaO$.

The lime was separated from the acetic filtrate, as an oxalate, and gave ·108 of carbonate, equal to ·0605 of lime, being an ex-

cess of ·0075 over the amount required to form the phosphate, and corresponding to ·0134 of carbonate; the small amount of material did not permit us to determine whether a portion of the lime exists as fluorid. There was also obtained ·0032 of magnesia; the results from the calcined shell of *Lingula ovalis* are then as follows:

Phosphate of lime,	·0978	=	85·79 p. centum.
Carbonate of lime,	·0134	=	11·75
Magnesia, - -	·0032	=	2·80
	·1144	=	100·34

The proportion of phosphate of lime is that contained in human bones, after their organic matter has been removed.

The texture of the ancient Linguliæ was observed to be unlike that of most other fossil shells, being more or less dark brown in color, brilliant, almost opaque, and not at all crystalline. These characters are also found in the allied genus *Orbicula*, and we therefore examined an undescribed species of it, from the Trenton limestone, beautifully marked in a manner resembling *Conularia granulata*, and another large species also undescribed, from the Upper Silurian; both of these consist chiefly of phosphate of lime; and the shell of a recent species *O. lamellosa* from Callao, was found to be similar in composition. We have not yet been able to examine a specimen of the genus *Obolus*. The same dark color and brilliancy were also remarked in the genus *Conularia*, and the shell of *C. trentonensis*, proved on examination to be composed in like manner of phosphate.

The similarity of composition in these genera is in accordance with the acute observations of Mr. Hall, who finds that *Conularia* is almost always associated with *Lingula* and *Orbicula*, and remarks that "these shells so unlike in structure and habit, appear to have flourished under similar circumstances, and to have required the same kind of ocean bed or sediment."—Palæontology, vol. i, p. 101.

For the sake of comparison, we have examined the following fossil shells: they have a common character, distinct from those already described, being lighter colored, more translucent and granular in texture; *Atrypa extans*, *Leptæna alternata*, and *Orthis pectenella* from the Trenton limestone; *O. enatica* from the Hudson River group, and *Chonetes lata ?* from the Upper Silurian, besides *Isotelus gigas*, and a species of *Cythere* from the Trenton. All of these consist of carbonate of lime, with only such traces of phosphate as are generally found in calcareous shells.

In the Report already quoted we have given a description of some phosphatic bodies which resemble the coprolites of the Calciferous sandstone, and are found at Rivière Ouelle in thin layers of a conglomerate limestone, which is interstratified with

red and green shales, and belongs to the top of the Hudson River group or the base of the Oneida Conglomerates. The phosphatic masses are very abundant, and rounded, flattened, or cylindrical in shape, and from one-eighth of an inch to an inch in diameter; they sometimes make up the larger part of the conglomerate. Iron pyrites in small globular masses occurs abundantly with them, often filling their interstices, but is not found elsewhere in the rock. These coprolites are finer grained and more compact than those from the Ottawa, and have a conchoidal fracture; their color is bluish or brownish black; the powder is ash-grey, becoming reddish after ignition. They have the hardness of calcite and a density of 3·15. When heated they evolve ammonia with an animal odor, and with sulphuric acid give the reactions of fluorine. The quantitative analysis of one gave—

Phosphate of lime, $PO^5, 3CaO$,	-	40·34 p. c.
Carbonate of lime, with fluorid,	-	5·14
Carbonate of magnesia,	- -	9·70
Peroxyd of iron with a little alumina		12·62
Oxyd of manganese,	- - - -	*trace*
Insoluble silicious residue,	- -	25·44
Volatile matter,	- - - - -	2·13
		95·37

The iron exists in part at least as carbonate, and its introduction in so large a quantity, giving color and density to the coprolites, is doubtless connected with the formation of iron pyrites by the de-oxydizing action of organic matters. The production of an equivalent of bisulphuret of iron, from a neutral protosulphate of iron, which alone could exist in contact with limestone, must be attended with the elimination of an equivalent of protoxyd of iron; for $2(SO_3 . FeO) - O_7 = FeS_2 + FeO$.

It is remarkable that no traces of Lingulæ or any other shells have been detected with these coprolites. Thin sections of them are translucent, and under the microscope are seen to consist of a fine granular base, in which are imbedded numerous grains of quartz, and small silicious spiculæ, like those of some sponges. In a bed of sandstone, associated with these conglomerates and slates at Rivière Ouelle, were found several hollow cylindrical bodies, resembling bones in appearance. The longest one is an inch and a half long, and one-fourth of an inch in diameter. It is hollow throughout, and had been entirely filled with the calcareous sandstone, in which it is imbedded, and whose disintegration has left the larger end exposed. The smaller extremity is cylindrical, and thin, but it gradually enlarges from a thickening of the walls, and at the other end becomes externally somewhat triangulariform; the cavity remains nearly cylindrical, but the exposed surfaces are rough and irregular within.

The texture of these tubes is compact, their color brownish black with a yellowish brown translucency in thin layers. Analysis shows them to consist, like the coprolites, principally of phosphate of lime. One hundred parts gave,

Phosphate of lime, - - - -	67·53
Carbonate of lime, - - - -	4·35
Magnesia, - - - - -	1·65
Protoxyd of iron, - - - -	2·95
Insoluble silicious sand, - - -	21·10
Volatile, animal matter, - - -	2·15
	99·73

The microscopic examination of a section, shows that the walls of the tube are homogeneous, unlike the coprolites, and that the silicious sand in the analysis, came from the sandstone which incrusted the rough interior of the fossil. The phosphate is finely granular and retains no vestige of organic structure. The chemical composition and the remarkable shape of the specimens however, leave little doubt of their osseous nature, unless we suppose them to be the remains of some hitherto unknown invertebrate animal, whose skeleton, like those of Lingula, Orbicula and Conularia, consisted of phosphate of lime, a composition hitherto supposed to be peculiar to vertebrate skeletons.

Montreal, Jan. 5th, 1854.

Art. XXV.—*On a new Meteorite from New Mexico;* by Dr. F. A. Genth, of Philadelphia.

I am indebted to Prof. Joseph Henry, Secretary of the Smithsonian Institution, for a small piece of an interesting meteorite from New Mexico. It was labelled "Native Iron," and is said to occur there in large quantities. Fortunately it was just sufficient for an examination, the results of which I here give. There is no doubt that the mineral is of meteoric and not of telluric origin.

It is very crystalline and shows a distinct octahedral cleavage. Its color is iron-gray, its lustre metallic. Quite ductile. Sp. gr. (at 18° Cels.) = 8·130.

Dissolves readily in diluted nitric acid, leaving a small quantity of insoluble residue—which, however, was also slowly dissolved by strong nitric acid or aqua regia, but still more easily by fusion with bisulphate of potash.

The methods used for its analysis were the following: In analysis I, the meteorite was dissolved in strong nitric acid; nickel and cobalt were separated from iron by carbonate of baryta; nickel and cobalt were separated by hydrocyanic acid, potash and oxyd of mercury.

In analysis II, the meteorite was dissolved in diluted nitric acid, and the residue filtered off on a weighed filter. In the filtrate iron was separated from cobalt and nickel, by addition of a sufficient quantity of acetate of potash, in order to convert the nitrates into acetates, and evaporation to dryness in a water-bath. The dry mass was boiled with water and filtered. From the filtrate, which contained the whole quantity of oxyds of cobalt and nickel, these were precipitated by caustic potash. The precipitate of sesquioxyd of iron was re-dissolved in hydrochlorid acid, and precipitated by ammonia. This method gives excellent results, if used with care. The only objection might be, that the sesquioxyd of iron, thus separated, is difficult to filter.

The insoluble residue was ignited and fused with bisulphate of potash. On treating the fused mass with water, a white substance of the appearance of tartaric acid remained, which hydrochloric acid slowly dissolved. This substance and sesquioxyd of iron were precipitated by ammonia, and from the filtrate, oxyd of nickel separated as usual. The precipitate was weighed, dissolved in hydrochloric acid, and the iron precipitated by sulphid of ammonium, after the addition of tartaric acid and ammonia. From the sulphid of iron, the iron was determined as usual. From the filtrate, the other substance remained after the tartaric acid was destroyed by heat. It was, however, a very small quantity, and only sufficient for *one* blowpipe reaction. The borax bead gave in the inner flame an enamel of a bluish color. I therefore believe that it is tartaric acid, though the reactions somewhat differ.

The insoluble residue seems to be a combination of Iron, Nickel and Titanium. It contains *no* cobalt. Neither part of the meteorite contained carbon, sulphur, phosphorus or tin.

	I.	II.
Iron, - - - -	= 96·17	95·92
Nickel, - - - -	= 3·07	3·57
Cobalt, - - - -	= 0·42	
Insoluble, - - -	= ——	0·57
	99·66	100·00

The insoluble part consisted of a steel-colored powder in microscopic crystals, which showed three-sided planes. Its composition is:

Iron, - - - - -	= 55·07 p. c.
Nickel, - - - - -	= 28·78
? Titanium, - - - -	= 16·15
	100·00

It is remarkable that the elements in the insoluble part are in the following ratio:

Fe : Ni : ? Ti = 6 : 3 : 2.

ART. XXVI.—*Introductory Essay, in Dr. Hooker's Flora of New Zealand:* Vol. I.*

DR. J. D. HOOKER, the Botanist of the Antarctic Expedition under Capt. Sir James C. Ross, on his return to England—combining with his own extensive collections and observations all the accessible materials which have been accumulating in herbaria ever since the first voyage of Capt Cook—courageously assumed the task of preparing general floras of the three principal masses of southern land, in which his researches were made; viz., Antarctic America, New Zealand, and Tasmania. The first of these undertakings was accomplished several years since, in the publication of the *Flora Antarctica;* including Antarctic America with the Falkland Islands, the Campbell and Auckland Islands (properly pertaining to the New Zealand region), and the remote Kerguelen's Land. Some abstracts were given in this Journal at the time, from parts of the work possessing a general interest.

The recent completion of the fourth fasciculus, and first volume, of the second work, viz.: the *Flora Novæ Zelandiæ*, comprising all the Flowering plants of that group of islands, affords our author an opportunity to discuss, in an introductory essay, some topics of high interest to the philosophical naturalist; topics upon which his aptitude for such investigations, and the unparalleled opportunities of observation which he has enjoyed, and improved to the utmost, in almost every clime, must needs give no small weight to his opinions. Deeming as we do this essay to be an important and timely one, we propose to make a somewhat extended analysis of it, leaving out of view for the present the body of the work, as interesting to the systematic botanist alone.

In the Essay in question (occupying 39 quarto pages), Dr. Hooker gives, 1. The History of New Zealand Botany, and the probable limits of its flora. 2. An exposition of the views adopted in the descriptive part of the work, as to the affinities, limits, origin, variation, distribution, and dispersion of plants generally. 3. The illustration and development of these points by an analysis of the New Zealand Flora, and its relation to that of other countries.

The history of the Botany of New Zealand, from the visit of Sir Joseph Banks and Dr. Solander, during Capt. Cook's first voyage, in 1790, down to the present time, need not arrest our attention. The actual number of species inhabiting these islands is a matter which it would be interesting to know, even approximately. Dr. Hooker has brought together about 2,000 species in

* The Botany of the Antarctic Voyage of H. M. Discovery Ships Erebus and Terror, in the years 1839—1843, under the command of Capt. Sir James Clark Ross; by Joseph Dalton Hooker, M.D., etc. etc. II. Flora Novæ-Zelandiæ, Part I. Flowering Plants. London. Lovell Reeve, 1852–1853. pp. 312, 4to, tab. 70.

his Flora, including upwards of a hundred of the lower Cryptogamia of which the materials are in too imperfect a state for satisfactory determination. This is more than double the number comprised in the latest preceding catalogue, that of M. Raoul, "who in 1846 enumerates only 920 species; which may be reduced to 770, if the naturalized and erroneous species be eliminated." "In 1838, Mr. Cunningham gave 640 species, which should be reduced to 570: in 1832, M. Richard included 350 in his list: Forster's Prodromus has 154; and Banks' and Solander's collections amount to 426. This rapid increase of the flora, which has thus been quintupled in twenty years, is mainly due to the attention which has been devoted to the lower orders. This may be easily shown; for, whereas in all the earlier enumerations and collections the number of Flowering plants exceeds the Flowerless, in M. Raoul's catalogue they are equal, and in the present work the relative proportions are reversed; the Phænogamic plants being to the Cryptogamic as 1 to 1·6, i. e. about two to three." As to the probable ratio of the known materials to the whole flora, Dr. Hooker remarks that "the islands have been botanized upon by upwards of 35 individuals, whose specimens have (with a few unimportant exceptions) all passed under my eye. The flora of the Northern Island has been tolerably well examined, so far as its flowering plants are concerned; though there remains a good deal to be done on the west coast, especially in the neighborhood of Mount Egmont. Dr. Lyall alone has collected on the Southern Island, and on the west coast north of Dusky Bay. The Middle Island has been visited by few explorers; its north and east coasts alone having been botanized: the west, and the whole mountain range require a careful survey; and, considering how many Auckland and Campbell Islands' plants are still strangers to New Zealand, it cannot be doubted that much remains to be discovered there. Excepting from the above mentioned tracts, I do not expect much novelty among Flowering plants, for the following reasons: 1. There is a remarkable sameness in the flora throughout large tracts, (in which respect New Zealand contrasts remarkably with Tasmania); 2. Because out of the 730 flowering plants known, there are scarcely one hundred that have not been gathered by several individuals; 3. Because the collections I have lately received, though some of them are extensive, and from scarcely visited localities, yet contain little or no novelty. With Cryptogamia the case is widely different; and it is difficult to estimate the vast number, especially of Mosses, Hepaticæ, and Fungi, that will reward future explorers in what, as far as Flowering plants are concerned, are exhausted fields." From the data now possessed, and from a comparison of the same with the flora of better known countries, Dr. Hooker ventures the opinion that there are not more than 4000 species in

New Zealand, of which 1000 may be Flowering plants. "Compared with any other countries in the same latitude, this is a very scanty flora indeed, especially as regards Flowering plants; of which Britain contains, in about the same area, upward of 1,400 species; and in Tasmania, not yet well explored, and containing only one-third of the area, upwards of 1000 have already been discovered. In Cryptogamous plants, on the other hand, these islands are extremely rich, not only proportionately to the Phænogamic, but absolutely so. Great Britain, where the lower orders have been assidously studied for fifty years, contains about 50 Ferns, and Tasmania 64;" while Dr. Hooker's list of New Zealand Ferns (including Lycopodiaceæ), after reducing almost half as many nominal species to more varieties, contains upwards of 114 species. The same result would appear all the more strikingly on a comparison with any equivalent *continental* tract in the northern hemisphere. In all British America and Oregon only 62 species of Ferns and Lycopodiaceæ are recorded.

Dr. Hooker's second chapter, on the limits of species, their dispersion and variation, deals with matters of higher general interest. To bring the points in question fairly into view, he assumes four positions as heads of the subjects upon which he proceeds to discourse, namely:

"1. That all the individuals of a species (as I shall attempt to confine the term) have proceeded from one parent (or pair), and that they retain their distinctive (specific) characters.

2. That species vary more than is generally admitted to be the case.

3. That they are also much more widely distributed than is usually supposed.

4. That their distribution has been effected by natural causes; but that these are not necessarily the same as those to which they are now exposed."

The first of these propositions should have been divided into two:—inasmuch as the first clause is not only open to some very specious if not cogent objections which do not apply to the others, but is from its very nature incapable of being supported by the kind of evidence which may sustain the other propositions. The fact that we constantly see like individuals reproduced, and under favorable circumstances increased, from a parent stock, lays, indeed, a solid ground for the inference that this process has been going on from the beginning: but how things go on, and how they began, are two different questions; and it is seldom, if ever, that the facts and deductions which account for the former, can be made to throw much *direct* light on the latter. Why is it not antecedently just as probable that several or many individuals of each species of plant, as identically like each other as are the offspring to the parent now, were created at the beginning, as

that each began with a single pair? We would still maintain that the objective idea of species arises from this "perennial succession of like individuals (to use the phrase of Linnæus), sustaining to each other the relation of parent and progeny; and we think that the evidence on that side of the question strongly favors the inference, that plants, at least, have been distributed each species from a single and specific primordial area: but we know not what scientific evidence makes it needful to maintain the doctrine of the single creation of species in the restricted form of a single initial individual or pair. Were we even to go so far as explicitly to assume the likelihood, in certain cases, of the original creation of numerous individuals of a species, all alike in character, we could not be charged with infringing the rule that no more causes should be assumed than are requisite to ensure the result; since—to say nothing of the imminent risk of the premature destruction of a single individual—in many cases both of animals and vegetables, a considerable number would be required at the outset to fulfil the relations established between the individuals, either of the same or of different species. It would be more satisfactory, therefore, because less hypothetical and more within the reach of evidence, if the proposition were stated in the more general form; viz., that all the individuals of a species have proceeded from a common stock, assigned to a limited primordial area;—in which form its bearing upon the other propositions would not be altered. Indeed, Dr. Hooker expressly states, in the commencement of his remarks upon the first proposition, that he lays no stress upon that particular hypothesis, nor does he advance any arguments in support of it. The whole section is, in fact, mainly devoted to the illustration of the second clause, namely, that species retain their distinctive characters from age to age. As the section can hardly be abridged without injury, we extract a large portion of it. After stating various views that are maintained in respect to the origin or development of species, the author proceeds:

"Arguments in favor of these views are not wanting, derived both from the animal and vegetable kingdoms; the chief of which are drawn from a large class of well established facts, upon the bearings of which the most distinguished and candid naturalists are divided in opinion: such are—the great number of genera whose species have baffled all attempts at circumscription by fixed characters,—the facility with which breeds of certain plants and animals may be propagated, and the comparative certainty with which some few varieties are reproduced under favorable circumstances,—the great facility with which many plants hybridize, and the facts of hybrids having proved fertile,—the sudden appearance and unexplained cause of many varieties or sports,—and the difficulty of accounting for the existence of plants and animals in two or more localities, between which they cannot have been

transported by natural causes now in operation. These are all questions relating to the diffusion and variation of species, which will be discussed here and in the following section.

Arguments in favor of the single creation, and permanence of species, are all based upon general considerations of the phenomena of distribution. Comparative anatomy, which has thrown so great light upon this branch of study in the sister kingdom, has not done so much for plants; this arises from several causes: 1. The habits of allied plants do not differ so remarkably as those of animals, and there is consequently less modification of their functional organs. 2. The relation of these modifications to the habits and wants of the species, is in the animal kingdom directly appreciable, but in plants no such connection can be traced.* 3. The individual organs of support, respiration, and reproduction, are infinitely more variable and susceptible of change and even obliteration in plants, without affecting the life either of the individual or of the species.† The result of these facts is that we have the means in animals of appreciating the extent and value of differences, by combined observations upon structure and functions, upon habits and organization, which we have not in the vegetable kingdom, and which the phenomena of cultivation assure us do not exist to a degree that has, within the limits of our experience, proved available for throwing much light on the subject.

The arguments in favor of a permanence of specific characters in plants are:—

1. The fact that the amount of change produced by external causes does not warrant our assuming the contrary as a general law. Though there are many notorious cases in which cultivation and other causes produce changes of greater apparent value than specific characters generally possess, this happens in comparatively very few families, and only in such as are easily cultivated. In the whole range of the vegetable kingdom, it is difficult to produce a change of specific value, however much we may alter conditions; it is much more difficult to prevent an induced variety from reverting to its original state, though we perse-

* The structure of woods offers many illustrations of this; very closely allied plants (especially *Leguminosæ*) differing entirely in the nature, arrangement, and development of the vascular and cellular tissues of their trunks. Though to a great extent these differences accompany a habit of growth (as in the case of erect and scandent *Bauhinias*), there is nothing in the abnormally developed wood of the climbing *Bauhinia* that would lead a skilled physiologist ignorant of the fact to say that it was better adapted to a climbing than to an erect plant; the function is experimentally known to be indicated by the structure, but the structure is not seen to be adapted to the function. This is not so in the sister kingdom, for we confidently pronounce an animal to be a climber, because we see that its organs are adapted to the performance of that function; here the habit is not only indicated by the structure but the latter is explained by the function which it enables the animal to fulfill.

† To take an extreme case of this;—many plants are known, in a wild and cultivated state, which propagate abundantly by root or division, where they do not do so by seed. *Anacharis Alsinastrum* is a conspicuous example: it is a unisexual water-plant, of which one sex alone was introduced from North America into England, where it has within a few years so spread by division as to be a serious impediment to inland navigation. The Horse-radish is another example, it being, I believe, never known to seed or even to bear perfect flowers. A still more remarkable case has been pointed out to me by Mr. Brown, in the *Acorus Calamus*, a plant spread (not by cultivation) over the whole north temperate hemisphere, which bears hermaphrodite flowers, but very rarely seeds.

vere in supplying the original conditions; and it is most difficult of all to reproduce a variety with similar materials and processes.*

2. In tracing widely dispersed species, the permanence with which they retain their characters strikes the most ordinary observer; and this, whether we take such plants as have been dispersed without the aid of man (as *Sonchus oleraceus*, *Callitriche*, and *Montia*) through all latitudes from England to New Zealand; or such as have within modern times followed the migrations of man (as *Poa annua*, *Phalaris Canariensis*, Dock, Clover, *Alsine media*, *Capsella bursa-pastoris*, and a host of others); or such as man transports with him, whether such temperate climate plants as the cerealia, fruits, and flowers of the garden or field, or such tropical forms as *Convolvulus Batatas* and yams, which were introduced into New Zealand by its earliest inhabitants;—all these, in whatever climate to which we may follow them, retain the impress of their kind, unchanged save in a trifling degree.

3. With comparatively few exceptions, plants are confined within well-marked limits, which, though often very wide, are sometimes as much the reverse; while the instances are rare of sporadic species, as such are called which are found in small numbers in widely sundered localities. These facts seem incompatible on the one hand with the theory of species spreading from many centres, and on the other with their varying indefinitely; for were it otherwise, sporadic distribution would be the rule, insular floras would not necessarily be peculiar, and similar climates would have similar, if not identical species, which is not the case.

4. A multitude of allied species of plants grow close together without any interchange of specific character; and there are instances of exceedingly closely allied plants keeping company under many modifications of climate, soil, and elevation, yet never losing their distinctive marks.

5. The individuals that inhabit the circumference of the area occupied by a species, are not found passing into other species, but ceasing more or less abruptly; their limits may meet or overlap those of one or more very similar species; when the individuals associate, but do not amalgamate.

6. One negative argument in favor of distribution from one centre only, is, that taking the broadest view of the dispersion of species, we find that the more extensive families† are more or less widely distribu-

* I am quite aware that this argument will be met by many instances of change produced in our garden plants: but, after all, the skill of the gardener is successfully exerted in but few cases upon the whole: out of more than twenty thousand species cultivated at one time or another in the Royal Gardens of Kew, how few there are which do not come up not only true to their species, but even to the race or variety from which they spring; yet it would be difficult to suggest a more complete change than that from the Alps or Polar regions to Surrey, or from the free air of the tropics to the thoroughly artificial conditions of our hot-houses. Plants do not accommodate themselves to these changes: either they have passive powers of resisting their effects to a greater or less degree, or they succumb to them.

†This rule does not extend to the Natural Orders themselves. The *Compositæ*, whose facilities for dispersion are proverbial, are amongst the most local; and the same may be said of *Leguminosæ* and *Solaneæ*, whose seeds retain their vitality in a remarkable degree: a few of their species are remarkably cosmopolite, but the greater number have generally narrow ranges.

ted, very much in proportion to the facilities they present for dispersion. Thus the most minute-spored Cryptogams* are the most widely dispersed of all organized nature; plants that resist the influence of climate best, range furthest; water-plants are more cosmopolite than land-plants, and inhabitants of salt, more than those of fresh water: the more equable and uniform is the climate of a tract of land, the more uniformly and widely will its plants be distributed.

7. The species of the lowest orders are not only the most widely diffused, but their specific characters are not modified by the greatest changes of climate, however much their stature and luxuriance may vary. Fungi offer a remarkable instance of this: their microscopic spores are wafted in myriads through the air; the life of the individuals is often of very short duration, and many of them being as sensitive as insects to temperature and humidity, they are ephemeral in all senses; sometimes appearing only once in the same spot, and remaining but a few days, never to reappear within the observer's experience. The specific characters of many reside in the diameter, form, color, and arrangement of their most minute organs, whose analysis demands a refinement of microscopic skill; yet the most accomplished and profound botanist in this Natural Order (who has favored me with the descriptions of the New Zealand Fungi) fails to find the most trifling character by which to separate many New Zealand species from European.

8. The fact, now universally conceded by all intelligent horticulturists, that no plant has been acclimated in England within the experience of man, is a very suggestive one, though not conclusive; for it may be answered, that plants which cannot survive a sudden change, might a slow and progressive one. On the other hand, plants have powers of enduring change when self-propagated that they have not in our gardens; thus I find a great difference in the hardiness of individuals of several Himalayan plants,† depending upon the altitude at which they were gathered. In these cases the species is the same, and the parent individuals were not even varieties of one another, except so far as regards hardiness; in other words, the specific character remains unaltered in spite of the change of constitution, just as the climate of one part of the globe disagrees with the human race of another, and is even fatal to it.

Such are a few of the leading phenomena or facts that appear to me to give the greatest weight to the opinion that individuals of a species are all derived from one parent: for such arguments as the New Zealand Flora furnishes, I must refer my readers to the following chapter. I would again remind the student that the hasty adoption of any of these theories is not advisable: plants should be largely collected, and

* The fact (first communicated to me by the Rev. M. J. Berkeley) of the spores of Fungi having been found by Professor Ehrenberg mingled with the atmospheric dust that has fallen on ships far out at sea, is one of the most decisive proofs of this.

† Thus some of the seedling Pines whose parents grew at 12,000 feet appear hardy, whilst those of the same species from 10,000 are tender. The common scarlet *Rhododendron* of Nepal and the Northwest Himalaya is tender, but seedlings of the same species from Sikkim, whose parents grew at a greater elevation, have proved perfectly hardy.

studied both in the living and dried states, and the result of their dissection noted, without reference to any speculations, which are too apt to lead the inquirer away from the rigorous investigation of details, from which alone truth can be elicited. When however the opportunity or necessity arises for combining results, and presenting them in that systematic form which can alone render them available for the purposes of science, it becomes necessary for the generalizer to proceed upon some determinate principles."

The considerations here adduced bear partly in favor of the single creation of species of plants, partly on their permanence of specific characters from age to age; which are different questions, although closely connected. In respect to the first, we have the mere statement of the nature and kind of evidence that is available for the support of that doctrine: for its application to a particular flora, by which its amount and weight may be tested, we are referred to a subsequent portion of the essay. So, likewise, as to the permanence of species, our general conclusions must needs depend upon a knowledge of the limits within which the same species may vary; a topic which is discussed in the following section, mainly in view of its practical botanical applications. It were well that the considerations on which the doctrine of the single, as opposed to the double or multiple, origin of species rests, should be succinctly presented at one view, that their value may be estimated. For it is obvious that the very establishment of Dr. Hooker's second and third propositions may react powerfully upon the first. If species generally are much "more widely diffused than is usually supposed," then the theory of the double or multiple origin of species, as maintained by Schouw and others, becomes all the more likely, at least until adequate natural means of dispersion are clearly shown. And if, at the same time, "species vary in a state of nature more than is usually supposed," while what we term their specific characters are permanent, though the amount and value of these characters differs greatly in different species, then it may with the more probability be supposed that many of these differences were aboriginal. And these two inferences, taken together, would perhaps necessitate the conclusion, maintained we believe substantially by Prof. Agassiz, that species are ideal, as much so as genera, and even that they may have been represented from the beginning by as many individuals, and distributed over as wide an area, as at any subsequent time. A view which, by separating the idea of genetic connection from our conception of a species, seems to leave these no ground of objective reality to rest upon. We are reluctant to rest the very basis of natural history upon *a priori* conceptions, and therefore cling to the material view, that a species is not only "a primordial form" (the definition of the late Dr. Morton), but one represented in nature by the perennial succession of essentially like

individuals, which sustain to each other the relation of parent and offspring, and which, as we may actually follow them onward from generation to generation, so we may mentally trace them backward to a common and probably local stock of homogeneous individuals, if not to a single individual or pair. This furnishes a common standard to which our empirical determinations of species may be referred, and the discordant views of different describers be rendered commeasurable; as it reduces the differences to questions of the probable value of characters in each particular case. For the fact must not be overlooked, that the greater part of the species characterized in our systematic works are only provisional determinations—more or less probable conclusions from incomplete data.

Some valuable and timely remarks are to be found in the following extract:

"It is not surprising that two naturalists, taking opposite views of the value of characters, should so treat a variable genus that their conclusions as to the limits of its species should be wholly irreconcilable. Some naturalists consider every minute character, if only tolerably constant or even prevalent, as of specific value; they consider two or more doubtful species to be distinct till they are proved to be one; they limit the ranges of distribution, and regard plants from widely severed localities as almost necessarily distinct; they do not allow for the effects of local peculiarities in temperature, humidity, soil, or exposure, except they can absolutely trace the cause to the effect; and they hence attach great importance to habit, stature, color, hairiness, period of flowering, etc. These views, whether acknowledged or not, are practically carried out in many of the local floras of Europe, and by some of the most acute and observant botanists of the day; and it is difficult to over-estimate the amount of synonymy and confusion which they have introduced into the nomenclature of some of the commonest and most variable of plants. In such hands the New Zealand genera *Coprosma*, *Celmisia*, *Epilobium*, etc., may be indefinitely extended. The principles I have adopted are opposed to these: I have based my conclusions on this subject upon a very extensive examination of living plants in all latitudes, with my attention particularly directed to the influence of external causes, not only on the general phenomena of vegetation, but also upon individuals. Added to this, I have paid a great deal of attention to variable plants, both of tropical and temperate climates, and studied them in a living state, both wild and cultivated, and also in the herbarium. The result of my observations is, that differences of habit, color, hairiness, and outline of leaves, and minute characters drawn from other organs than those of reproduction, are generally fallacious as specific marks, being attributable to external causes, and easily obliterated under cultivation. It has hence been my plan to group the individuals of a genus which I assume after careful examination to contain many species whose limits I cannot define, so that the species shall have the same relative value as those have of allied genera whose specific characters are evident. In doing so I believe I have

followed the practice of every systematist of large experience and acknowledged judgment since the days of Linnæus, as Bentham, Brown, the De Candolles, Decaisne, Jussieu, Lindley, and the Richards; names which include not only the most learned systematists, but the most profound anatomists and physiologists. I am far from supposing that the same materials of a difficult group would receive precisely similar treatment at the hands of each of these eminent men; but their results would so closely approximate as to be in harmony with each other, and available for scientific purposes: with all, the tendency would be to regard dubious species as varieties, to take enlarged views of the range and variation of species, and to weigh characters not only *per se*, but with reference to those which prevail in the order to which the species under consideration belong.

In working up incomplete floras especially, I believe it to be of the utmost importance to adopt such a course, and to resist steadily the temptation to multiply names, for it is practically very difficult to expunge a species founded on an error of judgment or observation.* There is further an inherent tendency in every one occupied with specialities to exaggerate the value of his materials and labors, whence it happens, that botanists engaged exclusively upon local floras are at issue with those of more extended experience, the former considering as species what the latter call varieties, and what the latter suspect to be an introduced plant the former are prone to consider a native. There is much to be said on both sides of such questions: the local botanist looks closer, perceives sooner, and often appreciates better, inconspicuous organs and characters, which are overlooked or too hastily dismissed by the botanist occupied with those higher branches of the science, which demand a wider range of observation and broader views of specialities; and there is no doubt but that the truth can only be arrived at through their joint labors; for a good observer is one thing, and the knowledge and experience required to make use of facts for purposes of generalization, another; minute differences however, when long dwelt upon, become magnified and assume undue value, and the general botanist must always receive with distrust the conclusions deduced from a few species of a large genus, or from a few specimens of a widely distributed plant.

I have been led to dwell at length upon this point, because I feel sure the New Zealand student will at first find it difficult to agree with me in many cases, as for instance on so protean a Fern as *Lomaria procera*, whose varieties (to an inexperienced eye) are more dissimilar than are other species of the same genus. In this (and in many similar cases) he must bear in mind that I have examined many hundred specimens of the plant, gathered in all parts of the south temperate hemisphere, and have found, after a most laborious comparison, that I could not define its charcters with sufficient comprehensiveness from a

* The state of the British flora proves not only this, but further, that one such error leads to many more of the like kind: students are led to over-estimate inconstant characters, to take a narrow view of the importance and end of botany, and to throw away time upon profitless discussions about the difference between infinitely variable forms of plants, of whose identity really learned botanists have no doubt whatever.

study of its New Zealand phases alone, nor understand the latter without examining those of Australia, South Africa, and South America. The resident may find two varieties of this and of many other plants, retaining their distinctive characters within his own range of observation (for that varieties often do so, and for a very uncertain period, both when wild and also in gardens, is notorious), and he may perhaps have to travel far beyond his own island to find the link I have found, in the chain of forms that unites the most dissimilar states of *Lomaria procera;* but he can no more argue thence for the specific difference of these, than he can for a specific difference between the aboriginal of New Zealand and himself, because he may not find intermediate forms of his race on the spot. We do not know why varieties should in many cases thus retain their individuality over great areas, and lose them in others; but the fact that they do so proves that no deductions drawn from local observations on widely distributed plants can be considered conclusive. To the amateur these questions are perhaps of very trifling importance, but they are of great moment to the naturalist who regards accurately-defined floras as the means for investigating the great phenomena of vegetation; he has to seek truth amid errors of observation and judgment, and the resulting chaos of synonymy which has been accumulated by thoughtless aspirants to the questionable honor of being the first to name a species.*

There are many causes which render it extremely difficult to determine the limits of species, and in some genera the obstacles appear to increase, the more the materials for studying them multiply, and the more we follow our analysis of them into detail; hence the botanist is often led on to an indefinite multiplication of species (with increased difficulty of determining those already established), or to a reduction of all to a few, or to one variable species. My own impression is, that the progress of botany points to the conclusion that in many genera we must ultimately adopt much larger views of the variation of species than heretofore, and that the number of supposed kinds of plants is (as I shall indicate elsewhere) greatly over-estimated; if it be not so, we must either admit that species are not definable, or that there are hidden characters throughout all classes of the vegetable kingdom, of which the botanist has no cognizance, and towards the acquirement of which, if they are ever to be revealed, all efforts in the direction in which we have been advancing appear to be vain. Could systematists as a body be accused of carrying out their investigations in an unphilosophical manner or spirit, or without due attention to all the modes of testing the validity of characters, afforded by the study of living and dried plants, by direct observation, and by experiment, there might be hopes of such a revelation; but such hopes are inconsistent with the great advances that have been made in systematic botany, which, hav-

* The time however is happily past when it was considered an honor to be the namer of a plant; the botanist who has the true interests of science at heart, not only feels that the thrusting of an uncalled-for synonym into the nomenclature of science is an exposure of his own ignorance and deserves censure, but that a wider range of knowledge and a greater depth of study are required, to prove those dissimilar forms to be identical, which any superficial observer can separate by words and a name.

ing all tended to a more perfect knowledge of the affinities of plants, we are assured have been the effect of progress in the right direction."

Without doubt the preponderating tendency of the ablest and most experienced botanists of the present day is, to cancel nominal species, and, taking a more enlarged view of specific characters, to reduce slight or varying modifications to a common type; a point not yet reached by zoologists, though probably it will be hereafter. Dr. Hooker's tendency in this direction is evidently very decided; possibly too much so. But it must be allowed that, while the botanists who multiply species unduly are always those who work upon scanty materials, or whose personal observations are limited to a single district of country; on the other hand, those who have access to the largest collections, or who have themselves botanized over various parts of the world, are for the most part as strongly inclined in the opposite direction. Now Dr. Hooker possesses both these advantages in an eminent degree. Young as he still is, no living botanist has investigated on the spot so many and so widely separated floras, and few like him have had constant access to the largest and best determined herbaria in the world. The principal danger here arises from *l'embaras des richesses.* It is hardly possible that a vast series of apparently confluent forms should receive the detailed examination which the less privileged botanist may concentrate upon his fewer materials; and much is left to the quick, almost intuitive judgment, which is liable to error, indeed, but in which the true genius of a botanist is generally disclosed.

Perhaps there is no equally well known flora which compels the botanist to allow of such wide limits of variation in so large a proportion of its species, as that of New Zealand: at least so it would appear at first sight. Yet this character seems to be exhibited more or less by insular floras generally, in which a considerable number of the peculiar species are apt to be surprisingly polymorphous, as was remarked by Bory de St. Vincent half a century ago. A. G.

(To be continued.)

Art. XXVII.—*On the relations which exist between Friction and Pressure;* by M. J. Nicklès.

In preceding numbers of this Journal I have proposed for the Mechanical Arts the use of magnetic adhesion, a principle purely physical; and it becomes necessary to ascertain the relations which exist on a horizontal plane—

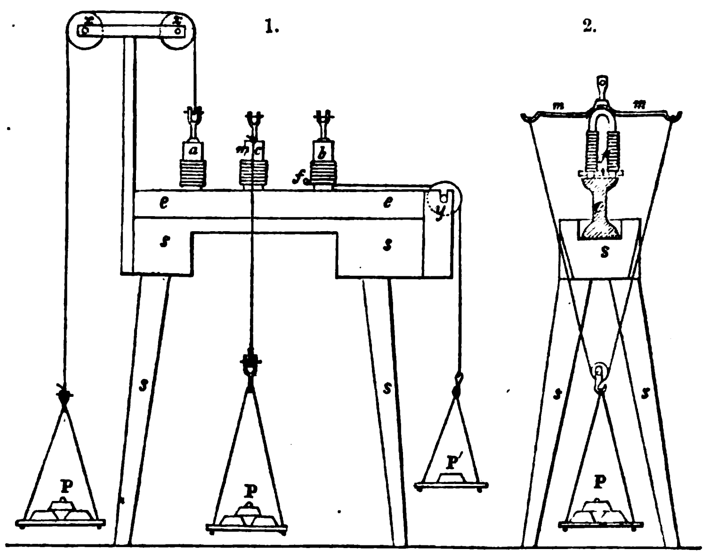
1. Between pressure and the friction of iron sliding on iron.
2. Between the pressure which one of these masses of iron converted into an electro-magnet exerts on the other, and the ef-

fort to overcome it in order to make this electro-magnet move on its armature.

The Treatise on General Mechanics by Morin does not mention the first relation, that of the friction of iron on iron. It is well known however, that in practice the number 0·33 is adopted for it.

I have reasons for thinking that the relation is the same in amount between magnetic pressure and the friction of sliding under this new condition. If the experiments which I have made on this subject do not prove that the friction is equal to one-third the pressure, they tend at least to establish an identity of relation between the two pressures and the two frictions. I have had occasion to recognize a like identity between the adhesion or friction of rolling under the influence of added weight alone and the adhesion or friction of rolling under the influence of magnetic attraction.*

The electro-magnet employed in the experiment was one of Joule's.† The helices consisted of 30 metres of copper wire, 2 mm. in section; each of its polar surfaces were 0·080 m. in length and 0·020 m. in breadth. They are shown in figures 1 and 2, which represent, in elevation and section, the apparatus as well as the



different phases in the experiments. The armature consisted of a piece of a railroad rail, *e*, placed level on a bar of wood, *s*; the magnet and armature had been carefully cleaned and polished.

A cord attached to the axis of the magnet was connected with a system of pulleys, *x*, *x*, and at the other extremity was attached

* See this Journal, vol. xvi, p. 337, Nov., 1858. † Phil. Mag., [4], ii, 450.

a scale for receiving the necessary weights, to determine the force of the magnetic attraction. This force, once known, the cord attached to the axis of the magnet was detached, and another was applied a little above the poles, at the height of a small pulley, *y*, fastened to the extremity of the rail: this cord which passed between the arms of the magnet terminated in a small rod of copper which was secured to *f*, on the arms, where it was held by rivets. A second scale, P′, smaller than the preceding, received the weights required for moving the apparatus onward, in the plane of the rail. The experiment was tried with a dry armature, and also with one wet with well water. To the weight carried, given in the table, 4 kilograms should be added, for the electro-magnet, and this is noted in the column of means. Moreover the horizontal sliding observed was a kilogram too great, this representing the effort to be overcome in causing the electro-magnet to slide when not magnetized, and this is deducted in the column of means. The results with the dry and wet rail are as follows:

Weight carried normally, or magnetic pressure.	Means.	Horizontal sliding.	Means.	Relations.
Dry rail, 231 k. 236 229	232+4=236 k.	73 k. 73 73	73−1=72 k.	0·30
Wet rail, 230 k. 224	237+4=231 k.	63 k. 67 63	64·5−1=63·5	0·27

The circuit being broken, a weight of 232 kil. was added to the magnet, by means of the bar *m m*, supported on the top of the magnet; the cord directly below carried a pulley to which a scale for the weights was adapted. The pressure was thus exerted normally on the magnet. The magnet itself weighed 4 kilograms; the charge was therefore 236 kil., which represents the pressure exerted on the armature in the preceding experiment.

	Normal pressure.	Means.	Horizontal sliding.	Means	Relations.
Dry rail, 236 k.	236 k.	236 k.	63 63	63	0·26
Wet rail, 236 k.	236 k.	236 k.	60 58	59	0·25

The results are hence as follows:

	Magnetic pressure.	Pressure by weight.
Relation of pressure to sliding on dry rail,	0·30	0·26
" " " wet "	0·27	0·25

It would have been interesting to have determined the amount of effort required to make the apparatus slide when both magnetized and weighted, to ascertain if the result would be the sum of the two slidings. But an accident which happened prevented the repetition of the experiments which were made in the month of January, 1851.

ART. XXVIII.—*Abstract of a Meteorologial Journal, for the Year* 1853: kept at Marietta, Ohio, in Lat. 39° 25′ N., and Long. 4° 28′ West of Washington City; by S. P. HILDRETH, M.D.

MONTHS.	THERMOMETER. Mean temperature.	Maximum.	Minimum.	Fair days.	Cloudy days.	Rain and melted snow in inches.	Prevailing Winds.	BAROMETER. Maximum.	Minimum.	Range.
January, ...	33·30	64	0	16	15	2·20	N., S. & S. W.	29·90	28·45	1·45
February, ...	37·10	64	11	12	16	4·92	N. & S. W.	29·55	28·85	·70
March, ...	40·66	74	14	17	14	0·88	N., S. & E.	29·70	28·80	·90
April, ...	53·00	80	24	17	13	5·92	N. N. E. & S.	29·55	28·75	·80
May, ...	61·13	88	34	16	15	3·21	S. W., S. S. E.	29·60	29·10	·50
June, ...	74·60	95	50	28	2	0·88	S. & S. W.	29·65	29·30	·35
July, ...	71·15	94	54	26	5	4·12	S. W., S. E. & N.	29·60	29·23	·37
August, ...	71·55	91	50	22	9	2·83	S., S. E. & N.	29·63	29·20	·43
September, . . .	64·33	87	40	20	10	2·80	S. S. W. & E.	29·63	29·00	·63
October, ...	48·86	82	28	22	9	4·29	N., W. & S. E.	29·70	28·98	·72
November, ...	46 17	70	20	14	16	2·83	N, S. S. E.	29·83	29 20	·63
December, ...	31·08	62	6	12	19	2·16	N., W., N. W.	29·32	28·60	1·02
Mean for the year, .	52·74			222-	143	37·04				

The temperature for the year is 52°·74, which is rather above the mean annual heat for this place. No very remarkable extremes of heat or cold have distinguished the year, although the temperature in June and July was rather excessive, rising on the last day of June to 95°, being higher than for several previous years, by two or three degrees—on a number of days it was above 90°. My thermometer had a northern exposure, on the back side of the house, protected from the reflected rays of the sun by trees. At Cincinnati the degrees of heat were considerably higher, rising to 90°; and on a number of days to 95° and 96°. In New York, by the aid of reflected heat from the brick walls and stone pavements, the temperature was more elevated than in any former year; rising to above a hundred, and causing the death of a large number of persons. Heat, alone, even to 110°, is not necessarily fatal; as in the gold diggings of California, and at Mosul, in Assyria, on the river Tigris, where the American Board of Missions have several missionaries, whose health remains good with the thermometer at 112° or higher. To render it deadly, requires some imprudent act, as drinking cold water, which paralyzes the motion of the heart, by its sudden reduction of temperature. A benevolent and all-wise creator, has regulated the laws of nations in a way most beneficial to man; so that water from springs and fountains shall be of the same temperature as the mean annual heat of the climate; thus in a measure preventing the injurious effects of cold water on the over-heated human frame in the tropical regions of the earth.

The amount of rain and melted snow was $37 \cdot \frac{04}{100}$ inches—being nine and a half inches less than in the year 1852; but this is not the lowest in the scale, as it is sometimes reduced to thirty-four inches.

The Ohio river was low all the summer, commencing early in June, as we had no rain at the summer solstice, a period when a rise in the Ohio is always expected, and seldom fails to appear. With the exception of a few feet of elevation in November, the water continued low until the setting in of winter, and closed with ice on the 25th December, when steamboats ceased running. The fall of snow was light, only about five inches near the end of the month. The lowest temperature in December, was six degrees above zero.

Winter.—The mean of the winter months was 36°·90. The coldest day was on January 29, when the mercury fell to zero. In February the lowest was 11° above. Great fluctuations of temperature occur in this month, as in 1818, the thermometer fell on one morning to 20°, and on the next to –22°; which destroyed all the peach trees in range of its influence. In, January 1851, the extreme cold being 23° below zero, killed a large portion of the bearing trees in the valley of the Muskingum; but as it was not repeated the following night, the effect was not so entirely disastrous, as some trees produced fruit in 1852, but have since died. In the latter part of January, the navigation on the Ohio was impeded by floating ice, but the river was not closed during the whole winter. There was but little snow, not more than four or five inches, and at any one time not over two inches in depth. The winter was a very mild one.

Spring.—The temperature of the spring months was 51°·55, which is rather below the mean for this climate. The amount of rain was quite abundant, especially in April, and is very important, as laying the foundation for the grass crop and other cereal productions.

Summer.—The temperature of the summer months was 72°·43, which is higher than any previous one for many years, the mean average being about 70°, or rather less. The heat commenced in June, and one of the hottest days was the thirtieth, rising to 95°. July is generally the hottest month, but in 1853 the temperature was three and a half degrees below that of June, being only 71°·15, and the latter 74°·60. At Cincinnati the heat was considerably greater than in Marietta, rising on two days to 98°, and on several to 94°. The mean for the month was five degrees above us, or 79°·32. It was six degrees above the mean for June in the last ten years. With this greatly increased range of the thermometer there was a less amount of rain than usual, there being quite a drought in June and July, which was doubtless one cause of the heat. It had a favorable effect

on the grape crop throughout the State, and especially near Cincinnati, where the vine is extensively cultivated. The fruit ripened early and without any tendency to mildew and blight, as it does in wet seasons, especially the last of June. Wheat also felt the healthy influence of dry weather, while maturing its seed; and was never of a better quality or more abundant in quantity. The heat of August was a little above July, owing, probably, to a less amount of rain, being only about half as much.

Autumn.—The mean for the autumnal months was 52°·12; and is higher than the average by one or two degrees, arising chiefly from the heat imparted to the earth in the summer months being radiated in September. The fall was very favorable to the ripening of fruits and grain, Indian Corn never being better, notwithstanding the excessive drought of June. It is a wonderful plant, and when judiciously tilled in a congenial climate, resisting both wet and dry weather above all other grains, its long fibrous roots penetrating the earth to a great depth, while its broad, tapering leaves, collect the dews and fogs from the night air, conducting the moisture to the ground, so that in droughts the soil is wet in the morning at the base of the stalks. For actual nutritive qualities, it stands at the head of all the grains, and will yield the greatest amount of bushels to the acre. Without this plant, the surplus production of beef and pork in the valley of the Ohio, would be small.

Floral Calendar.—March 3d, Yellow Crocus in bloom; 15th, Snow Drop; 22d, Bluebird building her nest; 24th, Hepatica triloba: 29th, Martin appears; 31st, Earth very dry, less than an inch of rain all the month.

April 1st, Primrose in bloom, Crown Imperial stalk sixteen inches high; 2d, Early Hyacinth, Forsythia viridissima, or Golden Bell—a new shrub from China; 7th, Hyacinth in full bloom, Narcissus, several varieties; 8th, Apricot; 9th, Peach in warm exposures, Crown Imperial; 11th, Sanguinaria Canadensis, hard frost this morning, temperature 24°; 12th, Peach generally in bloom, Pear just ready to open; 13th, Spirea Prunifolia; 17th, Sugar Maple; 18th, Cherry tree, Plum tree; 21st, Strawberry; 22d, Pear in full bloom; 25th, Apple tree, Red bud or Judas tree; 27th, Lilac in full bloom, Tulip begins to open.

May 2d, Tree peony in bloom, Chickasa plum; 4th, Quince, Black Haw; 11th, Weigelia rosea, one of the most beautiful and hardy flowering shrubs from China; 13th, yellow and white Calceolaria; 16th, Magnolia triple; 17th, Locust, Acacia robinia; 20th, Early Strawberry ripe, Burr's pine; 21st, White peony; 24th, Purple peony; 29th, Peas fit for table, Prince Albert. June 1st, Syringa fragans; Syringa Philadelphica; 12th, Ribes villosa; 17th, Cucumbers on table grown in open air with slight

protection; 20th, Red Cherry ripe; 22d, Antwerp Raspberry; 25th, Wheat harvest begins, a week earlier than common, from the heat and drought—less than an inch of rain during this month, meadows ready for mowing.

July 6th, Blackberry ripe; 15th, Sweet bough apple; 17th, Chandler Apple.

The crops of grain, potatoes and fruit good; the sweet potato was excellent; melons in great perfection, the hot and dry weather congenial to their habit; grapes were more abundant and perfect than in any former year.

Marietta, Ohio, Jan. 9, 1854.

Art. XXIX.—*On the Eye and the Organ of Hearing in the Blind Fishes (Amblyopsis spelæus,* Dekay) *of the Mammoth Cave;* by Jeffries Wyman, M.D.

The general structure of the blind fishes was described in a former number of this Journal,* but a more complete description was given in the New York Journal of Medicine, by Telkampf, who in company with J. Muller of Berlin, for the first time detected the existence of rudimentary eyes.† They are described as one twelfth of a line in diameter, round, black, destitute of a cornea, having an external layer of pigment, beneath which is a colorless membrane. No nerve was detected in connection with the eye, and the contents of the globe were not determined with certainty. Prof. Owen has described the organ as a simple eye-speck, "as in the leech, consisting of a minute tegumentary follicle, coated by dark pigment which receives the end of a special cerebral nerve."‡ Dr. John C. Dalton, Jr. has also detected the eyes and describes them as minute globular sacs containing blackish pigment, deeply imbedded in the adipose tissue of the orbit and measuring a little less than one seventy-eighth of an inch.§

Through the kindness of Mr. Charles Dean of Cambridge, and of Prof. Agassiz, I have had placed at my disposal some specimens of Amblyopsis, well preserved in alcohol, and have been able to make in some respects a more complete description than has as yet been given. I have had also an opportunity of inspecting superficially fourteen specimens varying from one inch and a half to four inches and a half in length, but in three or four only

* Vol. xlv, p. 94, July, 1848.

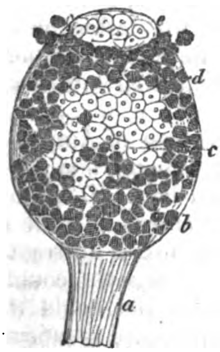
† New York Journal of Medicine, vol. v, p. 84, 1845. Dr. Dekay had previously mentioned the existence of eyes, but was evidently misled by some other appearance, since he states that eyes exist of the usual size, but are covered by the skin. He had not dissected them. Fauna of New York.

‡ Lectures on Comp. Anat., vol. ii, p. 202. See also his figure, p. 175.

§ New York Medical Times, vol. ii, p. 354.

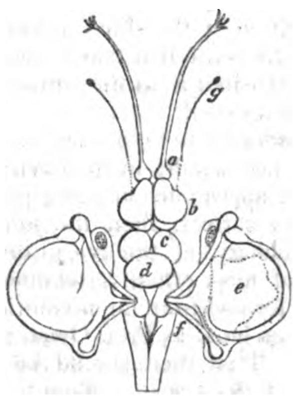
could the eyes be detected through the skin. In the three specimens recently dissected, the eyes were exposed only after the removal of the skin, and the careful separation from them of the loose areolar tissue which fills the orbit. In a fish four inches in length the eyes measured one-sixteenth of an inch in their long diameter, were of an oval form and black. A filament of nerve (fig. 1*a*) was distinctly traced from the globe to the cranial walls, but the condition of the contents of the cranium from the effects of the alcohol, was such as to render it impracticable to ascertain the mode of connection of the optic nerve with the optic lobes. A few muscular fibres were traced to the immediate neighborhood of the eye, and even in contact with it, but were not ascertained to have that regular arrangement which is seen in the more completely formed eyes of other fishes.

1.



Eye of Amblyopsis.

2.



Brain and auditory apparatus.

Examined under the microscope with a power of about twenty diameters, the following parts were satisfactorily made out: 1st, externally an exceedingly thin membrane, *b*, which invested the whole surface of the eye and appeared to be continuous with a thin membrane covering the optic nerve, and which was therefore regarded as a sclerotic; 2d, a layer of pigment cells, *d*, for the most part of a hexagonal form, and which were most abundant about the anterior part of the eye; 3d, beneath the pigment a single layer of colorless cells, *c*, larger than a pigment cell, and each cell having a distinct nucleus; 4th, just in front of the globe, a lenticular shaped, transparent body, *e*, which consisted of an external membrane containing numerous cells with nuclei. This lens-shaped body seemed to be retained in its place by a prolongation forwards of the external membrane of the globe; 5th, the globe was invested by loose areolar tissue, which adhered to it very generally, and in some instances contained yellow fatty matter; in one specimen it formed a round spot visible

through the skin on each side of the head, which had all the appearance of a small eye;—its true nature was determined by the microscope only. It is not improbable that the appearance just referred to may have misled Dr. Dekay—where he states that the eye exists of the usual size, but covered by the skin.

If the superficial membrane above noticed is denominated correctly the *sclerotic*, then the pigment layer may be regarded as the representation of the *choroid.* The form as well as the position of the transparent nucleated cells within the choroid correspond for the most part with the *retina.* All of the parts just enumerated are such as are ordinarily developed from and in connection with the encephalon, and are not in any way dependent upon the skin. But if the lenticular shaped body is the true representative of the crystalline lens, it becomes difficult to account for its presence in Amblyopsis according to the generally recognized mode of its development (since it is usually formed from an involution of the skin) unless we suppose that after the folding in of the skin had taken place in the embryonic condition, the lens retreated from the surface, and all connection with the integument ceased.

According to Quatrefages, however, the eye of Amphioxus is contained wholly in the cavity of the dura mater, and yet it has all the appearance of being provided with a lens. If his description be correct, then the mode of development as well as the morphology of the eye in this remarkable fish is different from that of most other vertebrates, since the lens never could have been formed from an involution of the skin, nor could the eye with its lens, as Prof. Owen asserts, be a modified cutaneous follicle. That there should be different modes of development of parts of the eye in different animals is by no means improbable, since we find this actually to be the case in another organ of sense, the nose. In some fishes the nostrils result from a depression or involution of the skin simply, and do not at any period communicate with the mouth; while in all of the higher vertebrates they are formed by subdivision of the primitive oral cavity. It is possible, therefore, that in Amblyopsis the lens may have been developed where we find it, and that it was never connected with the integument. Whatever views be taken with regard to its development, the anatomical characters which have been enumerated show, that though quite imperfect as we see it in the adult, it is constructed after the type of the eyes of other vertebrates. It certainly is not adapted to the formation of images, since the common integument and the areolar tissue which are interposed between it and the surface would prevent the transmission of light to it except in a diffused condition. No pupil or anything analogous to an iris was detected, unless we regard as representing the latter the increased number of pigment cells at the anterior part of the globe.

The Ear.—It is said that the blind fishes are acutely sensitive to sounds as well as to undulations produced by other causes in the water. In the only instance in which I have dissected the organ of hearing (which I believe has not been before noticed) all its parts were largely developed, as will be seen by reference to figures 2 and 3. As regards the general structure, the parts do not differ materially from those of other fishes except for their proportional dimensions. The semi-circular canals are of great length, and the two which unite to enter the vestibule by a common duct, it will be seen, project upwards and inwards under the vault of the cranium, so as to approach quite near to the corresponding parts of the opposite side. The otolite contained in the utricle was not remarkable, but that of the vestibule (fig. 3) andwhich is seen in dotted outline in fig. 2 *e* is quite large when compared with that of a Leuciscus of about the same dimensions as the blind-fish here described.

3.

Otolite.

The parts represented in fig. 2 are the olfactory lobes and nerves *a*, the cerebral lobes *b*, optic lobes *c*, the cerebellum *d*, the otolite in situ *e*, the medulla oblongata *f*, and the eyes *g*.

The parts in figures 2 and 3 are enlarged three times linear measurement.

Art. XXX.—*Additional Note to Researches on the Development of the Viviparous Aphides;** by W. I. Burnett, M.D.

Since the preparation of the paper on the development of Aphides, which appeared in the last number of the Journal, I have enjoyed the opportunity of making this series of investigations more complete by an examination of the terminal or last brood which appears at the end of autumn.

This terminal brood has hitherto been considered, as far as I am aware, to be composed exclusively of males and females, or, in other words, of perfect insects of both sexes. I was surprised, therefore, on examining the internal organs of the non-winged individuals, to find that many of these last were not females proper, but simply the ordinary gemmiparous form already described. Moreover so great was the similarity of appearance between these two forms—true females and gemmiparous individuals—that they could be distinguished only by an examination of their internal genitalia. Among the proper females there were, besides those which were filled with eggs or had already deposited them, other individuals in which the ovaries were but feebly developed, or at least, in which no mature eggs had been formed. An opportu-

* This volume, page 62.

nity was thereby afforded me to examine the structural differences between the true ovaries and their *quasi* representatives—the bud-like processes. The true ovaries had their usual, well-known structure—multilocular tubes containing nucleated cells which are probably the undeveloped germs; the bud-like processes, on the other hand, consisted of a row of cell-masses, oval and connected by a kind of peduncle, as described in detail in the preceding paper. These wide differences have, more than ever, persuaded me of the morphological dissimilarity of these two kinds of reproducing parts in this animal. It seems to me then that the real intrinsic difference between an ovum and a bud lies as deep as the conditions of sex itself, notwithstanding the latter often has, as in the present case for instance, some of the morphological characteristics of the former.

The appearance of sexless, gemmiparous individuals in the terminal brood would seem to indicate, moreover, that the conditions which determine the appearance of individuals usually exclusively male and female, are not, perhaps, referable to the fact of this being the last brood, but rather to relations of warmth and nutrition. This view is rendered more probable by the fact of the variation in the number of broods between the first and last, observed in the same species on different years—ranging between seven, nine, eleven or more. Moreover, Kyber, as quoted already in the preceding paper, by nursing continually in a warm room a collection of *Aphis dianthi*, keeping about them a summer temperature, succeeded in continuing uninterruptedly the series of sexless or gemmiparous individuals for four years. There are many other facts in insect life that indicate in like manner some direct relation between temperature and nutriment, and definite sexual development. The subject is as important as it is interesting in physiology, and these very animals will, perhaps, subserve the successful study of the primary morphological conditions of sex.

Art. XXXI.—*Correspondence of M. Jerome Nicklès, dated Paris, December* 30, 1853.

Academy of Sciences: Election of a Perpetual Secretary.—The Sciences represented in the Academy are divided into two classes: the *Mathematical* and the *Physical.* Each of these classes is subdivided into sections, consisting of six members, excepting the sections of Geography and Navigation, which have only three. The Mathematical class comprises five sections, as follows: *Geometry*, *Mechanics*, *Astronomy*, *Geography and Navigation*, and finally, *General Physics.* The class of the Physical Sciences contains six sections, namely, *Chemistry*, *Mineralogy*, *Botany*, *Rural Economy*, *Anatomy and Zoology*, and *Medicine and Surgery.* The President of the Academy is

elected for a year, and chosen alternately from the two classes. Each of the classes is represented also by a perpetual secretary, usually elected from its numbers. Thus Arago, of the Astronomical Section, was the Perpetual Secretary for the Mathematical Sciences, and M. Flourens, the distinguished physiologist is perpetual secretary for the class of the Physical Sciences.

Besides the sixty-three members, having a right to vote, there are also ten *free Academicians*, eight Foreign Associates, and 100 corresponding members, of which 45 pertain to the Mathematical class and 55 to the Physical.

The Academy of Sciences is hence organized, so as to include the most distinguished representatives of the sciences of all countries, and nominations are held very select. It is hence understood why the election of a perpetual secretary is an occurrence of great importance, and should have been with the Academy the subject of constant deliberation, ever since the death of Arago. When the decision was to be made, a large number of names were put on the roll; and finally the commission for preparing the list of candidates, fixed upon three names, taken from the Mathematical class, MM. Charles Dupin, Lamé, and Pouillet. On the proposition of M. Cauchy, the Academy added to those names that of the able geologist, Elie de Beaumont. The Academy was nearly complete in its attendance. Members detained at home for a long time past, on account of health, were conveyed to the meeting. Two ballotings took place, and on the second, the name of Elie de Beaumont was drawn out from the urn victorious, to the great disappointment of the Mathematicians, already inferior in numbers, and whose votes were divided between the three candidates.

A fifth name was obstinately put into the urn: it was that of M. de Senarmont, to whom mineralogical physics owes so much of its recent progress. For some time M. de Senarmont was the preferred candidate: but he declined the honor, excusing himself on the ground of his delicate health.

This election took place at the session of the 19th of December. M. Elie de Beaumont is fifty-five years of age. He was born on the 15th of September, 1798. One of his principal scientific claims consists in his labors along with von Buch, for the establishment of the theory of Elevation, which he has developed and extended. He is an elegant writer, and has vast literary acquirements; but it is against him that he has a feeble voice, which will hardly be heard in the Hall of the Academy, amid the agitations of the meetings, where they have been accustomed to the powerful resounding voice of Arago.

Publication of the works of Arago and Laurent.—The works of these two great philosophers are now in the press. The works of Arago will make 12 volumes in 8vo, and will consist of original memoirs, reports, historical eulogies, several of which have not yet been published, and a popular Astronomy. The printing is rapidly going forward.

Circumstances have hindered thus far the entire publication of the work left by Laurent,—a work in which this chemist, so prematurely taken from science, rapidly collected together his views on chemical theories, along with a historical account of organic chemistry, in the

construction of which branch of science he had had so large a part. The work is now in press and will soon be published. The avails of its publication are the only dowry which the unfortunate Laurent left to his widow and children, his long sickness having absorbed all his resources. As an addition to this, the French savants, headed by MM. Biot, Thénard, Dumas, Pelouze and Balard, have set on foot a subscription, to which all friends of science are invited to contribute.

Death of Theodore Olivier.—This representative of Descriptive Geometry, the student of Monge and of Hachette, the Director of the Conservatoire des Arts et Métiers, died recently while at the South for his health. Born at Lyons on the 21st of January, 1793, he early entered the Polytechnic School, where he became distinguished for his mathematical tastes, and especially for Descriptive Geometry, to which he afterwards devoted his life. He became Lieutenant of the Artillery and was attached as Professor to the School of Applied Science at Metz. In 1821 he was invited to Stockholm by the Swedish government, and established there a Polytechnic institution. On returning to France he organized, in conjunction with MM. Dumas, Péclet, and Lavallé, the Central School of Arts and Manufactures, which has produced all the distinguished engineers of the country, and which is now attended by students from all parts of Europe.

Olivier was an able Professor. His language, clear, precise and elegant, fitted him for public instruction. A chair of Descriptive Geometry was created for him at the Conservatoire des Arts et Métiers, and he filled it with distinction till the time of his death. For illustration in his lectures, he constructed moveable silk-thread models, such that the surface represented by the model could be changed from a surface of one kind to that of another. . This unique collection consists of over 50 models, made with most admirable care, precision and economy. Besides this collection, there is another likewise made by Olivier, illustrating the movements of cog-wheels, either as employed in the Industrial Arts, or as interesting simply in a mathematical point of view in the transmission of motion.

In 1815 he published an important memoir on White's theory of cog-wheels without friction from sliding. He afterwards occupied himself with the study of nodes. In 1842, he published his geometrical theory of cog-wheels; in 1843, his "Developpements de Géométrie Descriptive;" in 1845, a continuation on this subject; in 1847, its applications; in 1852, the 1st part of a new edition of his Memoirs, the continuation of which was occupying him intently when he was taken from Science, his pupils, and his friends.

M. Olivier succeeded General Morin in the direction of the Conservatoire des Arts et Métiers, and his place has been filled by M. Morin, celebrated for his fine work on Mechanics.

On the Proximate principles of Bran of Wheat.—Some years since, M. Millon, as a result of long labor, arrived at the conclusion that *bran is an alimentary substance;* that bran bread, and pilot bread ("pain de munition") was more healthy and more nutritious than white bread. This opinion has been contested, and Millon has been ironically attacked for not conforming to the regimen he recommends. But the opinion is now sustained by Chevreul, who declared his views on the

occasion of a memoir of M. Mourier on this subject. It is known too that according to Magendie's experiment, dogs could live on bran bread, whilst they died when kept on white bread. This fact which appeared so singular, is explained through the researches in question.

The inner surface of bran is covered with azotized principles which like diastase will dissolve starch, changing it into dextrine and sugar. These principles differ somewhat from diastase; still it is demonstrated that the bran acts as a ferment in fermentation, and consequently in a similar manner in digestion.

On the Ammonia contained in Rain-water.—M. Boussingault has continued at his country seat at Liebfrauenberg (Lower Rhine) his researches mentioned in the November number of this Journal. From his new investigations it appears that the rain of the country contains less ammonia than that of the city, and that the ammonia is more abundant at the beginning than at the end of a shower.

Boussingault has examined also the dew, and always found it to contain ammonia. The proportions, by several trials, were 6 milligrams to the litre; but the amount is reduced to 1·02 after a rainy day. On the 14th to the 16th of November a thick mist prevailed, so rich in ammonia, that the water had an alkaline reaction; a litre of the water contained about 2 decigrams of carbonate of ammonia. Seventy-five rains (including the dew and mist) examined by Boussingault between the 26th of May and the 8th of November, contained, as a mean, half a milligram of ammonia. The great quantity of ammonia contained in the mist appears interesting in its bearing on vegetable pathology: in fact, although ammonia in small quantity is favorable to vegetation, a large proportion would be injurious, and would show its effects especially on the leaves of flowers. Moreover, such a storm might have a deleterious influence on respiration, and especially on the lungs of persons with pulmonary affections.

Heating of Wire by the Voltaic Current.—Suppose we have a platinum wire, a, b, inserted between the electrodes of a battery composed of n elements; and we apply to two points, α, β, in the platinum wire, the electrodes of another battery also composed of n elements, two cases will be presented, according to the direction given to the currents; if the currents are contrary in direction, the intermediate space $\alpha\ \beta$ of the wire, will be instantly cooled so that you may touch it with your finger without burning it; whilst the two extremities $\alpha\ b$ and $a\ \beta$ will be raised to a temperature much higher than before. On reversing the direction of the current of the pile B, it will be, on the contrary, the portion $\alpha\ \beta$ of the wire which becomes highly heated, whilst the outer parts are reduced to a low temperature.

These facts, in which MM. de la Provostaye and Desains seem to see arguments against theory, are not out of the domain of acquired facts. These physicists admit that both currents traverse the joining wire, that they neutralize one another in one case, and add to one another in the other; and as the development of heat is regarded as due to the reunion of the two fluids, nothing prevents that the four equal streams should combine two and two in the common part of the current, and produce in all cases an elevation of temperature alike whatever may be the direction of one of the currents.

But this is not so in the experiment, and because the currents do not pass along the wires as MM. de la Provostaye and Desains regard it. By the aid of the platinum wire the two circuits of the pile form no more than one, whose tension varies with the direction of the currents. When both are propagated in the same direction, $a\,\alpha$, $b\,\beta$, the battery is arranged for quantity, and the wire reddens throughout its length. On the contrary, when the currents pass in opposite directions, the batteries A and B are coupled for tension, a condition less favorable for heating the wire. Such is the impression which has been produced in general by the reading of the note of MM. de la Provostaye and Desains. The explanation which we have just given appears to accord perfectly with the facts. I may be permitted to mention here an experiment made by me some years since, which constitutes a peculiar form of these phenomena.

I take an electromagnet which I place in a circuit, formed of four of Bunsen's pairs. The electromagnet carries 200 kilograms. If now I interpose four other elements *in an opposite direction*, I ought to have 0, according to both theories; in fact the magnetization disappears entirely. But if in place of four reversed elements I take only *two*, I find that the electromagnet retains part of its force; and, provided the mode of communication of the conductors remains the same in both cases, the batteries alone are varied. In this experiment, the currents were of contrary sign, as in the experiment $\alpha\,b$, $a\,\beta$, cited above, and by the mode of communication the pile was arranged for tension. The partial magnetisation produced in this case proceeds necessarily from a *derived* current; in the experiment with two batteries of four elements each, of contrary sign, the currents were equal in intensity, and opposite, and the power of the magnet $= 0$. In the other experiment, the difference in the two batteries caused a corresponding difference in the derived currents, and the magnet was in a certain degree magnetised.

Various Memoirs.—Among the memoirs brought before the Academy during the last three months, there are the following. One by Dr. Brainerd, of Chicago on the *means of neutralizing the poison of rattlesnakes;* presented through M. Flourens.—An interesting memoir by M. Langlois, Chief Physician at the Hospital "des Invalides," *on the action of carbonic acid on quinine and cinchonine;* M. Langlois finds that quinine left in water is rapidly dissolved under the influence of a current of carbonic acid and then crystallizes as carbonate.—Two memoirs by M. Virchow, relating to *the discovery in the human body of a substance of animal origin offering all the characters of cellulose;* the seat of this substance is in the brain and spinal marrow.—A Report by M. Pelouze on a work of some extent by MM. Rivot, Boguin and Beudant, made at the School of Mines, *on the use of chlorine in analysis.* Several new processes are brought forward: the problem is solved of the complete separation of lead and sulphur, and the analysis as exact as rapid is made of galena, by the aid of chlorine and an alkaline solution. All mineral substances are brought under consideration by the authors. They even employ their process in some cases of organic analysis, using it for example, for testing the sulphur of vulcanised india-rubber, etc.—A memoir of M. Schlösing, on testing for nitric acid accompanied by organic matters. The author who is inspector of tobacco, has applied his process to this vegetable substance.

We should also mention a fine work of M. Melloni on the magnetism of rocks, which tends to modify a little our mode of viewing magnetic polarity of many substances. But besides the abstract in the Comptes Rendus of the Academy, the paper has appeared entire in *L'Ateneo Italiano*, which is published at Paris, and whose principal end is to popularize and diffuse science in Italy.

Chemical reactions effected under the influence of High Pressure.—Towards the close of the last century, Hall fused carbonate of lime without decomposition, by means of a high temperature (21° to 23° Wedgewood). The means he used were suggested by considerations published by Hutton, respecting the influence that pressure might exert on geological phenomena. Hall used a closed vessel under a pressure of 8 atmospheres. In 1844, M. Fournet of Lyons called attention to this method, and since that time, these chemists have made frequent use of it. So also, M. Cagnard Latour, has operated on wood, converting it into a bituminous matter,—M. de Senarmont applies it to the artificial reproduction of minerals,—M. Schrötter, to changing ordinary phosphorus to red phosphorus,—M. Frankland, in his researches on the radicals, ethyle, methyle, amyle, etc.

Finally, M. Berthelot, by operating under pressure with heat, succeeds in preparing the fatty bodies by means of glycerine and the corresponding acid. We have already spoken of his labors.* He has recently made a full report of his results to the Academy, and we mention some of them. Numerous precautions are necessary in operating thus on organic matters, to avoid explosions and the burning that may follow them. The following details are from the *Journal de Pharmacie et de Chimie.*

Heating Apparatus.—We here speak only of processes carried on with a temperature above the boiling point of water, and in which an oil bath is used: oil prepared by long ebullition will support without boiling, a temperature near 400° C., and is decomposed only at a red heat.

To avoid accidents, the following are his arrangements. A square structure or vat, A, of brick, divided into three unequal parts, one above the other, contains the furnace, *b*, B being the doors, *c* the ash-pit, D the chimney; a plate of iron separates this part from the oven of the upper part. This plate has a circular hole, for receiving the boiler R, which contains the oil. This boiler is conical, at least 75 centimeters in height, so that its aperture is but little below the upper level of the vat. To prevent risk to the operator, it is necessary to close the opening of the vat with a heavy plate of iron. The space containing the oil bath is thus closed on all sides.

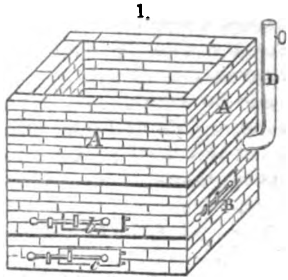
The boiler is half-filled with oil; and the temperature of the oil is shown by a thermometer, T, placed in a metallic tube which passes through a hole in the upper plate.

The closed vessels which are heated in this oil bath are almost always tubes of glass; and in order to avoid accidents from their explosion, they are enclosed in iron tubes (*s*), solid at one end, and at the other closed by a screw head. In spite of the precautions there are

* This Journal, vol. xvi, p. 100.

occasional explosions, which throw out or set fire to the oil, in consequence of the projection through the oil of gases produced by the explosion. M. Berthelot avoids this result by surrounding the iron tube by a case ("manchon") of sheet iron, closed below: the gas in this case, does not pass into the oil, but escapes above.

1.



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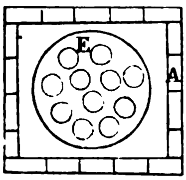

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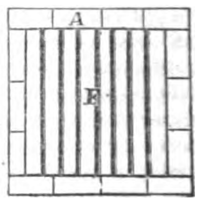

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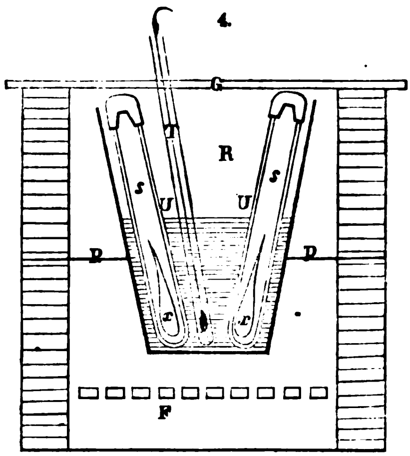

Explanation of the Figures.—1. A, brick structure, reduced to one-twentieth; *b* B, doors of the furnace; *c*, ash-pit; D, chimney.—2. Horizontal section of the structure at the height of the boiler, reduced to one-fortieth; E, boiler with its tubes.—3. Horizontal section of the same at the height of the furnace, reduced to one-fortieth; F, grating.—4. Vertical section, reduced to one-twentieth; R, boiler; *s*, iron tube with screw head; U, sheet iron case enveloping tube; *x*, glass tube contained within the iron tube; T, thermometer enclosed in an iron tube; F, grating; G, cast iron plate; P, sheet iron plate.

In recapitulation, the glass tube (x) is placed in a tube of iron (s), surrounded by a sheet iron case; the whole is heated in the oil bath, enclosed within a brick oven or vat.

The closed vessels employed are either balls, matrasses, or tubes, of glass. The first hardly resist a pressure of more than 3 or 4 atmospheres. They are closed by means of a lamp after the substance is introduced. Glass tubes are generally used; they should be hard, little fusible, unattacked by the reagents and by the heat of a lamp. The glass tubes used for organic analysis, are excellent for the purpose: They will sustain a pressure of 50 to 60 atmospheres; they resist

the tension of water, to about 270° C., and that of turpentine to above 360° C.

Preparation of the tubes.—The tubes are closed at one end with a blowpipe, and reheated in the lamp. After cooling, the solid reagents are introduced, and the open end is drawn to a point: the principal precaution requisite is to see that the tube retains the original relation between its thickness and diameter, upon keeping which, rather than upon its absolute thickness, its power of resistance depends. The tube being cooled, the liquids are introduced, and it is then closed by a lamp leaving a fine point at the extremity,—an indispensable precaution if there is an escape of gas, as the tube may then be opened readily without explosion. The quantity of liquid put in the tubes varies with the heat to which they are to be subjected. If to 360° or 400°, the tubes should not be over one-half or one-third full; and they should be short enough to be entirely covered by the oil when placed vertically in it; if to a red heat, tubes of 50 to 60 centimetres are employed, and they are filled only to $\frac{1}{100}$th or $\frac{1}{50}$th of their capacity.

Opening the tubes.—After the heat has been sufficiently continued, the tube is removed and opened, and this is the time of greatest danger. Often the tension of gas is so great, that the tubes break with the slightest shock. The following precautions are adopted.

1. If the presence of a permanent gas is suspected, it is necessary before heating the tube of glass to surround it with a triple or quadruple fold of wire gauze, extending it and doubling it over both extremities of the tube; only the point of the tube should be out. 2. After the complete cooling of the bath, the iron tube is unscrewed, and the glass tube is slid gently out and received on a linen rag. If the study of the gases developed is not an object in view, the tube is enveloped in 3 or 4 folds of linen, so as to show only the point, and then this point is broken with pliers. In some cases the gases are so abundant that explosion is unavoidable; and if the collection only of the solid ingredients is desired, the tube may be wrapped in linen and thrown on the ground: the solid substances are found mixed with the fragments of glass. If the collection of the gases is desired, other precautions are required which may easily be foreseen.

Economical Manufacture of Bi-chromate of Potash.—M. Jacquelain, Manufacturing Chemist at the Central School of Arts and Manufactures, communicates to the Société d'Encouragement, his researches on the best method of manufacturing bi-chromate of potash. The author has worked for 6 years on this point, and has published at different times papers on the subject. We give here his process, which has been patented.

The ore is (1.) heated to a red heat, and then dashed with cold water; and this is repeated a second time:—(2), then broken under iron rollers or a mill ("moulin à noix"), and afterwards with water under siliceous millstones or of iron, bringing together the millstones, so as to obtain No. 90:—(3), then made into a paste with carbonate of potash, 44 p. c., in an oven of iron, incorporating chalk 90 p. c., and calcining the mixture and drying it completely at 100° C.:—(4), thus calcined, it is put into earthen retorts, which have at the lower part a head of metal provided with an opening for admitting air previously heated, for

the purpose of oxydizing the chrome ore; the upper end of the retort is made to communicate with a chimney furnished with a register in order to regulate the escape of the hot air deprived of oxygen, and directing it upon the liquid of an evaporating boiler, for hastening the evaporation of water:—(5), after the roasting is thus completed, the material is removed to the boilers, to undergo five treatments successively with boiling water, in order to extract all the chromate of potash, using the last waters with a new supply of the roasted ore; if there is some chromate of lime in the liquid it is precipitated by means of carbonate of potash:—(6), the solution of chromate of potash is concentrated to one-third of its original volume, one-half of the potash of the chromate is saturated with sulphuric acid, which allows of a separation at once of the bi-chromate from the insoluble sulphate of potash which is precipitated; then the concentration is completed before conducting the liquid to the vats for crystallization.

M. Jacquelain expects considerable profit from this process, obtaining 425 francs for 500 kilograms of the bi-chromate, which amount may be manufactured in one day. To 100 kilograms of ore, 45 p. c. in purity, there is added successively 90 kilograms of chalk, 44 of carbonate of potash, and sulphuric acid to the value of 6½ francs; and from this 78 kilograms of the bi-chromate are obtained.

Photography.—For some time past, naturalists have sought to apply photography to zoological studies. A very successful attempt has been made by MM. Rousseau, naturalist attached to the Museum, M. Deveria, artist connected with the National Library, and Bisson Brothers, photographers. They have taken numerous photographs of animals with great success. We have seen figures of a Fungia, smaller than nature, which under a lens showed every tooth and plate of this coral with perfect distinctness. This fidelity could not be attained by the hand of man. As photography thus gives us economically what could not be obtained otherwise for any price, the Academy of Sciences, on the Report of M. Milne Edwards, have become impressed with the importance of these attempts, and have supplied MM. Rousseau and Deveria with the materials for prosecuting their labors in good earnest.

Another photographer, M. Bertsch, has presented to the Société d'Encouragement some fine photographic pictures taken under a microscope, and very much enlarged, of a tissue made up of a mixture of wool, cotton, and flax-cotton, and it is remarked that the fibres of these different substances show well their distinctive characters.

On learning the death of Arago, a French photographer, M. Claudet, established for some years in London, recalled his having taken in 1843, some Daguerreotypes of this philosopher. M. Claudet, aided by M. Lerebours, has taken from different positions pictures from the Daguerreotype, such as may be used with a stereoscope; and we now have a stereoscopic picture of the man who may be said to have acted as godfather to the Daguerreotype.

M. Niepce de St. Victor, of whom we have spoken in the number for November last, proposes to extend the use of his new process on glass, and operate with it as for an engraving on steel. When the plate has received an impression, he submits it to the vapors of fluohydric acid or else pours upon a little of the liquid acid; in the first case, the engraving is unpolished, in the second, it is deep and transparent.

Chemical examination of four new Quinquinas from the Province of Ocagna, New Grenada.—These four Quinquinas have never appeared in commerce, and it was important to ascertain their richness in quinine and cinchonine. M. Bouchardat, Chief Professor of the Faculty of Medicine and Pharmacy of the Hotel Dieu, who is preparing a monograph upon the Quinquinas both in a commercial and therapeutic view, is especially occupied with this question. The analyses of these Quinquinas were made by M. Ossian Henry, Jr. The results are given below:

No. 1. Specimen labelled Pale Red Quinquina from New Grenada.
No. 2. " " White " from the province of Ocagna.
No. 3. " " Brownish red " " "
No. 4. " " Rose red " " "

One kilogram of the Quinquina afforded, in grammes:

	No. 1.	No. 2.	No. 3.	No. 4.
Sulphate of Quinine,	0·18	0·06	——	15·50
Cinchonine, - -	0·02	0·12	——	4·00

It is seen that only one of these Quinquinas is of any interest: it is No. 4. M. O. Henry has not been able to determine the species to which the bark belongs.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Bartlett's Elements of Analytical Mechanics.*—Prof. Bartlett has presented the American student with a very complete and excellent introduction to the science of mechanics. A work in which the analytical or deductive method of investigation is systematically carried out, and is at the same time set forth in such a manner as to be intelligible to those who possess a fair knowledge of the calculus, has long been needed. We believe that Prof. Bartlett's work is the only treatise in our language in which this has been attempted. English writers on mechanics, following Poisson, begin their treatises by more or less simple demonstrations of the parallelogram of forces, and from this proposition ascend gradually to higher developments and to more general formulas. This method has undoubtedly its advantages and furnishes to a large class of minds the best introduction to a knowledge of the science. On the other hand the student who wishes to attain a complete command of general methods of investigation must pursue at one period or another in his studies a different course, and must gain that faculty of deducing particular results from general formulas which constitutes analysis. The whole science of mechanics is contained in six equations. Prof. Bartlett, following Lagrange, deduces these equations after a brief preliminary introduction giving the necessary definitions and explanations of the terms used. From these equations formulas applicable to particular cases are deduced, and the work will be found very thoroughly and completely illustrated by examples and problems. Much matter of a purely practical character is introduced, and the

treatise terminates with a series of tables of coefficients of friction, resistances to compression, tenacities, densities, &c. On the whole the work, though not free from faults, will give a real impulse to the cause of sound scientific education, and will be welcomed by both students and teachers. Mechanics is the mother of the physical sciences, and its study must precede that of any branch of physics. Any work which throws light upon its difficulties or which renders its study more attractive must be gratefully received, and regarded as a contribution to every branch of physical science. Prof. Bartlett's work promises to do good service as a carefully considered and well digested treatise upon one of the most difficult but most beautiful and useful of the sciences.

2. *Darstellung der Farbenlehre und Optische Studien von* H. W. Dové.—Under this title Prof. Dové has published the second edition of an admirably clear and well written exposition of the theory of colors. The first edition of this work appeared in 1836, and has long been out of print. The author has now materially enlarged and in some measure recast it, and has added a series of original investigations upon various optical subjects of great value and interest. The work has the rare merit of being both popular and scientific. It is perfectly intelligible to any cultivated mind, while to the physicist by profession, and especially to the teacher of physics, it is full of suggestions which serve to illustrate and adorn familiar topics. The work commences with a historical introduction concerning the propagation of light and the earlier views of its nature. From this it passes to a brief consideration of the Newtonian theory and its final and complete overthrow, and then, after allusion to the now almost forgotten vagaries of Goethe, proceeds to the main portion of the subject. The author classifies colors according to their origin, into Prismatic Colors or colors produced by refraction, Colors of Interference, and Colors of Absorption. These three classes of colors differing in origin are then separately considered, and the application of the wave theory to their explanation pointed out. The papers collected under the name of Optical Studies relate to stereoscopic experiments and apparatus, to the polarization of light, and to experiments on subjective colors. The style of the work is lively and attractive, and we are glad to learn that an English translation of it may be expected.

3. *Einleitung in die Höhere Optik von Dr.* August Beer.—With this title Dr. Beer has published a treatise upon the wave theory of light, which is in all respects the most complete and satisfactory general view of that theory which has yet appeared. It is true that almost all the best recent treatises on physics employ the doctrine of undulations exclusively. General treatises on physics, however, are rarely sufficiently full and explicit in the details of the application of theories, and usually confine themselves in a great measure at least to phenomena. The great work of Herschel on light has long been behind the age, while Radicke's Handbuch der Optik is obscure and not suited to the wants of the learner. Meantime memoirs and investigations without number have appeared, and the theory of waves may now be justly considered as almost perfect, and as entitled to rank with the theory of gravitation—in short to be considered as the second great physical generalization to which the human mind has arrived. A complete and

thorough elementary work on this branch of science has long been needed, and Dr. Beer's treatise deserves all praise. The work is divided into two parts. The first division, which is of a more elementary nature, treats of the principles of the wave theory for the simple case of isotropic media. The explanation of the attributes of color, intensity and polarization, is clearly set forth, and the fundamental laws of Catoptrics and Dioptrics are theoretically deduced. This portion of the work is in itself a very full treatise on descriptive Photics, though in a few particulars, as for example, upon the subject of circular polarization, more details would have been desirable. The chapter on interference is particularly worthy of notice, the subject being treated in the most general manner. This portion of the work closes with a description of Fessel's wave machine, for the representation of plane circular and elliptically polarized waves, as well as for the illustration of the interference of two rays. The second division treats of the analytical deduction of the laws of the motion of light, and of their experimental confirmation. The author obtains the equations for the condition of rest in the particles of the ether, and then those for a motion consisting of very small oscillations. The second chapter treats of isotropic media; the third of anisotropic media in general, and from this to the eleventh chapter inclusive the work is devoted to the phenomena of the motion of light in crystalline media. The treatise concludes with a description of the beautiful model of the wave surface in a biaxial crystal executed for Prof. Magnus, and the same we believe which was exhibited at the World's Fair in London, in 1852. In the body of the work many tables of optical constants are introduced, while the appendix gives the indices of refraction for a great number of substances as determined by different observers. This last table, however, as the indices do not refer either to definite rays in the spectrum, or to substances of definite constitution, is rather a blemish upon the work. Upon the whole, then, we regard Dr. Beer's treatise as one of the most truly important and valuable contributions to scientific literature which have appeared in recent times, and it is to be hoped that it may soon appear in an English dress, and thus become more generally accessible and useful.

4. *Die Lehre von der Reibungselektricität von* P. T. Riess.—Prof. Riess is one of the few physicists who has devoted his energies exclusively to the subject of frictional electricity—a subject which, though at one time very popular, has of late years been but little studied. The treatise before us is of a truly German character. It embraces the whole subject of which it treats, and it exhausts that subject completely and systematically. The author divides his work into six sections. The first treats of the action of electrified bodies during their insulation. The second of the action of electrified bodies during their discharge. The third of the mechanism and effect of electric discharges. The fourth of the action of the connecting wire of a battery at a distance. The fifth of the excitement of electricity, and the sixth of atmospheric electricity. The author abstains entirely from theoretical considerations, and devotes his work exclusively to phenomena and their laws. The great mass of isolated experiments with which works on electricity usually abound serves only to confuse the subject, and Prof. Riess

has most judiciously confined himself as much as possible to general principles and to the statement of numerical results. English treatises on physics of late have done little else than re-echo the opinions of the illustrious Faraday upon this branch of science, and we owe this German physicist no inconsiderable debt of gratitude for his faithful exposition and dispassionate criticism of the labors of other experimenters. The results of Prof. Riess's own labors form no inconsiderable or unimportant part of his work, and we find here reproduced his papers on the torsion electrometer, on the development of heat in the connecting wire of the electric battery, on induced electrical currents, and upon many other interesting subjects. No treatise which has yet been published upon frictional electricity, at all compares with this in real extent, fullness and completeness, and it is no small part of its merit that it has been drawn wholly from the original sources, and has not been built up upon some other and older work as a foundation. Such works as this are needed in every branch of science; they demand for their production the clearest insight, the most dispassionate judgment, and the most thorough and comprehensive knowledge. We earnestly hope that Prof. Riess' noble treatise may not long stand alone. W. G.

5. *On the Absolute Zero of the Perfect Gas Thermometer; being a Note to a Paper on the Mechanical Action of Heat;* by W. J. Macquorn Rankine, Esq., (Abstract of a paper read to the Royal Society of Edinburgh, 4th January, 1853.)—Temperature being measured by the pressure of a perfect gas at constant density, the absolute zero of temperature is that point on the thermometric scale at which, if it were possible to maintain a perfect gas at so low a temperature, the pressure would be null.

As no gas is entirely devoid of cohesion, the immediate results of experiment give only approximations to the position of this absolute zero. These approximate positions approach nearer to the true position as the gas is rarified.

The author having deduced the true position of the absolute zero from M. Regnault's experiments on atmospheric air and carbonic acid, soon after their publication, announced the result in the Edinburgh New Philosophical Journal for July, 1849, and in the Transactions of the Royal Society of Edinburgh, vol. xx.

The present paper gives the details of the method of determination which he adopted, and a copy of the diagram which he used.

The following were the results arrived at:

The absolute zero of the perfect gas thermometer is

$$\left.\begin{array}{l}274^\circ{\cdot}6 \text{ Centigrade, or} \\ 494^\circ{\cdot}28 \text{ Fahrenheit,}\end{array}\right\} \text{below the temperature of melting ice.}$$

The coefficient of expansion of a perfect gas, in fractions of its volume at the temperature of melting ice, is consequently,—

$$\text{Per degree of the Centigrade scale, } \frac{1}{274{\cdot}6} = 0{\cdot}00364166.$$

$$\text{Per degree of Fahrenheit's scale, } \frac{1}{494{\cdot}28} = 0{\cdot}00202314.$$

6. *Aridium;* by Campbell Morfit and James C. Booth, (Communicated for this Journal.)—Bahr (*Jour. Prakt. Chem.*, lx, 27,) having

stated his conviction, after recent and careful examination of the Norwegian Chromic Iron Ore, that Aridium, which Ullgren first announced as a new metal obtained by him from that mineral, is nothing more than oxyd of iron admixed with traces of phosphoric acid and oxyd of chrome; we take this opportunity of publishing that our experience is also adverse to the reality of its existence as a distinct element. Of numerous specimens of iron from various sources subjected to a searching method of analysis, (*Chem. Gaz.*, 1853, p. 413,) not one was found to contain a particle of Aridium.

7. *Note to J. D. Dana's Contributions to Chemical Mineralogy*, p. 210; by the Author.—In writing the formulas on pages 218 to 220, fractions are used as preferable often to whole numbers. Thus, $\ddot{\bar{\mathrm{A}}}\mathrm{l}\,\dddot{\mathrm{S}}\mathrm{i}^{\frac{3}{4}}$, $\ddot{\bar{\mathrm{A}}}\mathrm{l}\,\dddot{\mathrm{S}}\mathrm{i}^{\frac{2}{3}}$, $\ddot{\bar{\mathrm{A}}}\mathrm{l}\,\dddot{\mathrm{S}}\mathrm{i}^{\frac{1}{2}}$, $\ddot{\bar{\mathrm{A}}}\mathrm{l}\,\dddot{\mathrm{S}}\mathrm{i}$, are obviously more immediately appreciated and compared than the corresponding formulas, $\ddot{\bar{\mathrm{A}}}\mathrm{l}^4\,\dddot{\mathrm{S}}\mathrm{i}^3$, $\ddot{\bar{\mathrm{A}}}\mathrm{l}^3\,\dddot{\mathrm{S}}\mathrm{i}^2$, $\ddot{\bar{\mathrm{A}}}\mathrm{l}^2\,\dddot{\mathrm{S}}\mathrm{i}$, $\ddot{\bar{\mathrm{A}}}\mathrm{l}\,\dddot{\mathrm{S}}\mathrm{i}$. The old objection to fractions that atoms are indivisible is of no value. The ratios are correctly presented and these are all that analysis affords. The actual number of molecules may be for the first, for example, $\ddot{\bar{\mathrm{A}}}\mathrm{l}^{40}\,\dddot{\mathrm{S}}\mathrm{i}^{30}$ or $\ddot{\bar{\mathrm{A}}}\mathrm{l}^{24}\,\dddot{\mathrm{S}}\mathrm{i}^{18}$, etc., and Mr. T. S. Hunt has given good reason for believing that the molecules are in some such multiples. Hence $\ddot{\bar{\mathrm{A}}}\mathrm{l}^4\,\dddot{\mathrm{S}}\mathrm{i}^3$, taken as an expression of the absolute number of molecules is beyond doubt false; taken as a ratio simply, the fact is better exhibited by the fraction.

In the formulas, such as $(\frac{1}{2}\dot{\mathrm{R}}^3+\frac{1}{2}\ddot{\bar{\mathrm{R}}})\dddot{\mathrm{S}}\mathrm{i}$ or $(\frac{3}{5}\dot{\mathrm{R}}^3+\frac{2}{5}\ddot{\bar{\mathrm{R}}})\dddot{\mathrm{S}}\mathrm{i}$, the sum of the fractions is a unit; and in the first of these two formulas, they show that $\dot{\mathrm{R}}^3$ is to $\ddot{\bar{\mathrm{R}}}$ as 1 : 1; in the second, as 3 : 2. These expressions or formulas of course come under the more general formula $(\dot{\mathrm{R}}^3, \ddot{\bar{\mathrm{R}}})\dddot{\mathrm{S}}\mathrm{i}$, in which $\dot{\mathrm{R}}^3$ and $\ddot{\bar{\mathrm{R}}}$ are represented as mutually replaceable.

In the oxygen ratio of nepheline, on p. 220, 1 : 3 : 4, should be 1 : 3 : 4½.

It may be objected to the new view of the fundamental form of Tourmaline that the planes of it are less common on crystals than those of $-\frac{1}{2}R$ (the R of authors). But it is true also in Calcite that $-\frac{1}{2}R$ is one of the most common forms, much more common than R.

8. *On the Production of Crystalline Structure in Crystallized Powders, by Compression and Traction*; by Sir David Brewster, K.H., D.C.L., F.R.S., etc., (Trans. of the Roy. Soc. of Edinb., vol. xx, part 4; cited from the L., E. and D. Phil. Mag., vi, 260.)—The influence of compression and dilatation in producing the doubly refracting structure in solids of all kinds, whether crystallized or uncrystallized, which do not possess it, and in modifying that structure in all crystals which do possess it, has been long known; but with this class of phenomena, those which I am about to describe have no connexion whatever.

In the course of experiments on the *double reflexion* and polarization of light which I discovered in the *chrysammates of potash* and *magnesia*, *murexide*, and other crystals, I was surprised to find that these substances could be spread out upon glass by hard pressure, like grease or soft wax; and that in the case of chrysammate of potash and other bodies, when the powder could scarcely be distinguished from snuff, I obtained a transparent film, exhibiting the phenomena of double reflexion and polarization from its surface as perfectly as if I had been using a large crystal.

In subsequently repeating these experiments, and examining under polarized light the film thus produced by compression and traction, I was surprised to observe that the streaks and separate lines of the film, as well as the film itself, had regular axes of double refraction, as if they were regularly crystallized portions of the substance under examination. These streaks and capillary lines, which were often of extreme minuteness, did not appear to consist of insulated particles merely dragged into a line; but when the substance possessed the new property in perfection, the lines of polarized light were continuous, and the crystallographic as well as the optical axes of the particles were placed in that line. In other cases, where the experiment was less successful, the insulation of the particles was easily recognised, though the general mass of them was crystallographically arranged.

In making these experiments, the natural crystalline powder, or the particles of the crushed crystal, may be placed either upon a polished glass surface or upon a piece of glass ground on one side. In those cases where the substance is soft, the polished surface is preferable; but when the powder is hard, and considerable pressure necessary, it is better to place it upon the ground surface of a piece of glass, as the particles are detained between its minute elevations, and submit more readily to the combined force of pressure and traction. When the powder is thus placed, I take a polished and elastic knife, and with its broad point I compress and drag the powder in a given direction till there is the appearance of a polished surface on the compressed substance. In general, I have used both the smooth and the rough glass, and have frequently obtained results with the one which were not given by the other.

If we now place the plate of glass in a polarizing microscope with the field dark, we shall find that the streaks and lines produced by traction have, in certain substances, regular neutral and depolarizing axes, as if they were prismatic crystals of the substance under examination. With the *chrysammate of magnesia*, a red powder with specks of yellow reflected light, the phenomena are peculiarly splendid; the natural colors of the substance, which vary greatly with the thickness of the streaks and films, being combined with the different tints which they polarize. As the crystals of this substance possess unusual reflexion, this property is displayed in the crystallized streaks produced by traction; and the superficial colors which they reflect vary with the azimuth which the plane of incidence forms with the plane passing through the axis of the prism.

The remarkable property which I have now described I have found, in a greater or a less degree, in the following crystals:—

Chrysammate of magnesia,	Palmic acid.
" of potash,	Amygdaline.
Hydro-chrysammid.	Tannin, pure.
Murexide.	Quinine, pure.
Aloetinate of potash.	" acetate of.
Aloetinic acid.	" sulphate of.
Oxamide.	" muriate of.
Palmine.	" phosphate of.

Quinine, citrate of.	Cerium, oxyd of.
Cacao butter.	Parmeline,
Veratric acid.	Lecanorine.
Esculine.	Indigo red.
Theine.	Ammonia, oxalate of.
Silver, cyanid of.	" sulphate of.
" acetate of.	Soda, chromate of.
Platina and magnesia, cyanuret of.	Lead, iodid of.
" and barytes, cyanuret of.	Strychnine, sulphate of.
" potassium, cyanuret of.	" acetate of.
" ammonia, chlorid of.	Soda, native nitrate of.
Potash, oxymuriate of.	Berberine.
" chromate of.	Mucic acid.
Urea, nitrate of.	Solanine.
Sulphur.	Asparagine.
Camphor.	Mercury, oxymuriate of.
Cinchonine.	Isatine.
" sulphate of.	Alizarine.
Meconic acid.	Manganese, sesquioxyd of.
Brucine, sulphate of.	Lead, protoxyd of.
Morphia, acetate of.	Tungstic acid.
Tin, iodid of.	Chromo-oxalate of potash.

In submitting other crystals to the influence of compression and traction, I have found great numbers which do not exhibit the least trace of transparent streaks and lines, the separate particles being merely dragged into lines, and exhibiting only a quaquaversus polarization. On the other hand, there is another class of crystals whose powders or particles are forced into distinct and transparent streaks and lines, in which the individual particles have a quaquaversus polarization and no trace of a prismatic arrangement. As these crystals have a peculiar relation to those in the preceding list, I shall enumerate the most important of them in the following table; that is, those in which the powder has been dragged into transparent and continnous streaks and lines, resembling externally portions of a solid body; for it is only by a comparison of the physical, or perhaps the chemical qualities of the two classes of bodies, that we can expect to explain the new property which is possessed only by one of them.

Hydrate of potash, pure.	Soda, acetate of.
Indigotic acid.	Mercury, prussiate of.
Urea.	" muriate of.
Citric acid.	" sulphuret of.
Silver, nitrate of.	Barytes, acetate of.
Meconine.	Zinc, chromate of.
Naphthaline.	" sulphate of.
Soda, nitrate of.	Cobalt, sulphate of.
Potash and copper, sulphate of.	Magnesia and soda, sulphate of.
Soda, phosphate of.	Borax.

As both compression and traction are necessary in producing the transparent streaks and lines in both classes of the substances I have

enumerated, it became interesting to ascertain what effect was produced by each of these forces acting separately, and which of them was chiefly influential in developing the doubly refracting arrangement exhibited by the substances that possessed it.

The force of compression was undoubtedly the agent in forcing the separate particles into optical contact, while that of traction drew them into a line, and tended to dilate the film in the direction of that line, and to draw its particles from each other; or overcome their attraction of aggregation in that direction. It is quite possible, too, that these forces may have exercised some influence in modifying the doubly refracting structure of the substance under examination; but as such a question has no bearing upon our present subject, I have not attempted its solution.

Without expecting any very interesting result, I submitted to examination several of the soft solids which possess double refraction, such as *bees' wax*, *oil of mace*, *tallow*, *and almond soap*. The last of these substances, though in common use, is a very remarkable one. Owing to its particles not being in optical contact, it has a fine pearly lustre, and may be drawn out into long and slender strings. Upon laying a portion of it on glass, it has a quaquaversus polarizing structure, with a tendency to form circular crystals; but when it is drawn out into strings, and laid upon glass, these strings have neutral and depolarizing axes, like the streaks formed by compression and traction. In the present case it is by traction alone that this crystalline arrangement of the particles is produced.

In *oil of mace* and *tallow* a similar effect is produced by compression and traction. With bees' wax the depolarizing lines are still better displayed, and the effect is considerably increased by mixing the bees' wax with a small quantity of rosin.

As the preceding experiments place it beyond a doubt that the optical or crystallographic axes of a number of minute particles are dragged by pressure and traction into the same direction, so as to act upon light like regular crystals, it became interesting to discover the cause of phenomena which certainly could not have been anticipated from any theoretical principle with which we are acquainted. The primary force, and indeed the only apparent one exerted in these experiments, is a mechanical force; but it is not improbable that a secondary force, namely, that of electricity, may be generated by the friction which accompanies the forces of pressure and traction. That such a force is excited with certain crystals will not admit of a doubt; but even if it were developed in every case, this would not prove that electricity was the agent in producing the phenomena under consideration. In subjecting asparagine to compression and traction, I observed, upon placing it in the polarizing microscope, that its particles were moving about under an electrical influence, but in no other case did the same phenomenon present itself to me.

The experiments with soft solids, but especially those made with the almond soap, exclude the supposition that the electricity of friction is the cause of the crystalline arrangement of its particles; though it is not improbable that the sliding of the particles upon one another, as produced by traction, and their mutual separation, as in the case of

tearing asunder mica or paper, may produce enough of electricity to have some share in giving the same direction to the axes of the particles.

When a portion of almond soap is placed upon glass, the axes of its particles lie in every direction, and have no tendency to assume the crystalline arrangement. The forces of aggregation emanating from three rectangular axes, are not strong enough to overcome the inertia, as we may call it, arising from the natural *quaquaversus* adhesiveness of the substance, and from the water interposed between its particles; but when the portion of soap is drawn out into a thread, these resistances to crystalline arrangement are diminished; elementary prisms, or crystals whose length is greater than their breadth, will have a tendency to place their greatest length in the line of traction; and all lateral obstruction to the play of its natural polarities being to a great extent removed, when the substance is drawn into a capillary thread the molecules will have free scope to assume their natural crystalline arrangement.

The application of these views to the powders and particles of hard crystals is not so readily apprehended; but when we consider that the pressure brings the molecules of the substance within the sphere of their polarities, and that the force of traction reduces the compressed film into separate streaks and lines, like the threads of the almond soap, we have reason to conclude, that, even in hard substances, the atoms, when released from their lateral adhesions and brought into narrow lines, will assume the crystalline arrangement.

In the course of these experiments, I have observed in some cases where the crystalline arrangement was very imperfectly effected, a tendency in the atoms to quit their position, as if they were in a state of unnatural constraint, like the particles of silex and manganese in certain kinds of glass which experience a slow decomposition. If this should prove to be the case, either partially or generally, which time only can show, it will doubtless arise from the non-homologous sides of the elementary atoms having come into contact; a condition of the crystalline lines perfectly compatible with the existence of neutral and depolarizing axes, and of the colors of polarized light, provided that the non-homologous sides in contact deviate from their proper position, either 90° or 180°. If we cut a plate of mica, for example, into two pieces, and combine them by turning one of them round 90° or 180°, polarized light transmitted through them perpendicularly will exhibit the same colors as when they were in their natural position, and also the same neutral and depolarizing axes. If the polarized light is transmitted obliquely, the *hemitropism* of the combination, as we may call it, will be instantly discovered by the difference of color of the two plates.

II. Geology.

1. *On the Gold Fields of Victoria or Port Philip;* by H. G. Wathen, Esq., Mining Engineer, (Quart. Jour. Geol. Soc., vol. ix, p. 74, communicated by P. N. Johnson, Esq., F.G.S.)—*General Description, Geographical and Geological.*—A chain of mountains, or rather a series of distinct ranges, runs round the southeastern corner of Aus-

tralia, nearly parallel to the coast line, and from fifty to eighty miles from the sea, forming part of the main chain of the continent, and rising at its highest summit, Mount Kosciusko, to 6500 feet above the sea-level. This mountain chain in Victoria consists of clay-slates, mica-slates, and flinty slates, in successive steps, forming collectively, a recurring series.

The slates are nearly or quite vertical, with a north and south strike, and are intersected by numerous quartz-veins, running at an acute angle with the slates. Vast plains of trap, forming high table-lands, run up to the base of the mountains and probably cover their lower slopes. It is in the valleys and gullies of these mountains, and not very far from their junction with the trappean plains, that the rich deposits of gold are found. The auriferous districts are commonly broken by deep valleys and precipitous steeps. The hills are thickly forested; the soil poor and gravelly, and the surface strewn with angular fragments of white quartz.

Gold-fields.—Gold has been found at several points remote from each other along this zone of mountains; but incomparably the richest deposits hitherto opened in the Colony of Victoria, and indeed in the entire continent, are those of Ballarat and Mount Alexander, the latter far exceeding the former in extent and richness, while even the former is said by Californian miners to surpass in richness and yield all that they have witnessed in that region of gold.

Mount Alexander gold-field.—Mount Alexander lies in latitude 37° South, longitude 144° 20′ East, and is about 75 miles north-west of Melbourne. It was named by the first explorers Mount Byng, and is thus distinguished on many maps. It is a rocky granitic mountain, with a rugged flattened outline, towering some hundreds of feet above the summits of the forested ranges of slate-rocks which surround it, and of which it is the centre and nucleus.

The enormous amount of gold which this district has yielded has chiefly been derived from two valleys with their lateral gullies and ravines. These valleys are known by the names of the streams or "creeks" that run through them. One of these, Forest Creek, takes its rise in Mount Alexander itself; the other, Fryer's Creek, has its source in the high and broken ranges of slate that environ the Mount. Both Creeks are tributaries of the River Loddon. The workings extend five or six miles along the valley of Fryer's Creek, and about ten along that of Forest Creek. At Fryer's Creek gold has been found in large quantities beneath the bed of the stream, near its source, in the upland gullies. Forest Creek, on the contrary, appears to grow barren as it approaches the higher granite country, where it originates. On the banks of the River Loddon gold is found in small quantities, lodged in the crevices of the rocks, but no large deposits have been met with on the river, and even the stream into which Forest Creek runs, though itself only a feeder of the Loddon, proves far less rich than Forest Creek and its mountain affluents. In short, it would seem that the gold had been arrested in the small mountain ravines and gullies, and was never washed down to the large streams. Auriferous sands on river-banks or in alluvial plains are unknown in the Colony. When within 12 inches of the surface, the gold is disseminated in a quartzose

gravel; when found at lower depths, it is almost always imbedded in clay, usually of a very tenacious kind.

Ballarat Gold-field.—The Ballarat gold-field, which is about fifty-five miles north-west of Geelong and Port Philip Bay, lies at the junction of the slates with the trappean country, about seven miles from an extinct and now forest-grown volcano, known as Mount Boninyong. A second similar black volcanic mount rises out of the slate ranges, about ten miles due north of Boninyong. Granite crops out in small patches between the two Mounts.

This auriferous tract is united to that of Mount Alexander by a succession of similar dark forested ranges, rough, rocky, and sterile, strewn over with quartz, and consisting of the same series of micaceous, flinty, and clay-slates.

Volcanic tract.—At the western base of these sombre hills lies a large tract of the most fertile and beautiful country—the garden of Australia Felix—the rich soil of which is the product of decomposed lava. These park-like plains, sprinkled over with groups of trees, are diversified by numerous domelike lava hills, without trees, but of the richest verdure. I have counted no less than twenty-four of these remarkable bold hills from the summit of one of them. The south and east sides are commonly steeper than the others. They are usually flat at the top; but in one of them, which I named Mount Lyell, after the illustrious geologist, there is a small crater, which had the reputation of being fathomless, but which I found to be in fact about 50 feet deep, consisting of an upper cup or crater about 15 feet in diameter, contracting below into a narrow rocky shaft or well, 30 feet deep, and three or four wide. The freshness of the traces of the flow of the lava, which is of a soft and perishable kind, indicates that the epoch of igneous action cannot be very remote. Altogether this volcanic region forms a most interesting subject for geological research and speculation.

Quartz Veins.—The sedimentary rocks are traversed by numerous veins of quartz, about 3 feet wide, of unknown length, in some districts descending to an unknown depth, in others not more than three or four feet deep. These veins or dykes run N. and S., or N. N. E. and S. S. W., and always make an acute angle with the laminæ of the slates. They seem to be the original matrix of all the gold found in the valleys and creeks. The quartz is often intersected by many joints and narrow fissures, filled with a red ferruginous earth, in which particles of gold are disseminated. Gold is also found implanted in the quartz itself, and attached to the sides of its cavities. These auriferous veins were discovered and wrought before the alluvial gold deposits or "Diggings;" and as they were worked with profit by the rude means at the command of the untrained diggers, they would doubtless well repay those who may operate upon them with all the appliances of modern European mining, so soon as the existing social excitement shall have subsided and wages shall have fallen from their present extravagant height. The first gold-working in the Colony was on a quartz vein running through one of the trappean plains so common in this country. The auriferous qnartz is not milk-white, but has a delicate yellowish color, and a waxy lustre. That which is much broken and fissured appears richer than

the more hard and solid. Sometimes large boulders of quartz are found deep beneath the surface, in the midst of auriferous clay; but it is remarkable that in such cases the quartz boulders rarely or never contain gold, however rich the clay it lies in may be.

These quartz veins appear, as already said, to be the original seat and matrix of the gold. The slate rocks having undergone continual degradation during the lapse of ages, the quartz veins also have suffered decay and disintegration when their enclosing walls no longer existed; the joints and fissures in the veins of course aiding the destructive process. Hence the gold disseminated in their mass became liberated, and, together with the materials of the quartz veins and slate rocks, were washed down into the gullies and creeks, where the latter formed the beds of clay, gravel, &c., now found in these depressions; whilst the particles, grains, and nuggets (or pepites) of the precious metal by their own weight descended to the lowest of the permeable beds, and into the chinks and cavities of the slate rocks beneath, forming the "pockets" of the miners.

Mode in which the Gold is deposited.—Occasionally the gold grains are seen strewn on the top of the soil. Sometimes they lie 30 feet beneath the surface, and may also be met with in other localities at every intermediate depth. The "Diggings" may however be conveniently classed into two divisions: first, "Surface Workings;" second, "Pit" or "Hole Workings." In the first the gold is either found lying on the surface or (much more commonly) is diffused through the gravelly soil to the depth of six or 12 inches, beneath which is usually a stiff red clay containing little or no gold. These deposites are commonly on the sides and crests of hills adjoining rich gullies. The second or deeper class of workings consists of pits or "holes" from three or four to 25 or even 30 feet deep. In these deposits the gold is almost always imbedded in a stiff clay. When any spot is rich on the surface no gold will be found immediately beneath, and *vice versâ* when rich below it will yield nothing on the surface.

These deeper or Pit Workings are of three kinds:—

1. In the channel of an auriferous creek, at points where the stream is impeded by bars of vertical slates traversing the valley, gold is often found by sinking through the alluvial mud and earth down to the rocky channel beneath. Here the gold is lodged in a grey clay, which fills the chinks and fissures of the slate rock whence the miners extract it by means of knives, spoons, shears, or any other tool they can meet with. Where the bed of the stream expands into an alluvial flat, the auriferous deposit will also increase in width. Such was the first-worked "Golden Point" of Mount Alexander, a local expansion of the bed of Forest Creek. If it should happen that the existing Creek has left its original channel, the run of the gold deposit then quits the modern Creek and follows its ancient channel. These workings in the beds of creeks are commonly from three to ten feet deep. They were the first undertaken at Mount Alexander. The deposits are richest at points where the stream has been impeded in its course, either by frequent sinuosities or by being crossed by a bar of slate as already mentioned.

2. A second kind of deep auriferous deposit is met with in the dry gullies which descend from the higher ranges to the main valleys,

generally with a gentle inclination, from a quarter of a mile to a mile in length. These gullies in some spots are narrowed by the converging hills and sometimes expand into open slopes or flats. Here the gold is commonly found, at from 10 to 20 feet beneath the surface, in a reddish or yellowish clay, lying either upon the fundamental rocks, in the chinks of the vertical slate, or else upon a thick tenacious white or yellow clay, known by the miners as "Pipe Clay." This is sometimes of unknown depth, and sometimes passes imperceptibly into the vertical laminæ of soft micaceous slate. In some of these gullies there is a continuous line of workings half a mile in length. The richest deposit is always found in what appears to be the *ancient channel* or bed of the gully, where the opposite slopes of the rocky gully meet deep beneath the overlying strata of gravel and clay. The breadth of the area which yields gold is usually not more than a few feet, rarely if ever more than a few yards. The superior strata clearly owe their origin to running water. They differ much in composition in different localities. They may be hard or soft—may consist of tenacious clay or of sandy gravel. When first turned up they almost always are of some bright hue of red, yellow, or white; but this soon fades away on exposure to the air. It is remarkable, that these gullies are, with scarcely an exception, on the south side only of the valley.

3. The third kind of deep workings are those on the sides and crests of the low rounded hills or acclivities at the sides of the auriferous gullies. It often happens that the width of an auriferous gully is contracted before it falls into the main valley by spurs from the lateral hills, which, protruding from either side, form a kind of gateway to the gully. In such localities the gold deposit was found to continue across the gully up to the foot of these enclosing hills, and thence up their sides to the rounded crest, where the rich field commonly ceases. In the gully below, the gold-bearing deposit may be at a considerable depth. At the crest of the hill it will also be deep; but intermediately, at the foot of the hill, the "holes" will be perhaps only two or three feet deep, or the gold may in this intervening space be scattered in the surface gravel; so that a section through the hill and gully below would exhibit the gold deposit.

The alluvial strata on the sides and tops of these hills have a general conformity to the present surface, but are extremely irregular, so that two pits, a few yards apart, may present two totally different sections; as though the beds had been deposited by means of strong conflicting eddies and currents. They consist sometimes of stiff red and yellow clays, like those in the gullies; but there also frequently occur beds of a very hard reddish concrete, composed of quartz and slate pebbles. At Ballarat large boulders of quartz, two or three feet in diameter, were found imbedded in the auriferous clays, and, more rarely, detached masses of a conglomerate of fragments of lava, trap, and quartz, imbedding rounded pieces of gold. At these workings the rich "pockets" of gold were commonly associated with a bluish clay, running in irregular veins and patches. So rich was this clay, that 9 lbs. weight of gold have been taken from a single tin-dishful of it, about fourteen inches in diameter and five or six inches deep.

Enormous amounts of gold have been taken from some of these rounded alluvial hills. The yield, however, is not so uniform as in the gullies; a rich spot and a barren may often lie close together. In these deposits, as in those of the dry gullies, the gold is usually imbedded in red or yellow clays, lying immediately on the fundamental slates, or on the "pipe clay." When the gold-yielding clay lies on the rock, small lumps or nuggets of gold will sometimes slip down between the vertical slates.

In conclusion, the methods of separating the gold from the gravels and clays are the same as those used elsewhere in New South Wales and California, and vary of course according to the means at the command of the miners*.

2. *On the Structure of Agate*; by THEODORE GÜMBEL, (Leonhard u. Bronn's N. Jahrb. f. Min., 1853, pp. 152–157; Quart. Jour. Geol. Soc., ix, 259.)—The curious and beautiful appearances afforded by agates have long made them of primary importance in mineralogical cabinets; but until of late years particular attention does not seem to have been paid to the internal structure of these bodies. Dr. J. Zimmerman is the first, to my knowledge, who observed† that the different varieties of quartz—as amethyst, calcedony, carnelian, jasper—formed the concentric layers of the nodules, which were either hollow or occupied with crystals.‡

In the Jahrbuch of the Imperial Geological Institute of Vienna for 1851,§ is a very interesting memoir on the interior structure of agates by Prof. Dr. Franz Leydolt, where he states that, on being submitted to the action of fluoric acid, the amorphous portions are dissolved before the crystalline layers or bands; and the agate surface being thus prepared, it is made use of in printing an exact copy of itself. The six beautiful plates accompanying the memoir perfectly exemplify Prof. Leydolt's views, and show—*first*, that the parts towards the outer surface consist of several spherules variously combined, which are composed of layers of diverse character; *secondly*, that towards the centre of the nodule is a large mass of amethystine quartz, the nucleus of the latter again being formed of very small concentric spherules.

In the *Jahrbuch für Praktische Pharmacie*, &c. 1852, is a short paper of mine on the rotatory motion of matter in the amorphous condition, in which I have shown, that in a sphere of blown glass the material is not homogeneous, but consists of lamellæ overlying one another at varying angles and confusedly distorted. As in the thin pellicle of blown glass the intimate structure of the soap bubble is as it were fixed, so I sought to make further researches by means of experiments on molecular movement, such as can be observed in so many instances. One of the most successful experiments was the use of melted stearine with which very fine graphite had been mixed, spangles of which easily

* Besides the Ballarat and Mount Alexander gold-fields, "diggings" have been opened at Mount Blackwood and on the Moorabool River, near Ballarat; on the Plenty and Yarra Yarra Rivers, N. E. of Melbourne; on the Mitta Mitta River and Lake Omeo, in the N. E. part of the Colony; as well as at several points along the eastern portion of the Boundary-line between Victoria and New South Wales.

† In his Taschenbuch für Mineralogie.

‡ See also Mr. Hamilton's Paper on the Agate Quarries of Oberstein, Quart. Jour. Geol. Soc., vol. iv, p. 215.—Transl.

§ Vol. ii. No. 2, p. 124.

indicated the intimate motion of the mass. By this easy experiment it appears that in some parts there was a strong tendency to the formation of spheres, and which existed even in the interior of the larger spheres, giving rise to smaller spherules.

III. Botany and Zoology.

1. *Fungi Caroliniani Exsiccati. Fungi of Carolina, illustrated by natural specimens of the species;* by H. W. Ravenel, Corr. Mem. of Acad. Nat. Sciences, Philadelphia, &c. Fasc. II.—We are glad to see Mr. Ravenel encouraged to so early an issue of another century of Carolina Fungi. In the quantity and quality of the specimens, this fasciculus is equal and perhaps superior to the first. The paper is superior, though that of the former was certainly good enough. In these respects the collection thus far is equal to anything of the kind we are acquainted with. Rather more than half of the species are exclusively American, some of which are now for the first time published.

We much regret the necessity of finding any fault with a work of such general excellence and accuracy. But we do not remember to have seen a work containing so large a proportion of typographical errors. There are also some unfortunate specimens of Latinity. The first volume contained much fewer blemishes of this kind. In a work containing so little letter-press, and so easy of correction, such errors are the less excusable. M. A. C.

2. *Comparative Anatomy*; by C. Th. v. Siebold and H. Stannius. Translated from the German, and Edited with Notes and Additions, recording the recent progress of the Science, by Waldo I. Burnett, M.D. Vol. I, *Anatomy of the Invertebrata;* by C. Th. v. Siebold. Boston, 1854: Gould & Lincoln.

This work, promised many years since, has at length appeared, and in a form creditable to all parties concerned. As a treatise upon Anatomy, this work of von Siebold and Stannius is well known to naturalists, and has rightly been regarded as the most complete and comprehensive that has ever been published; and its appearance in an English dress together with the additions of the translator, will be regarded as a contribution of no little value, especially to American and English students.

In order to illustrate in brief some of the distinguishing features of the work we cannot do better than quote a paragraph from the Notice of the Translator and Editor. He says: "In the text will be found a lucid yet succinct exposition of the anatomical structure of organs, arranged as far as practicable under distinct types. The details on which this typical summary is based, are comprised in notes which are as remarkable for their erudition as for their copiousness; indeed, the utmost care has been taken in the literature of the various subjects treated, and the student will find here the most reliable and at the same time the fullest reference to the bibliography of nearly every subject in comparative anatomy. In this way, the work as a whole furnishes a complete dictionary of the science, and will prove invaluable even as a work of suggestion and reference, to those who would pursue any special line of inquiry and research in this department."

Dr. W. I. Burnett, who is well known by his own thorough and minute researches in many points in anatomy, has performed his duties as translator with great fidelity, and has also increased largely the value of the edition by his extended additions to the notes and references. He has thus included the most recent results of foreign researches, and has made the work more complete in its exhibition of American Science.

The typographical appearance of the volume is excellent. We have seen no scientific work published in this country that is more creditable in its appearance. It is filled with technical words, and the bibliographical references in the notes are given, as to their titles, in the language in which the works referred to are written. This technicality of terms and the intermixture of some half dozen different languages, must have rendered the work one of great difficulty to the printer. The type is clear, distinct, and apparently new, and the correct relation between that of the text and the notes gives the page a very handsome aspect. As to the orthography of nomenclature, the work will be found to have no superior, for it is evident that the most scrupulous care has been taken in the correct and most approved spelling of scientific terms. We make these remarks, because in these days of rapid book-making, where all is sacrificed to cheapness of sale, it is really pleasant to meet with volumes the comeliness of whose pages delights the eye, equally as their science instructs the mind. We hope that we shall see more in equally excellent style, and from the same enterprising publishing-house, Messrs. Gould & Lincoln, of Boston.

The second volume of this encyclopedian anatomical work is, we learn, in press, and will be issued as soon as practicable. It will comprise the anatomy of the Vertebrata, and we wish Dr. Burnett no better success than that it may prove the mate of the present volume.

G. N. P.

IV. Astronomy.

1. *New Planet, Euterpe* (27).—Mr. J. R. Hind announces the discovery of another asteroid on the evening of Tuesday, Nov. 8, in *Taurus.* It is less brilliant than stars of the ninth magnitude. The following elements were calculated by Mr. Charles Mathieu from observations of Nov. 8, 12, and 17th.

Epoch, 1853, Nov. 8·395103.

Mean anomaly,	336° 1′	
Long. perihelion,	84 2	Mn. eqnx.
" asc. node,	96 16	Nov. 8.
Inclination,	1 26	
Excentricity,	0·18902	
Semi-axis major,	2·26007	
Siderial revolution,	3·397 years.	

2. *New Comet,* (Astron. Jour., 66.)—Mr. Robert Van Arsdale of Newark, N. J., discovered a new comet on the 25th of November in the constellation *Cassiopeia.* Its position Nov. 25 at 6^h 50^m P. M., was R. A. 2^h 7^m, Dec. $+60°$ $12'$. Nov. 30 at meridian passage 9^h 5^m, R. A. 1^h 44^m, Dec. $+54°$ $10'$.

V. MISCELLANEOUS INTELLIGENCE.

1. *Contributions to Meteorology,—Mean results of Meteorological Observations, made at St. Martin, Isle Jesus, Canada East, (nine miles west of Montreal) for* 1853; by CHARLES SMALLWOOD, M.D.— (The geographical co-ordinates of the place are 45° 32′ N. Lat., and 73° 36′ W. Long. Height above the level of the sea, 118 feet.)

Barometric Pressure.—The readings of the barometer are all corrected for capillarity, and reduced to 32° F. The means are obtained from three daily observations, taken at 6 A. M., 2 P. M., and 6 P. M.

The mean height of the barometer in January was 29·757 inches, in February 29·654, in March 29·584, in April 29·654, in May 29·644, in June 29·648, in July 29·479, in August 29·598, in September 29·325, in October 29·500, in November 29·637, and in December 29·456 inches. The highest reading was on the 28th of January, and indicated 30·382 inches; the lowest was also in January, on the 24th day, and was 28·638 inches; the yearly mean was 29·578 inches, the mean yearly range was equal to 0·993 inches. The atmospheric wave of November was marked by its usual fluctuations, the final trough terminated on the 30th day.

Thermometer.—The mean temperature of the air, by the *standard* thermometer, was in January 16°·68, in February 16°·36, in March 29°·68, in April 41°·36, in May 56°·34, in June 68°·66, in July 68°·04, in August 68°·61, in September 58°·04, in October 43°·37, in November 31°·00, in December 16°·57. The highest reading of the *maximum* thermometer was on the 16th of June, and marked 99°·2; the lowest reading of the *minimum* thermometer was on the 27th of January, such was —28°·7 (below zero). The mean temperature of the quarterly periods was, Winter 19°·22, Spring 42°·46, Summer 68°·43, Autumn 44°·10. The yearly mean was 42°·89, and the mean yearly range 59°·27. The greatest intensity of the sun's rays was in August, and indicated 143°·6, the least intensity was in January, and was 64°·0, and the lowest point of terrestrial radiations was —22°·1 (below zero) in December.

The mean humidity (saturation being 1·000) was, in January ·909, in February ·906, in March ·881, in April ·858, in May ·895, in June ·739, in July ·727, in August ·741, in September ·834, in October ·855, in November ·798, in December ·759. The yearly mean was ·825.

Rain fell on 99 days, amounting to 44·201 inches, and was accompanied by thunder and lightning on 17 days. The greatest amount of rain which I observed, fell in September; it commenced at 5.10 P. M., on the 14th, and continued until 5.40 P. M. on the 15th, and amounted to 5·142 inches. I have only observed once, this year, a yellow matter fall with the rain, and that was on the 24th day of September. It was without thunder or lightning, but was accompanied by slight hail. *Snow* fell on 37 days, amounting to 116·81 inches on the surface. The first snow of the winter 1852–3 fell on the 17th day of October, 1852, and the last fell on the 14th day of April, 1853; the whole amount of snow in the winter 1852–3 amounted to 119·10 inches. The river Jesus was frozen over on the 28th day of November. The last steamer left Montreal (on the St. Lawrence) on the 7th of December; the first

steamer arrived at Montreal on the 15th day of April. The winter fairly set in on the 18th day of December.

The amount of evaporation was measured regularly from the 1st of April to the 31st of October, and amounted in April to 1·80 inches, in May 2·51 inches, in June 3·41 inches, in July 3·98 inches, in August 3·16 inches, in September 2·23 inches, in October 2·31 inches. This period includes what I consider could be taken with anything approaching to accuracy, owing to frosty weather.

The most prevalent wind during the year was the W. S. W., the least prevalent was the E.; in the winter quarter the most prevalent wind was N. E. by E., and the least S.; in the spring quarter the most prevalent wind was N. E., and the least so S.; in the summer quarter the most prevalent wind was W. S. W., and the least N.; in the autumn quarter the most prevalent wind was W. N. W., and the least E. The greatest velocity of the wind was on the 14th day of March, and was 32·60 miles per hour. The yearly mean of the maximum velocity was 15·81 miles per hour, the yearly mean of the minimum velocity was 0·32 miles per hour. The quarterly means were as follows: winter, maximum velocity 17·93, minimum velocity 0·25; spring, maximum velocity 16·68, minimum velocity 0·81; summer, maximum velocity 11·23, minimum velocity 0·29; autumn, maximum velocity 16·13, minimum velocity 0·18 miles per hour.

Crows were first seen on the 7th day of March, wild geese *Anser canadensis*, on the 30th day of March, swallows, *Hirudo rufa*, were first seen on the 1st of April; shad, *Alosa*, were first caught in this neighborhood on the 30th of May; fire-flies, *Lampyris corusca*, were seen on the 10th day of June: frogs, *Rana*, were first heard on the 23d of April.

The Aurora Borealis was visible on 39 nights as follows:

January 12th, 10 P. M. Faint auroral arch, dark segment underneath.—13th, 10 P. M. Idem, *Zodiacal light*, bright.

February 1*st*, 10 P. M. Faint auroral streamers.—8th, 4 A. M. Faint auroral light.—14th, 10 P. M. to daylight. Bright auroral arch.—20th, 10 P. M. Faint auroral arch. *Lunar halos* were visible on two nights during this month,—*Zodiacal light* was very bright also on 5 nights.

March 8th, 10 P. M. Faint auroral light to horizon, occasional streamers. *Zodiacal light* still visible and bright.

April 1st, 9 P. M. Low auroral arch, dark segment underneath; 10 P. M. streamers, segment vanished.—5th, 9 P. M. Zenith clear, N. W. horizon clouded with *strati*, Aurora Borealis faint; 9.30, auroral arch 40° high, dark segment underneath at the horizon.—6th, 8 P. M. Faint low arch; 9 P. M., arch 20° broad, dark segment underneath; 9.40, streamers in N. W. of a yellow green color; 10.30, streamers extending to the zenith.—10th, 9 P. M. Low faint auroral arch. *Zodiacal light* very bright on 5 nights during this month.

May 1st, 10 P. M. Faint auroral light.—2nd, 8.40 P. M. Splendid display of clouds of auroral light, forming a distinct arch stretching from the Eastern to the Western horizon, the apex of the arch passing the zenith, extending through the constellations *Bootes* and *Leo*; 9 P. M., auroral clouds in the N. W. low and very near the horizon, arch

very faint; 9.5, arch resumed the same brilliant appearance as at 8.40; 9.10, the whole of the Eastern and Western heavens were lighted up with a splendid display of auroral clouds, assuming various shapes and colors from yellow to crimson, arch disappeared; 9.30, all vanished.—4th, 9.10 P. M. Low auroral arch, dark segment underneath, occasional streamers.—30th, 10 P. M. Low faint auroral light to the horizon. *Lunar halo* visible on the 20th, diameter 63°.

June 14th, 9 to 10 P. M. Auroral streamers, moderate brightness, dark segment underneath.—30th, 10 P. M. Faint auroral light.

July 10th, 9 P. M. Auroral light, dark segment, occasional streamers; 10 P. M., dark segment and streamers vanished.—11th, 11 P. M. Faint auroral light to the horizon.—12th, 10 to 11 P. M. Streamers to the zenith, extending from N. N. W. to E.—18th, 1 to 2 A. M. Low dark arch of auroral light, moderate brightness, occasional streamers.—23d, 10 P. M. Auroral streamers of moderate brightness.—26th, 10 P. M. Faint auroral arch.—27th, 10 P. M. Auroral light to the horizon, splendid streamers. Shooting stars numerous during the month.

August 7th, 10 P. M. Faint auroral streamers, dark segment in the North.—25th, 10 P. M. Faint streamers of auroral light.—31st, 10 P. M. Faint auroral light. Shooting stars numerous from the 6th to the 13th. Comet first seen here on the evening of the 22d day, in the constellation *Leo*, at $8^h\ 20^m$ M. T., R. A. $11^h\ 30^m\ 10^s$, Declination N. 20°·5.

September 1st, 8.50 P. M. Splendid display of auroral clouds, forming four distinct arches, of about 3° in width, with dark segments between, stretching from E. to W. from a point centered as it were in *Arcturus*. The most southern arch passing at its zenith through *Aquila*, the next through *Lyra*, the next through *Polaris*, under which was a dark segment, from which were sent up frequent streamers. These appearances continued with slight intermissions in intensity of color, from 8.50 till 9.50 P. M. The southern or superior arch remained the longest time visible. The northern horizon was lighted up for some time, but faint (until 10.5). Stars of low magnitude were visible through these appearances.—2d, 8.50 to 11.40 P. M. Much the same appearance as last night, but the arches not so well defined. The most southern arch was several degrees south of the zenith. Many floating auroral clouds extending from E to W.—3d, 7.30 P. M. Auroral arches again seen this evening, only two in number, the most southerly a little N. of *Polaris*, very dark segments in the N. to the horizon, occasional streamers.—12th, 10 P. M. Faint auroral light.—18th, 10 P. M. Faint auroral light.—24th, 10 P. M. Faint auroral arch, dark segment underneath.

October 23rd, 10 P. M. Faint auroral light.

November 9th, 10 P. M. Floating auroral clouds—very high wind.—27, 10 P. M. Faint auroral light to the horizon. *Zodiacal light* very bright and well defined apex at *α Leonis*. (Regulus.) Base in East very extended.

December 4th, 8 P. M. Auroral light bright to the horizon.—20th, 10 P. M. Auroral arch; no dark segment.—28th, 10 P. M. Low auroral light to the horizon.

Electrical state of the atmosphere.—The atmosphere has *daily* afforded indications of electricity, varying in intensity, and kind: the

highest tension has been *generally* noticed in the winter season; the tri-daily observations (which could not be condensed) would occupy too much space for the columns of this Journal.

Ozonometer.—Observations have been carefully registered twice daily, for some years, of the amount of ozone present in the atmosphere; the slips of iodized paper are carefully preserved in a dark place, after having been exposed to the atmosphere, shaded from the sun, and rain. As a general rule, rain or snow shows an increase, and so far as my own observations go, a high electric state of the atmosphere *does not* show an increase in the amount of ozone.

St. Martin's, January 25, 1854.

2. *Tornado in Knox Co., Ohio, Jan.* 20, 1854, (Cleveland "Plaindealer.")—The tornado broke out not far from 3 P. M. and was preceded by a light rain which had fallen all day accompanied by wind and cold. For a short time before the tornado made its appearance the weather is said to have grown rapidly warm, and the wind to have lulled. The hurricane first manifested itself in the western part of Miller township, about eight miles west of south from Mount Vernon. It seemed to spring into full life and passion all at once. No serious traces of its work were seen until it blew down the stable and unroofed the house of John Robinson, residing in the west end of the township. The course of the storm at this time was east by northeast, which course it preserved to the close. It passed on, tearing up fences, unroofing and prostrating some twenty buildings. In its course it entered a piece of woods, half a mile long, and cut a swath through it of the uniform width of a quarter of a mile. The forest looks as if some giant had gone through it with a sickle. Almost every tree is blown down flat upon the ground. The few which remain standing are stripped of their branches, and their trunks are twisted till they look like whip-cords. The appearance of the prostrate woods is very curious. In the center of the storm-track the trees are laid in parallel lines. Toward the outer edge on each side, they gradually diverge from the parallel, till at last they lie quite at right angles to the line of the storm. Along the edges of the path the trees are not at all injured. The storm travelled about ten miles, occupying from two to five minutes, according to different estimates, in passing any one point. It seems to have worn the appearance of a mighty black pillar, reaching from earth to heaven, irradiated by blinding flashes of lightning, and accompanied in its devastating march by the music of a hundred cannon.

3. *Discovery in Photography*, (Edinb. New Philos. Journal, Jan., 1854).—A letter from Berlin of the 17th, says,—It is well known that the paper prepared for photography grows more or less black by rays of light falling on it. One of our young painters, M. Schall, has just taken advantage of this property in photographic paper to determine the intensity of the sun's light. After more than 1500 experiments, M. Schall has succeeded in establishing a scale of all the shades of black which the action of the solar system produces on the photographic paper: so that, by comparing the shade obtained at any given moment on a certan piece of paper with that indicated on the scale, the exact force of the sun's light may be ascertained. Baron A. von Humboldt, M. de Littrow, M. Dove and M. Poggendorff have congratulated

M. Schall on this invention, which will be of the highest utility not only for scientific labors, but also in many operations of domestic and rural economy.

4. *Fishes of Northern New York—Frozen Fish*, (Sci. Amer., January, 1854.)—Our lakes and streams, which, I believe, are the highest fishing waters in the State, and perhaps in the United States, were originally well stocked with the lake and brook or spotted trout. We have yet good fishing in all except lakes Sandford and Henderson, whose waters have been raised from their former level by the construction of dams, thereby destroying their spawning beds. Besides the trout, we have pickerel, perch, and a variety of smaller fish. The pickerel were introduced into Lake Sandford from Schroon Lake, five years ago. The stock originally came from Lake Champlain, though now our pickerel are quite different in appearance, and far superior, both in flesh and flavor, to the Lake Champlain pickerel: perhaps on account of the purity of water here. They have so multiplied in Lake Sandford, that upwards of three hundred have been caught through the ice this winter, weighing from two to fourteen pounds each.

But to the point. I have witnessed, repeatedly, the two winters I have been here, the resuscitation of frozen trout, pickerel and perch, on thawing them out in fresh running water, even after they had been carried for miles.

It is only under certain circumstances, however, that they will revive. If caught on a day when it is cloudy and freezing hard, and if not hurt with the hook, and they freeze immediately on being thrown on the ice, they will revive on being thawed out. But if allowed to toss about in the sun, on a clear day, and probably not freeze for an hour or two after they are caught, then they will never revive.

It is so common a thing, that I have only to go back to the last day I was fishing for an example of it. I went down to Lake Sandford with one of our men, on the 29th ult., and at night we carried home in our packs eleven pickerel, all frozen hard and bent and curved, just as they happened to twist themselves before freezing. We put them into a trough of running spring water, and when thawed out found six of them alive. The others had probably been caught in the warmest part of the day, and died before they froze. The same day fifteen fine brook trout were brought from Lake Andrew, five miles distant, in a pack, and on being thawed out several of them revived, though I did not notice how many. They are, however, a much more delicate fish than the pickerel or perch, and more easily hurt and killed than either of them.

On the afternoon of the 24th ult. I had fished faithfully for pickerel till sundown, without even getting an encouraging nibble; tired at last of that fun, I took out a small hook and line, and soon had twenty-five perch; they froze almost instantly; I strung them on a crotched twig, carried them so for two miles, and when thawed out, found fourteen of them alive, the rest having been hurt either by the hook or the twig.

The pond behind the village, formed by the damming of the river, is full of young pickerel; they are all from three fish put in there last winter—one male and two females; every one of them were brought from Lake Sandford frozen, and were put into the pond after they had

been thawed out in a trough. The male one I caught, it lay on the ice, frozen, for three hours, and then not finding a mate for him, I run a stick through his gills and dragged him home on the snow, two miles, threw him into the trough, and thought no more of him till next morning, when I found him alive and seemingly enjoying himself as well as his narrow limits would permit. I took pity on the poor fellow, carried him down to the pond, and he went off with a dart.

These are but a few instances of what occurs here almost every day the winter through. The fact of their resuscitation, after being frozen as I have described, is known to every one here who is in the habit of fishing in winter, and cannot escape being noticed, as the weather here is cold enough almost all the time to freeze them, and they have to be thawed out before they can be cleaned.

I have heard some say that they have taken trout when frozen, and whittled the fins and tail off, and on being thawed, found them alive; but I have never tried this nor any other experiment with them, and would not vouch for the truth of it. ROBERT CLARKE.

Adirondac Iron Works, Essex Co., N. Y.

5. *American Association for the Advancement of Science.*—The next meeting of the American Association will be held in Washington City, D. C., commencing with the 30th of April.

6. *Cabinet of Minerals for Sale.*—A cabinet of minerals, containing about one thousand specimens, is offered for sale in Philadelphia. It includes both Foreign and American specimens, well selected, and would make a good collection for any Academy or College where there is instruction in Mineralogy. It will be sold low. Address Wm. S. Vaux, Esq., No. 145 Mulberry st., Philadelphia.

7. *Cameroceras trentonense and Orthis Verneuili.*—Mr. Marcou has the authority of de Verneuil for saying that *Cameroceras trentonense* is the same as *Orthoceras communis* of Europe (see page 201 of this volume). But if the figure given by Hisinger is to be relied upon, it is widely different. Wahlenberg's original description reads: "Continet siphonem modicæ crassitiei æquantem decimam circiter partem diametri transversalis totius testæ, eumque ut plurimum inter axin et ambitum tubi in medio situm." But in the C. trentonense, the siphon is marginal and full one-half the shortest diameter of the tube, and as nearly as possible to four-ninths of the longest diameter, the form of the shell being oval in all specimens seen.

De Verneuil cites *Orthis Verneuili* from the Ottowa River, Canada, in Logan's collection, and if this be correct, it is an American species. Mr. Marcou has therefore the authority of de Verneuil for pronouncing both of these species as American; yet there is reason to believe that the facts are otherwise.

8. OBITUARY.—J. E. TESCHEMACHER, (Communicated by the Boston Soc. of Nat. Hist.)—At a meeting of the Boston Society of Natural History held Dec. 21st, the President, Dr. J. C. Warren, spoke of the recent death of one of its members, Mr. J. E. Teschemacher, as follows:

Gentlemen:—Our Society has experienced a great loss in the death of Mr. Teschemacher, one of its most valuable members; and we must

turn aside a moment from the path of science to pay a tribute to his memory. This gentleman, who joined our Society in the year 1835, and has since that time been an able associate in our labors, and a large contributor to the advancement of science in our country, has suddenly terminated his mortal career at the age of sixty-three, from a disease of the heart.

Mr. Teschemacher was a botanist, a mineralogist and geologist, and the records of our Society will show that he studied these sciences not in a loose and general way, but with untiring minuteness and assiduity. Although born on a foreign soil, he became so entirely affiliated with his adopted countrymen, that no one ever considered him in any other light than that of a fellow-citizen, and all regarded him with affection as well as respect.

Our Society, moved by the great merit of Mr. Teschemacher, and the loss they have experienced from his death, have therefore—

Resolved; First, that a record be made in their transactions of the high estimation in which they hold the private qualities and scientific labors of Mr. Teschemacher, as manifested in his excellent papers on botany, mineralogy, some departments of geology, and particularly in his able and practical investigations of the carboniferous formations. We also regard his productions on the composition and improvement of soils as a valuable and permanent contribution to the agriculture of the country.

Resolved; Second, that the President of the Society be requested to prepare some notice of the life and labors of our learned associate.

Resolved ; Third, that a copy of these Resolutions, with the preamble and appended notice of his productions, be presented to his family, to the scientific journals of the country, and the daily paper which publishes the proceedings of this Society.

In compliance with the second of the above resolutions the President presented the following notice.

James Englebert Teschemacher, (of Hanoverian extraction on the paternal side,) was born in Nottingham, England, on the eleventh day of June, 1790. At the age of fourteen he commenced his commercial career in a mercantile foreign house of eminence in London, where he evinced application and business talents of a high order; and amid the extensive transactions of mercantile life, in which during a long series of years he was engaged, his fine, comprehensive mind, ever remained unshackled by any of the less elevating habits sometimes contracted in commerce. At an early period of his life he imbibed a taste for studying out of nature's beautiful book; thus acquiring that purity and love of truth so constantly pervading all his thoughts and writings.

In the year 1830 Mr. Teschemacher accepted the offer of a partnership in a house of considerable standing in Havana, and proceeded to Cuba with highly advantageous prospects; but these prospects faded on his approach, and he returned to England. After a brief space he made up his mind to repair to the United States with his family; reaching New York, February 7th, 1832, he finally settled in Boston, where during the space of twenty-one years, he was unremitting in his exertions for his family. Of his untiring zeal and enthusiastic devotion to science we need not speak: his hours of leisure, it may be natu-

rally inferred were few, but those few were employed (apparently as a recreation) in the severer branches of study, which frequently form the labor of a life, even with those who make science their occupation. Truly may he be said to have improved the talents committed to his charge.

The following is a list of his Academic papers and published works:

1885. June 3rd, A report on a specimen of Sulphur.
" July 5th, On a collection of Minerals from Russia.
1837. October 5th, On three species of Trillium.
1838. January 3rd, On a specimen of Moonstone.
" " 31st, On alloys with Nickel.
" February 20th, On Palms.
" March, An account of the Camelliæ.
" November, On the Gomphocarpus fruticosus.
1839. February, On the Lodovicea Sechellarum, or Noble Palm.
" June, On the Minerals of Milk Row Syenite Quarry, Charlestown.
1840. April 22nd, On Minerals from New York.
" July 15th, On plants from New Zealand.
1841. February, On plants from Illyria.
" April, On Phosphate of Uranium at Chesterfield.
1841. June, On a new species of Rafflesia from Manilla.
" August, On plants from Kentucky.
1842. March, On fossil Ferns from Mansfield.
" October, On corn from Texas.
" —— On soil from Huron Prairie, Ohio.
" —— On the identity of Pyrochlore with the Microlite of Prof. Shepard.
1843. January, On a slab with dendritic markings, from Newton, Mass.
" February, Remarks on Guano.
1844. —— On Beryls from Acworth.
1845. June, On Melo-cactus, from St. Diego, California.
" November, On Russian minerals.
" December, On Ferns from the Sandwich Islands.
1846. March, Mineralogical notices of Damourite and Pyrrhite.
" —— On fossil vegetation.
1848. January, On fossil vegetation.
" April, On angles of the mineral Arkansite.
1850. February, On the mineral Vermiculite.

Published Papers.

An address before the Boston Society of Natural History.
" " " " Horticultural Society.
" " " " Harvard Natural History Society.
An essay on Guano.

In the Boston Journal of Natural History, vol. ii, 1837:—

Notice of three species of Trillium, found in the vicinity of Boston.

Ibid, vol. iv, 1841.

On a new species of Rafflesia from Manilla.

On the occurrence of the Phosphate of Uranium in the Tourmaline locality at Chesterfield.

Mineralogical Notices—Ibid, vol. v, 1845.

On the occurrence of Uranium in the Beryl limits of Ackworth, N. H.

On Melo-cactus viridescens.

On the fossil vegetation of America.

9. *Outline of the Geology of the Globe, and of the United States in particular, with two Geological Maps and Sketches of Characteristic American Fossils;* by EDWARD HITCHCOCK, D.D., LL.D., Pres. Amherst College, and Prof. Nat. Theol. and Geology. 136 pp., 12mo. Boston, 1853. Phillips, Samson and Company.—This little volume is published as a sequel to the author's "Elementary Geology." It is necessarily brief in its allusions to the geology of different countries of the globe, but all are passed in review, and the subject is illustrated by a colored chart of the world, taken mostly from the geological chart of M. Boué. The latter half of the volume treats of North American Geology. A large geological chart of the United States closes the volume.

10. *The Microscope in its special application to Vegetable Anatomy and Physiology;* by Dr. HERMANN SCHACHT. Translated by FREDERICK CURREY, Esq., M.A. 132 pp. 12mo, with numerous illustrations. London, 1853. Samuel Highley, 32 Fleet street.—The figures in this volume which illustrate microscopic apparatus are confined to a single view of "Highley's Hospital Microscope." The work, after 30 pages on microscopes and the methods of using them, proceeds to the subject of cells, vegetable tissues, and the examination of the different parts of plants, in the course of which there are occasional wood-cuts. Two closing chapters treat of the drawing of objects in Natural Philosophy, especially microscopical objects, and on the preservation of objects for the microscope. The volume is a good companion for the microscope and opens to the student or amateur, subjects and methods of investigation that cannot fail of giving both profit and delight.

11. *Explanations and Sailing Directions, to accompany the Wind and Current Charts;* by M. F. MAURY, LL.D., Lieut. U. S. N., Superintendent of the National Observatory. 6th edition, enlarged and improved. 772 pp. 4to, with 16 plates. Philadelphia, 1854. E. C. & J. Biddle. Published by the authority of Hon. J. C. Dobbin, Secretary of the Navy.—A copy of the new edition of Lieut. Maury's Sailing Directions, reached us just as these pages were going to press, and we have but time to mention its contents. The sixth edition, owing to the demand for the work, has followed rapidly on those preceding. The volume has a miscellaneous character; but bears in all its topics on the great subjects of physical geography and navigation. The topics discussed are the following. The Wind and Current Charts, including general observations on the atmosphere and sea. Universal System of Meteorological Observations, and Report of the Maritime Conference at Brussels. Explanatory Notes for keeping Abstract Log. Influence

of the Gulf Stream on the Trade of Charleston. Currents of the Sea. General Circulation of the Atmosphere, and its probable relation to the Magnetism of the Earth. Red Fogs and Sea Dust. Clouds and the Equatorial Cloud-Ring. Red Sea Currents. Geological agency of the Winds. Saltness of the Sea. Open Sea in the Arctic Ocean. Physical Geography of the Sea. Gales in the Gulf Stream. Wind and Current Charts; Pilot, Thermal and Trade Wind Charts. Routes to and from Europe. Routes to Rio. Passage round Cape Horn. Route to California, and between California and Australia; Routes from Europe and the United States to Australia; and from the Sandwich Islands, home. The general system of Meteorological Observations proposed, if carried out, must give rapid progress to our knowledge of physical geography. With so large an object as the earth for our study, and especially its atmosphere and oceans, whose motions are world-wide in their system, the examinations at a single place, or even many places, are alone of little account. It requires the combined action of civilized nations, with accurate observers spread over all lands and seas, to procure the data for even one great generalization in this department.

12. *The Annals of Science, being a Record of Inventions and Improvements in Applied Science, including the Transactions of the Cleveland Academy of Natural Sciences;* conducted by HAMILTON L. SMITH, A.M. January. 32 pp. 8vo. February. Cleveland, Ohio.—The Annals of Science appeared in January of enlarged size and as a monthly instead of semi-monthly: it moreover embodies the Transactions of the Academy of Natural Sciences of Cleveland. Its pages contain contributions by J. S. Newberry on coal plants of Ohio, who is one of the most able writers on the subject in the country, and is bringing to light many new species; by Dr. J. P. Kirtland, on fishes, also one of our best naturalists; by the editor on microscopes and microscopy; and other papers of importance. The Annals is issued at the low price of one dollar a year. It contains much that bears on practical science, and records of discoveries which are of general interest, and will be a valuable acquisition to any who may become subscribers to it.

13. *Transactions of the American Philosophical Society, Philadelphia*, vol. x, new series, Part III.

Contents—Art. XXII. Description of an extinct species of American Lion; *Felis atrox;* by JOSEPH LEIDY, M.D.

XXIII. A Memoir on the extinct Dicotylinæ of America; by JOSEPH LEIDY, M.D.

XXIV. Chemical Examination of two Minerals from the neighborhood of Reading, Pennsylvania; and on the óccurrence of Gold in Pennsylvania; by CHARLES M. WETHERILL, Ph.D.

XXV. On a New Variety of Asphalt; *(Melan-asphalt,)* by CHARLES M. WETHERILL, Ph.D.

XXVI. On the Decomposition of the Alkaline Sulphates by Hydrochloric Acid and Chlorine; by R. A. TILGHMAN.

XXVII. Notes on the Classification of the Carabidæ of the United States; by JOHN L. LE CONTE, M.D.

XXVIII. Revision of the Elateridæ of the United States; by JOHN L. LE CONTE, M.D.

APPENDIX.

Notice of a collection of Fishes from the southern bend of the Tennessee River, in the State of Alabama; by L. Agassiz.

The only information we have at present upon the fishes of the Tennessee River, has been published by Dr. D. H. Storer, who mentions nine species from the vicinity of Florence, Alabama, in the Proceedings of the Boston Society of Natural History for 1845, and of which short descriptions appeared in his Synopsis of the Fishes of North America, in 1846. Having lately received a collection of not less than thirty-three species from the same water system, brought together by the untiring efforts of Dr. Newman, of Huntsville, who has most kindly placed them in my hands for description, it seems desirable that an early notice of the general character of the ichthyological fauna of that region should be published, to serve as a standard of comparison with the fishes of the other western and southern rivers, in the study of their geographical distribution. I arrange them below according to their natural affinities.

PERCOIDS, *Cuv.*—Whether the genera Perca, Labrax, and Lucioperca, are really wanting in the Tennessee River remains to be ascertained. No specimens of these genera were found among those forwarded by Dr. Newman; though many less conspicuous forms were collected. Thus far the genera Grystes, Centrarchus, and Pomotis, as understood at present by ichthyologists, are the only representatives of the family of Percoids in the Tennessee River.

1. Grystes, *Cuv.*—I have already shown in my "Lake Superior" that the genera *Grystes* and *Huro* of Cuvier do not differ essentially one from the other, and must therefore be united into one natural group; moreover when the fishes of Kentucky shall be better known, it may become necessary to substitute for either of them the name of *Lepomis*, introduced in ichthyology by Rafinesque, as early as the year 1820, for the western species of this genus. If I hesitate to make the change now, it is simply because I have not the means of deciding upon the value of his many species. The species of this group are indeed very difficult to characterize. They differ chiefly in the relative size of their scales, the presence or absence of teeth upon the tongue, though Cuvier denies the presence of teeth on the tongue of any of

them, &c. There are besides marked differences between the young and the adults. These circumstances render it impossible to characterize any one species without comparative descriptions and figures. The species from Huntsville, known there under the name of Trout, differs equally from the northern species mentioned in my "Lake Superior," and from that of the Southern States described by Cuvier and Valenciennes as Grystes salmoneus. Its snout is shorter, the posterior end of the upper maxillary extends beyond the hinder border of the eye, the head is higher, and the scales much larger in the dorsal as well as in the ventral regions. No teeth on the tongue. I call this species provisionally *Grystes nobilis*, Ag. It reaches a large size, and weighs occasionally from ten to fourteen pounds.

2. Centrarchus, *Cuv.*—Under this name Cuvier has combined a variety of Percoids agreeing in general form; their body being oval and compressed, and the two dorsals continuous; but these fishes differ from one another in so many respects that they require to be further subdivided.* I shall retain the name of *Centrarchus* for that group of species of which *Centrarchus irideus* may be considered as the type. Thus circumscribed, the genus Centrarchus may be characterized as follows: Body very broad, greatly compressed, above as well as below. Dorsal long and high, gradually rising, without a depression between the spinous and soft rays; spinous portion of the fin largest. Anal shaped like the dorsal, but with fewer spinous rays, extending between the ventrals. Mouth small. No species of this genus has been found in the Tennessee River.

3. Pomoxis, *Rafin.*—This genus was established by Rafinesque for a species closely allied to the *Centrarchus hexacanthus* of Cuv. and Val., and it well deserves to be retained. The body is much elevated and compressed, resembling somewhat Centrarchus proper. Like that genus it has a high dorsal and a high anal, of nearly equal size, and the spinous portion of these fins rises towards the soft rays without a depression; but in Pomoxis the soft portion of these fins is much the largest, whilst it is the smaller in Centrarchus; in Pomoxis the lower jaw is very prominent. The mouth is very large, which is smaller in Centrarchus. I have found representatives of this genus in all the Western States, from the western parts of New York to the Gulf of Mexico, and in the southern Atlantic States, but none in the northern Atlantic States. The species from the Tennessee River, called there Speckled or White Perch, agrees fully with the description given by Rafinesque of his *Pomoxis annularis*, with the sole exception of a *golden ring at the base of the tail*, which may be

* DeKay has contrived to render the genus Centrarchus of Cuvier still less natural, by introducing into it his *Centrarchus fasciatus* and *obscurus*, which truly belong to the genus Grystes. See "Lake Superior," page 295.

faded in the specimens sent by Dr. Newman, from Huntsville. Not having however specimens from the locality quoted by Rafinesque, I must leave it for further investigations to determine beyond any doubt their specific identity or difference. *Centrarchus hexacanthus*, Cuv. and Val., belongs unquestionably to this genus.

4. Ambloplites, *Rafin.*—This is another of the natural genera established by Rafinesque for one of the many distinct types combined by Cuvier and Valenciennes under the name of *Centrarchus*. The well known *Centrarchus æneus* may be considered as its type, though Rafinesque founded his genus upon another species, from Kentucky, which has remained unnoticed since. The genus Ambloplites is easily distinguished from the preceding ones by the structure of its dorsal and anal fins. The spinous portion of the dorsal is much longer than the posterior soft portion of that fin and scarcely half its height, causing a marked depression to appear between the spinous and the articulated rays. The same is the case with the anal, which is also long; but low in its anterior spinous portion. The general form of these fishes is oval, and the body less compressed than in the preceding genera. The species from the Tennessee River agrees in every respect with Rafinesque's *Ambloplites ichtheloides*. It is called at Huntsville Goggle-eyed or Black Perch. In adopting the genus Ambloplites and referring this species to it with Rafinesque's authority I have acted with that discretion due to an author who labored under the greatest difficulties when preparing his work upon the fishes of the Ohio. It is true he himself describes this species as *Lepomis ichtheloides;* but he also suggests the desirableness of distinguishing it generically and proposes a new name for the genus, should it be admissible. Finding it to be so, I do not hesitate in giving him the fullest credit for his suggestion, even though I must add that he has described another variety of the same species under the name of *Ichthelis erythrops*. I have found both these varieties among the fishes sent to me by Dr. Newman, and I have no hesitation in considering them as specifically identical with one another and as agreeing fully with Rafinesque's descriptions. Should naturalists be more generally inclined to correct simply what they consider as errors in their predecessors instead of discarding altogether what they can not at once determine, we should have much fewer of those nominal species in our descriptive works, which are the curse of our scientific nomenclature. *Ambloplites ichtheloides* is much stouter and more elongated than *Ambl. æneus;* body less compressed above; face broader, lower jaw less prominent, and strongly arched from side to side; mouth opens less obliquely upwards; spinous rays of dorsal and anal shorter than in A. æneus; dorsal sprinkled with white spots.

5. Calliurus, *Rafin.*—Among the many Percoids found in the freshwaters of the United States there is one very common in South Carolina, which was first described by Cuvier and Valenciennes under the name of *Pomotis gulosus*, and afterwards referred by them to the genus *Centrarchus.* This species however belongs neither to Centrarchus nor to Pomotis, if we are to consider genera as expressing the same general features under a variety of modifications; for all true Pomotis are fishes with a small mouth, feeding on worms, while P. gulosus has a large mouth like Grystes and is a voracious animal living upon small fishes, which he chases with great energy. Again, Centrarchus has fins widely different in their structure from those of P. gulosus; there being a large number of spinous rays in advance of the anal in Centrarchus proper and those genera mentioned above which have been finally separated from Centrarchus; whilst P. gulosus has only three, like the true Pomotis. Notwithstanding these peculiarities I have been hesitating for a long time to consider P. gulosus as the type of a distinct genus, until I ascertained that there exist many species of this type in different parts of the country, all of which reproduce the essential peculiarities of P. gulosus under a variety of modifications. Upon a careful investigation of all the works in which American fishes are mentioned, I ascertained however that Rafinesque had already established a distinct genus for a species of this type described in his *Ichthyologia Ohiensis* under the name of *Calliurus punctulatus.* It is hardly surprising that this genus should have been overlooked by European ichthyologists and that it should even have escaped the notice of the authors of the great French *Histoire naturelle des Poissons*, for the fishes of the Ohio river have remained entirely unnoticed since Rafinesque, until Dr. Kirtland published his interesting and highly valuable papers upon the fishes of Ohio, in the Journal of the Natural History Society of Boston. Dr. Kirtland however, though the first author who has done full justice to the valuable contributions of Rafinesque to the Ichthyology of the United States, does not mention the species described by Rafinesque, as *Calliurus punctulatus*, and so this genus has remained unnoticed until now. It has occurred to me that it would be but justice to a naturalist, whose labors have been so generally neglected, to call the attention of Ichthyologists to these facts. I subjoin a short diagnosis of the genus *Calliurus:* Body oval, rather elongated, not compressed above. Dorsal long and low in its anterior portion, with a slight depression between the spinous and soft rays; posterior portion of the dorsal shorter than the anterior, though higher. Anal not half the size of the dorsal, with only three spinous rays. Mouth large, opening somewhat upwards, the lower jaw being longer than the upper. The species from Huntsville is identical with Rafinesque's *Calliurus punctulatus.* It is called there Black Perch or Goggle-eye.

6. Pomotis, *Rafin.*—Every ichthyologist must be familiar with the freshwater sunfishes, so common throughout, the United States; but it is perhaps not so generally known that the authority to which the genus Pomotis ought to be ascribed is questionable. Indeed, I find it universally ascribed to Cuvier: but that name occurs already in Rafinesque's Ichthyologia Ohiensis, published in 1820, as a subgenus of his genus *Ichthelis*, which he there divides into *Telipomis* and *Pomotis.* It seems therefore probable to me that Cuvier not considering these subdivisions necessary, and finding the name *Pomotis* better adapted to express the prominent character of all the species of this group, adopted the name of Pomotis in preference to *Ichthelis*, and in conformity with an objectionable practice, followed by some naturalists, to which Cuvier however did not adhere in other instances *of applying a new authority whenever the range of a genus is modified*, allowed in this case his name to supersede that of Rafinesque, which I would however restore, in conformity with the more just practice now prevailing. If it were further asked, what should be done with the name of Ichthelis which was proposed by Rafinesque as early as 1818. Whether it should be made a synonym of his own subgenus Pomotis? or disregarded altogether, because Pomotis has come into general use? I would suggest that neither would be the proper course to follow. It is my opinion that in a complete monograph of this group, the name *Ichthelis* should be finally restored to its right and *Telipomis* and *Pomotis* used for such sections or genera as it may become necessary to separate from it, now that the number and diversity of species of this group has increased beyond expectation. This is at least the course I shall adopt when publishing the descriptions of the many new species of this type I have collected in the Southern States. For the present, I limit myself to describing the seven species sent to me by Dr. Newman, six of which are new to science.

1. *Pomotis sanguinolentus*, Agass.—Called Sun Perch at Huntsville. The general outline of the body is that of Pomotis nitida, *Kirtl.*, but the back is more compressed, the dorsal and anal fins are more pointed behind, and the spinous rays are longer, the base of the anal is shorter. The sides of the head are marked with irregular undulating longitudinal lines of a metallic steel blue color, extending from the cheeks across the gill cover to the base of the pectorals and even continuing alone the sides of the body in dotted lines. There are generally four of these lines below the eyes, the first being close to its margin, and extending backwards along and around the border of the opercular appendage and returning, meets the centre of the hinder margin of the eye, but reappears immediately in front of the eye and continues to the edge of the upper jaw. Though the opercular appendage is

rather large, the lateral line is so high near the back, that it is not covered by it anteriorly. The general color is of a reddish brown, mottled with red above and passing gradually into a uniform bright brick-red color prevailing upon the lower part of the body, and sprinkled with irregular light dots.

2. *Pomotis inscriptus*, Agass.—Small species, the outline of which is more elongated than in P. sanguinolentus. The gill covers are marked as in that species with three or four lines of a metallic steel blue color; opercular appendage long, directed more obliquely upwards than in any other species here described, black, with a light border which is a continuation of two of the lines of the cheeks, the one running below the eye, the other terminating behind the eye. Each scale of the back and sides is marked in its centre with a short narrow black line, hence the sides are regularly striped with dark interrupted lines as numerous as the rows of scales. Spinous rays all comparatively long and slender; the passage from the anterior to the posterior part of the dorsal gradual. All the fins except the pectorals are tinged with black at the extremity. General color dark olive above, lighter beneath.

3. *Pomotis notatus*, Agass.—Called Pond Perch at Huntsville. Body more elongated than in P. vulgaris; its upper and lower curve nearly equal. Opercular appendage very short, not extending beyond the base of the pectorals; its hinder margin is orange-colored, with a black spot in front, from which a faint dusky band extends to the eye. The spinous rays of the dorsal and anal are more slender than in P. vulgaris, and the articulated rays are crossed by fewer dotted or broken dark lines. The pectoral fins are long, extending beyond the base of the anal, as in *Ichth. macrochirus*, Raf. The color is of a uniform light olive; the sides, gill cover and belly being silvery; scales not dotted with black as in many similar species.

4. *Pomotis incisor*, Val.—Also called Pond Perch at Huntsville. This species resembles very closely the preceding, and is considered the same by the fishermen; but its profile is more arched and slants more abruptly; the black opercular appendage is not encircled with a brighter margin. Sides of the head not banded, but of a uniform color throughout. Dorsal and anal not banded, but darker colored than in the preceding species. There is moreover a dark black spot near the base of the hind rays of the dorsal in P. incisor which is wanting in P. notatus. General color of the body the same in the two species.

5. *Pomotis obscurus*, Agass.—Also called Pond Perch at Huntsville. Resembles P. incisor in the outline of the body, except that the profile is still more precipitate and the body somewhat more elongated as well as much stouter, especially in the region

of the head and across the pectorals. The opercular appendage is longer and broader, but also without a light posterior margin. The posterior soft rays of the dorsal are marked with a black spot as in the preceding species, but all the spinous rays of that fin are shorter and stouter. It is a dark colored fish throughout the lower as well as the upper side of the body, almost uniformly brown, the belly only being somewhat lighter in hue. The face and lower jaw are of a leaden color. The fins are all darker than in P. incisor, especially the ventrals.

6. *Pomotis bombifrons*, Agass.—Body higher than in P. obscurus and profile even more arched. Forehead prominent especially over the eyes. Head quite broad and short. Opercular appendage black, and small; a light narrow band runs along its lower margin. No black spot upon the hind part of the dorsal. The last spinous rays of this fin are shorter than in P. obscurus, thus making the passage to the soft rays more abrupt and marked, the soft portion of the fin being almost as prominent as in Ambloplites and Calliurus when compared with the spinous rays. Body light brown, fins lighter colored; scales of the belly and sides dotted with golden orange. The face and under jaw have not the leaden color of P. obscurus. Considering the peculiar form of the vertical fins and of the forehead, it may become necessary to separate this species from the other Pomotis. Indeed, I know already several species which agree in these respects with one another and must at all events form a distinct group in the genus.

7. *Pomotis pallidus*, Agass.—This species resembles P. incisor in the outline of the body, the nature and coloration of the scales, and in the size and form of the fins, but it differs greatly from it by its large mouth, the free extremity of the upper jaws reaching the vertical line of the middle of the eye, by the presence of teeth upon the palate, and by the ventral fins being placed immediately under the pectorals. The black opercular appendage which is very short, has a narrow orange border behind. There is a black spot at the base of the posterior rays of the dorsal. Both dorsal and anal are marked by one or two dark stripes; the caudal is crossed by several dotted vertical lines. There are eight or nine dusky bars across the sides, between the head and tail. This species bears the same relation to Pomotis, that Pomoxis bears to the true Centrarchus, in the size of the mouth, and the form of the body, and I have no doubt it will some day become the type of a distinct genus.

ETHEOSTOMOIDS, *Agass.*—There are comparatively few natural families in the animal kingdom so limited in their geographical distribution as to be entirely circumscribed within the boundaries of a single continent, and these few belong mostly to the type of Vertebrata. Though among fishes we should least ex-

pect such local groups, considering the greater uniformity of the conditions of existence prevailing in the medium they inhabit, when compared with the main land, yet there are several families of this class, the geographical range of which is quite limited. I need only mention the Goniodonts of South America, the Labyrinthici of the Indian Ocean and the Sunda Islands, the Lepidostei of North America, &c. Another natural family thus located within narrow limits is that of *Etheostomoids*, which I have for the first time characterized in my work "Lake Superior," p. 298. This family is founded upon the genus *Etheostoma* of Rafinesque, to which are added the genera *Pileoma* and *Boleosoma* of Dekay (of which the genus *Percina* of Haldeman is a synonym) and my genus *Pœcilichthys.** The three first of these genera were referred by their authors to the family of Percoids; but the absence of an air-bladder and of pseudobranchiæ, and the incomplete suborbital arch precludes such an association. Indeed these fishes are more closely allied to the true Cottoids and in particular to the genus Gasterosteus than to the Percoids, though the want of connection between the single suboperculum and the preoperculum forbids also a more intimate alliance with that family. The form of the ventrals of the Etheostomoids reminds us somewhat of those Gobioids in which the two ventrals are distinct. Since the publication of the work above mentioned, I have become acquainted with three new genera of this family, for which I would propose the names of *Hyostoma*, *Catonotus*, and *Hadropterus*.

The more extensive knowledge I have acquired of this family by these recent accessions enables me to give more precision to the characters assigned at first to its genera; as follows:

1. Etheostoma, *Rafin.*—Head elongated pointed; mouth terminal, widely open, not protractile, broad; jaws of equal length. Opercular apparatus and cheeks bare. First dorsal distinctly separated from the second. Anal and second dorsal smaller than the

* The genus *Pœcilichthys* was first mentioned under the name of *Pœcilosoma*. Being however at the time of its publication far away from Cambridge, and unable to consult my library or any other, I did not perceive that that name was already preoccupied; I would therefore change it now to Pœcilichthys. Several new species of this genus have been discovered since. One described by Dr. Kirtland as *P. erythrogaster* from the vicinity of Cleveland, Ohio. Ann. of Sci., Jan., 1854, p. 4. Another collected by Mr. Geo. Stolley in the Osage River, Mo., remarkable for its brilliant colors, the body being light brown, with dark black lines upon the sides of the back and with broader transverse bands alternately black and orange red, especially bright upon the sides of the tail; dorsals banded with black, white and red. I call this species *P. spectabilis*. Another found by Dr. L. Watson in small creeks near Quincy, Illinois, similar in color to the preceding but without black stripes along the back, also less compressed. I call this species *P. versicolor*. Specimens of this species were also received from Osage River. A fourth species from the Osage River, Mo., also discovered by Mr. Geo. Stolley, is of a greenish color mottled with black, the second dorsal, the caudal, the anal, the ventrals and the pectorals being dotted all over with minute dark specks. I call this species *P. punctulatus*.

first dorsal, but equal to one another. Caudal lunate. Type of the genus: *Ethblennioides*, Raf.

2. Catonotus, *Agass.*—Head elongated, obtuse; mouth terminal, widely open, not protractile, lower jaw longer than the upper. Opercular apparatus, cheeks and neck destitue of scales. First dorsal much lower than the second, with clubshaped rays when full grown; membrane of this fin extending to the base of the second dorsal. Anal smaller than the second dorsal. Caudal rounded. Only one species known: *C. lineolatus*, Agass., discovered by Dr. L. Watson in small creeks near Quincy, Ill. The whole body olive green with close narrow interrupted black longitudinal lines; transverse lines of the same color across the caudal.

3. Pileoma, *Dekay.*—Head conical, pointed, truncated at the end, in form of a hog's snout; mouth moderate, in form of an oblique arc of a circle, opening below the end of the snout, very slightly protractile. Lower jaw shorter than the upper. Operculum and cheeks scaly. Membrane of the first dorsal not reaching the base of the second. Anal smaller than the second dorsal. Caudal truncate or slightly lunate. Type of the genus: *P. caprodes.* (*Etheostoma caprodes*, Rafin.)

4. Hadropterus, *Agass.*—Head conical, obtusely pointed, rounded at the end; mouth moderate, terminal, not protractile, jaws nearly equal. Operculum and cheeks scaly. Membrane of the first dorsal extending to the base of the second. Anal and second dorsal large and equal. Caudal truncate or slightly lunate. Only one species known: *H. nigrofasciatus*, Agass. From the neighborhood of Mobile, Alabama. Discovered by Albert Stein, Esq. Brown above, lighter below, with transverse black bands, wider in the middle than nearer to the back or the belly.

5. Hyostoma, *Agass.*—Head short, blunt, rounded, with swollen cheeks. Mouth comparatively small below the snout, slightly protractile. Lower jaw shorter than the upper, which may be concealed in a deep furrow below the snout. Opercular apparatus and cheeks scaly. First dorsal long, but not reaching the base of the second. Anal smaller than the second dorsal. Caudal slightly lunate. Only one species known: *H. Newmanii*, Agass. Discovered by Dr. Newman in the vicinity of Huntsville, Alabama, where it is called "*Salmon.*" This fish is uniformly brown with irregular transverse black blotches. A red stripe along the base of the first dorsal.

6. Pœcilichthys, *Agass.*—Head short and strong, tapering into a rounded snout. Mouth terminal, proportionally broad, not protractile, though the maxillary bone be moveable. Opercular apparatus scaly, cheeks bare. First dorsal distinctly separated from the second. Anal smaller than the second dorsal. Caudal truncate or slightly rounded. The species of this genus are among

the most brilliant freshwater fishes in the world. Type of the genus: *Etheostoma variatum*, Kirtl. Several new species are mentioned in the note above.

7. Boleosoma, *Dekay*.—Head short, rounded; mouth below the end of the snout, small, horizontal, slightly protractile. Opercular apparatus and cheeks scaly; neck scaleless. Membrane of the first dorsal reaching the base of the second, though the two fins are distinctly separated. Second dorsal much larger than the anal. Caudal rounded. Type of the genus: *Boleosoma tessellatum*, Dekay. For references to other species, see "Lake Superior," page 299.

All the representatives of this family are confined, as far as we know, to the fresh waters of North America; not a single species having thus far been noticed either in Europe or Asia. To this circumstance we must no doubt ascribe the total neglect of the genus Etheostoma of Rafinesque by European ichthyologists.

The genus Hyostoma is the only type of this family I am acquainted with from the southern bend of the Tennessee River. It is true, Dr. Storer has described two species of Etheostoma from the vicinity of Florence, Alabama, but they do not seem to occur farther east; at least I have found nothing to remind me of his species in the collection forwarded by Dr. Newman.

It is a fact worthy of notice that not a single species of Gasterosteus has as yet been discovered in the Mississippi River or its tributaries, or in any of the rivers emptying into the Gulf of Mexico. I have also searched in vain for them in the southern Atlantic states, though they are common in the northern states and in the waters emptying into the St. Lawrence.

SCIÆNOIDS, *Cuv.*—In the old world no representative of this family is known to inhabit the freshwaters, whilst in North America a remarkable species has been found in Lake Champlain, Lake Erie, Lake Ontario and the Ohio River, which truly belongs to this family and has generally been referred to the genus Corvina, under the name of *Corvina Oscula*. It should however be remarked that this species is but remotely allied to the genus Corvina and must in reality be considered as the type of a distinct genus, which has already been characterized, thirty-four years ago by that indefatigable naturalist, Rafinesque, under the name of *Amblodon*. Nobody has however thus far taken the trouble to examine the value of this genus, nor even to state on what ground it has been rejected by those who have incidentally noticed it as a synonym of Corvina. The truth is that Rafinesque was right in considering this Corvina Oscula as a distinct genus, the characters of which he has well defined, as may be seen by comparing his description in the *Ichthyologia Ohiensis* with that below. Moreover I have lately ascertained that there are several

species of Amblodon in different parts of the United States and that this type is not limited to the Northern States but extends west as far as the western parts of Missouri and South as far as Louisiana and Alabama.

AMBLODON, *Rafin.*—External characters of Corvina, combined with the form and appearance of Pogonias. Upper pharyngeals distinct, covered with broad, hemispherical teeth closely set, like pavement stones and arranged in regular rows; outside of these are a few small pointed teeth. The lower right and left pharyngeals are soldered together into a broad triangular plate, covered with teeth of the same kind and arranged in the same manner as upon the upper pharyngeals. In the genus Corvina the lower pharyngeals are distinct as the upper ones and support short conical teeth not numerous, nor closely set. From want of a sufficient number of specimens I am unable to determine whether the specimens from the great Lakes are specifically identical with those of the Ohio River described by Rafinesque as *Amblodon grunniens;* but I have ascertained that the species of the Ohio River differs from that of Huntsville, which I call *Amblodon concinnus*, Agass. This species differs from A. grunniens in having the body less elongated, the profile steeper, and the dorsal fin placed further forwards. The profile is most arched immediately over the upper attachment of the preopercle, in A. grunniens it is most prominent over the opercule. The dorsal fin ends slightly in advance of the base of the pectorals; in A. grunniens behind these. The serrated edge of the preopercle is directed more obliquely downwards and backwards, making the inferior angle of the preopercle more acute. This species is known in the Tennessee River by the name of Drum. It reaches there the weight of fifty pounds.

Amblodon lineatus, Agass.—This species sent to me by Mr. Geo. Stolley from the Osage River, Mo., resembles more A. concinnus than A. grunniens, but the head is shorter; the prominence of the forehead is nearer the dorsal fin, immediately over the opercle, thus having a less arched profile. The anterior border of the eye nearly reaches the profile of the head. The spines of the dorsal fin are bent more backwards. The dark coloration of the centres of the scales, especially in younger specimens produces the appearance of regular lines following the direction of the rows of scales, hence the name of this species. It grows also very large, and bears in Missouri the same name of Drum as the species of the Tennessee River. Mr. Stolley informs me that the Amblodons are very sluggish, and live at the bottom of muddy waters, where they are often seen progressing slowly, raising as it were, clouds of dirt before them, now lying upon one side of their body, then turning upon themselves or

plunging headlong into the soft ground with their body in a vertical position. They feed upon worms, and small shells, large numbers of which are often found crushed to pieces in their stomach; they however bite occasionally at a minnow.

Esoces, Cuv. (Joh. Müller.)—Though we have only the genus Esox representing this family in North America, it is perhaps not superfluous for me to state that I agree with the modifications J. Müller has introduced in this group since it was first established by Cuvier. We have one species from the Tennessee River, called Pike at Huntsville.

Esox crassus, Agass.—This species agrees fully with the type of Esox reticulatus in having both the operculum and cheeks covered with scales. It is, however, a much deeper fish than *E. reticulatus;* its scales are larger and nearly of an hexagonal form. The scales of the preopercle and cheeks are as large as those of the body; those on the opercle are smaller. The superior orbital ridges are more prominent; the depression between these ridges is deeper. The anal and caudal fins are shorter. The body is marked as in Esox reticulatus. The genus Esox has a very wide range in North America, but there is no difference of structure between those of the Canadian Lakes and the western waters, and those of the Atlantic lakes and rivers, as Mr. Girard affirms in a notice recently published in the Proceedings of the Academy of Natural Sciences in Philadelphia (1853, page 386). In the first place my Esox Boreus, from Lake Superior, does not belong to the same type as Esox Estor, its cheeks being covered with scales. Moreover, I know already three species from the western waters, one of which is noticed above, the cheeks and operculum of which are as completely covered with scales as in Esox reticulatus. There are in reality more species of the type of Esox reticulatus, in the western waters and the Canada lakes, than of the type of Esox Estor, and far from excluding one another these types occur there together. As to the application of the names Pike and Pickerel to the different type of our Esoces, it cannot be justified, since such a use would be a scientific sanction of the misapplication of English names to our native animals, which has already led to so much confusion. Unless applied as a generic apellation, the name *Pike* must be retained for the European Esox Lucius, to which only it belongs by right; whilst the name *Pickerel* designates the young of that fish. It would be quite as advisable to introduce in our scientific nomenclature the name of *Calf* to distinguish the Bisons from the type of our domesticated cattle, as to apply the name Pickerel to any particular species or set of species of the genus Esox.

(To be continued.)

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Art. XXXII.—*The Primitive Diversity and number of Animals in Geological Times;* by L. Agassiz.

There is a view generally entertained by naturalists and geologists that genera and species of animals and plants are greatly more numerous at the present age of the world than in any previous geological period. This seems to me an entire misconception of the character and diversity of the fossils which have been discovered in the different geological formations, and to rest upon estimates which are not made within the same limits, and with the same standard. Whenever a comparison of the diversity and number of fossils of any geological period has been made with those of the living animals and plants belonging to the same classes and families, it has been done under the tacit assumption which seems to me entirely unjustifiable, that the fossils formerly inhabiting our globe are known to the same extent as the animals which live at present upon its surface; while it should be well understood that however accurate our knowledge of fossils may be, it has been restricted, for each geological formation, to a few circumscribed areas. Comparisons of fossils with the living animals ought, therefore, to be limited to geographical districts corresponding in extent to those in which the fossils occur; or, more properly, a fossil fauna with all its local peculiarities ought to be compared with a *corresponding* fauna of the present period, and not with *all* the animals of the same class living at present *upon the whole surface* of the globe. And when this is done

with sufficient care and proper allowance is made for the limited time during which investigations of fossils have been traced compared with that which has been almost everywhere devoted to the closer study of living animals, it will be seen that the number and diversity of species peculiar to each special fossil fauna is, in most instances, equal to those found to characterize zoological provinces of similar boundaries, at the present day. And this may be said of the fossil faunæ of all ages. In many instances the result is even quite the reverse of what is generally supposed to be the fact, for there are distinct fossil faunæ which have yielded much larger numbers of species, presenting a greater variety of types than any corresponding fauna in the present age. Some examples will justify this perhaps unexpected statement.

The number of species of shells which are found living along the shores of Europe, does not exceed six hundred. About six hundred species is again the number assigned to the whole basin of the Mediterranean, including both the European and African coasts. Now the most superficial comparison between them and the fossil species which occur in the lower tertiary beds in the vicinity of Paris, shows the latter to exceed twice that number; there are indeed twelve hundred species of fossil shells now known from the eocene beds in the immediate vicinity of Paris, affording, at once, a very striking evidence of the greater diversity and greater number of species of that geological period when compared even with those of a wider geographical area at the present day.

If it be objected that the variety of forms which occur in tropical faunœ is greater than that which we observe on the shores of our temperate regions, and that the temperature of the tertiary period having been warmer we may expect a larger number of fossil species from those deposits, I would only refer to local enumerations of marine shells from several tropical regions, to sustain my assertion that the number of fossil shells of the eocene beds of the *immediate vicinity of Paris*, is much greater than that of any local fauna of the present period, even within the tropics. A catalogue of not quite three hundred species of shells given by Dufo as occurring around the Sechelles Islands, the extent of which may fairly be compared with that of the lower tertiary beds around Paris, will suffice to show, that in a tropical local fauna the number of species known to exist in the present day is far inferior to the number of species known to have occurred during the deposition of the lower tertiary beds in the vicinity of Paris. Another catalogue by Sganzin, of the shells found about Mauritius, Bourbon and Madagascar, gives also less than 300 species for that extensive range of seas surrounding those islands. Let us further compare the results of the investigations of the shells of the Red Sea by Hemprich, Ehrenberg and Rüppel, and there

again we find a smaller number, and a more limited variety of types than are found in the tertiary of Paris; for the whole basin of the Red Sea has thus far yielded only 400 species of shells. Let us finally take the most accurate survey of this kind we have of any shore, that of Panama by Prof. Adams, extending over 50° of latitude, 28° N. of the equator, and 22° S. of it, including the most favorable localities for the growth of shells in the Pacific under the tropics, and yet we shall find his list exceeding but little the number of 500 species. In this instance again we find that the advantage in number and variety is in favor of the tertiary period, and not of the present age. If a different result has been obtained by the estimates made before this, it is owing to the circumstance, that the *fossils known from a few localities within narrow geographical limits* were compared with the *living species known to occur upon the whole surface of the globe.* But let us trace these comparisons through other geological periods, with reference to other classes also, and we shall find in every instance, similar results. The tertiary fossils of Bordeaux, though less numerous in species than those of the eocene in the vicinity of Paris, will compare with any local fauna of the present period as favorably for variety and number of species as those of the lower tertiaries. This may be said, with the same certainty, of the tertiary shells of the Sub-appennine Hills, or of those of the English Crag of which we now possess a very complete list.

If from the tertiary periods we pass down to the cretaceous, do we not find in the deposits of Mæstricht, or in those of the age of the white chalk, a number and variety of shells as great as that which may be found on any shore or in any circumscribed marine basin of an extent at all comparable with that of the cretaceous beds within similar limits? Do we not find in the lower cretaceous strata such as the green sand or the Neocomien, other assemblages of the remains of Mollusks, which, in number and variety, are not inferior to those of the white chalk? The oolitic series, again, will stand a similar comparison quite as well. We need not even take the whole group of those deposits, but consider each subdivision of the Jurassic period by itself, and still we find in every one, local faunæ of Mollusks, assuming of course, a different character from those of the cretaceous or tertiary, but nevertheless sufficiently diversified to admit of an estimate, as advantageous, with respect to the points under consideration, and to the local faunæ of the present day as to the cretaceous assemblages of fossils, or those of the tertiary period. Of course, in accordance with the peculiar character of the age, different families prevail in these different periods; the Cephalopoda are extremely numerous and surprisingly diversified during the cretaceous and oolitic periods; while they dwindle down to

a few representatives in the tertiaries, and so with other families. The shells found in the deposits of the new red sandstone period, of the coal period, and of the still earlier ages, are perhaps less numerous on the whole, though they can hardly be said to be less diversified; for, the extinct forms which occur among them, are quite an equivalent to the variety of their families which have lived during more recent periods; and the daily increase of the species found in the different palæozoic deposits shows that, even in point of numbers those ancient faunæ may, even in the present state of our knowledge, be compared with local faunæ of similar extent at the present day.

Desirous of making the most accurate comparison possible between the *subdivisions* of the paleozoic formations of the state of New York with *local faunæ of similar extent* in the present seas, I have requested Professor J. Hall to furnish me with summary indications respecting the results of his extensive investigations in this field, and I have obtained from him the following statement:

"I regard the *Potsdam and Calciferous Sandstone* as disconnected with the groups above, forming of themselves with their fauna (not yet well known in this country) a distinct geological period. The entire number of species thus far known in these rocks, admitting all of Owen's species, is however only twenty-six."

"The *Chazy limestone* has 45 species restricted to itself, and one other species which is also known in the Black River Limestone. The *Birdseye limestone* has 19 species restricted to itself and two others which pass upwards. The *Black River limestone* has 13 species restricted to itself, and one common to it, and the Chazy limestone, one common to it and Birdseye, and one common to it and the Trenton, and one other which is common to the beds below and above, extending into the Hudson River group," making together 81 species for these three sets of beds.

"The *Trenton limestone* has 188 species restricted to itself, and 30 species passing upwards into the Hudson River group. The entire number of species known as occurring in the Trenton limestone, including those which occur in rocks above and below is about 230. This statement includes some species discovered since the publication of the 1st volume of the Palæontology of New York, and which would make the restricted species about 200."

"The *Hudson River group*, including Utica slate, has about 60 restricted species, besides those which are common to it and the rocks below, making altogether about 100 species."

"You will observe that the development of life at the Trenton period, has been far the most marked, though it is true that this

formation is much thicker than either of the preceding limestones, the Chazy being the thickest, and the Black River the thinnest of the three below the Trenton."

"In that portion of the upper Silurian period included in the 2d vol. of the Palæontology of New York, the fossils of the Medina Sandstone, Clinton group, Niagara and Onondaga Salt groups, amount to 341. *Medina and Clinton groups* 123 species. *Niagara and Onondaga Salt group*, 218 species."

"The Medina Sandstone and arenaceous beds of the Clinton group contain 50 species, leaving for the calcareous beds of the Clinton group 73 species, which, added to the 218 species of the Niagara and Onondaga Salt groups, give 291 species as the total number of species of the calcareous beds of these groups. The Niagara is here the more important period, and though not thicker than either of the others, contains about 200 species restricted to itself. Of the Niagara group 67 species are Corals and Bryozoa. Of the 73 species from the calcareous beds of the Clinton group, 19 are Corals and Bryozoa."

"In the *lower Helderberg group*, including the Water lime, Pentamerus limestone, Delthyris Shaly limetone, and upper Pentamerus limestone, I expect to describe about 200 species, exclusive of Corals and Bryozoa, of which I know already about fifty species."

"The *Oriskany Sandstone* may contain about 60 species of fossils altogether, perhaps less."

"In the *upper Helderberg group*, which is the next great Calcareous formation, I anticipate a less number of species except Corals and Bryozoa, of which there are more than 100 species in New York and the western localities. Of all that is yet known in these limestones besides Corals and Bryozoa, it would be unsafe for me to estimate more than 100 species."

"From the *Hamilton*, *Portage* and *Chemung groups* I anticipate at least 300 species within New York, and I shall not be surprised if more complete investigations produce double that number in New York and the West."

"The number of species given here I regard as only approximate. I hope this general statement may meet your present requirement, but I regret that I cannot now give you more definite information, particularly regarding the Upper Helderberg. I give you from this and the higher groups an estimate based on the species known to me at the present time; but my final investigations always reveal a greater number than I anticipate."

These statements of Professor Hall place already each of the principal group of rocks of the state of New York in the category of distinct independent successive faunæ, equivalent each to as many local faunæ of the present period, for we may repeat that the fauna of the Sechelles contains only 258 species, and that of

Mauritius, Bourbon and Madagascar, 275. Nay, upon 3000 miles of coast along the western shores of the American continent, within the tropics only twice the number of living species have been obtained as occur respectively in each successive greater subdivision of the paleozoic system within the narrow limits of the state of New York only. (See above the results of Professor Adams's investigations upon the coast of Panama.)

It is a most unexpected and very significant coincidence that the late admirable investigations of Elie de Beaumont upon the mountain systems, have led him to the recognition of nearly ten times as many periods of great disturbance in the physical constitution of the earth's surface, as he himself knew twenty-five years ago, each attended by the upheaval of as many mountain chains, differing in their main direction. The investigations of palæontologists having an entirely different character, and founded upon facts which until recently have apparently had only a remote connection with the other series of phenomena, have nevertheless brought them at about the same time to like conclusions respecting animal life, showing that the periods of disappearance and renovation of organized beings upon earth, have been much more frequent than could be supposed even ten years ago, each set having probably been characteristic of one of those long periods of comparative rest, intervening between two great successive geological cataclysms.

What is true of Mollusca, may be said of all other classes. Among Radiata, are not the coral reefs of the palæozoic ages as rich in species as any coral reef of the Pacific? Let us even compare the most extensive list of corals yet given as belonging to any circumscribed locality,—those of the Red Sea as described by Ehrenberg,—those of the Feejee Islands as described by Prof. J. D. Dana,—and let us inquire whether the palæozoic rocks of the state of New York do not show as great a variety and as large a number of species in their successive reefs. Again, the coral reefs of the oolitic period in Normandy, or in the Jura of Switzerland, and the Alp of Wurtemberg, have they not increased our lists of fossils as largely and introduced into our zoological works as various forms as are known from any of the most diversified coral regions in the world at the present day?

Passing from the corals to the Echinoderms, the question may be reversed, and it may be fairly asked whether there is any sea shore extending over tens and tens of degrees of longitude and latitude even under the tropics which has yielded as large a number of those Radiata, as occur in almost any of the geological formations? The number of Crinoids found in the single set of beds known under the name of Niagara limestone, equals the whole number of Echinoderms found around all the coast of the United States. The Crinoids, Echini, and Star-fishes of the

oolitic period, or any of the subdivisions of that formation, surpass the number of species of that class which may be gathered around the coast of entire continents in the present day. The diversity of forms of these animals comparing them with those of the cretaceous periods, is equally great, though the Crinoids begin to diminish in number. But the variety of Spatangoids and Clypeastroids which come into play, compensate largely for the diminution of the family of Crinoids.

The type of Articulata may seem, in the present condition of our knowledge, to form an unanswerable objection to the broad statement I have made above, for the hundred thousands of insects which are known in the present creation will hardly allow a comparison with the fossils. But let us examine upon the principles by which we have been guided in the preceding computations, what is the true state of things respecting the occurrence of Articulata in former geological periods. We can, of course, hardly expect to find worms well preserved in geological formations, on account of the softness of their body, which will scarcely allow of preservation to a greater degree than Medusæ. But a few instances in which impressions of these animals have been found justifies the assertion that they existed as well in former periods as now. The impressions of Medusæ found in the lithographic limestone of Solenhofen, which are preserved in the Museum of Carlsruhe, not only carry back the existence of this class to the Jurassic period, but justify the question whether a large number of the fossil polypi from older periods, which have been described as belonging to that class, are not in reality nurses of Medusæ similar to the Campanulariæ, and Sertulariæ of the present day, which are now known to be no Polyps, but one of the alternate generations of Medusæ. And as for the worms, we find in each geological formation, from the oldest to the most recent, fossil Serpulæ, or similar solid cases of worms in as large numbers as we find these animals any where at the present day. And where the existence of Serpulæ is established by such unquestionable evidence as that of their calcareous cases, are we not justified in the inference that those entirely naked worms which are found every where existing with Serpulæ, had also their corresponding representatives during former geological periods?

With the class of Crustacea the difficulty in the comparison is already less; for, in the tertiary beds of Sheppy there have been found a variety of lobsters, shrimps and crabs, which would favorably compare with the crab fauna of any limited shore in the present day; and I doubt very much whether such a variety of Crustacea could be collected any where on a shore of equal extent to that of the white chalk of Sussex, as Dr. Mandell has uncovered in the vicinity of Lewes. For a comparison of the Crustacea of the oolitic period, I would only refer the skeptic to the

monograph of the Crustacea of Solenhofen by Count Münster, who has figured from that single locality more species than are known in the whole basin of the Mediterranean, excluding the minute species which have not yet been sought for among the fossils.

In earlier geological ages, during the deposition of the coal and other palæozoic rocks, the class of Crustacea presents a very different character. The gigantic Entomostraca and the extinct family of Trilobites take the place of the lobsters and crabs of later periods. But palæontological works illustrating the fossils of Sweden, Russia, Bohemia, England and France, have made us acquainted with as great a variety of species of those families as are found of the later representatives of the class in more modern deposits. So that among Articulata the class of Crustacea can be said to have been, at all periods, as largely represented, and to have shown as great a variety of forms as occur any where within similar limits in the present time.

The carcinological fauna of the whole Indian Ocean scarcely exceeds in variety or number of species that of Bohemia alone, as it is now known by the admirable investigations of Mr. de Barande.

From their minuteness and general structure, Insects might be excepted in such a comparison without affording a sufficient argument against the view I have taken of the subject, even if insects had nowhere been found in large numbers in a fossil state. For it must be plain that their preservation requires more favorable circumstances than the preservation of other animals more largely provided with solid parts. But though the fossil insects have not been sufficiently investigated in all geological formations, have we not several examples which show that in some geological periods, at least, they were as numerous as in the present day? The beautiful Monograph of Behrens, of the insects which occur in Amber, shows how varied these animals were during the period of the formation of that gum, and the unparalleled investigations of Professor Oswald Heer upon the insects of Oeningen and Radeboy have furnished us with means of comparisons which show that during the deposition of the Molasse of Switzerland, the insects were as numerous and as diversified there as they are any where in our day, within similar boundaries. And the fragmentary information which we already possess upon the insects of Aix in Provence, and those of Oeningen will justify the expectation that insects will finally be found very numerous in all the geological periods from that of the carboniferous deposit to the present day; that is to say, ever since terrestrial vegetation has had an extensive development. The discoveries by Hugh Miller of true trees in the old red sandstone will justify the prophecy that insects will be found, some day or other, even among palæozoic rocks older than the coal period.

But what of the Vertebrata? Is there not evidence, that, at the present day, they are more diversified and more numerous? Here again I answer, decidedly, No; granting only that there are periods during which the higher classes of these types did not exist, and that therefore, as a type, the vertebrata of the present day are more diversified; but the individual classes, from the time of their appearance have been in each former period, as numerous and even as various as they are at present.

Let us apply to these the same measure which we have applied to the Radiata, Mollusca, and Articulata, to justify this assertion, which seems so completely at variance with our knowledge of fossil vertebrata. Fishes occur, as is well known, in all geological formations. But should we compare the fossil fishes of each geological period as they are known from a few localities, with the whole number of fishes which exist all over the world in our day? It would be as unphilosophical as it would be inconsistent with our knowledge of the geographical distribution of animals. Like all other living beings, fishes are located within definite boundaries, and it will be but fair to compare the fossil species of a given locality with the special Ichthyological faunæ which occur in different oceans, or in different fresh-water basins. Now with this rule we may institute a comparison of the fossil fishes with the living ones, with reference to their number as well as to their variety.

The number of species of fossil fishes known at present from the tertiary deposits, in a single spot, upon the Island of Sheppy, is greater than the number of fishes which have been gathered around the coast of any of the islands of the Pacific Ocean, as far as we know the local Ichthyological faunæ of those regions; it is as great, nearly, as the whole number of fishes known from the shores of Great Britain. The same may be said of the fishes of Mount Bolca, or of Mount Lebanon, or of those of the white chalk of England, or of those of Solenhofen, or of those of the lias of Lyme Regis; and if we pass to older deposits, to the old red sandstone even,—thanks to Mr. Miller, and to the investigations of other British and Russian geologists,—do we not know from that old formation as many fishes as from any of the more recent ones, or from any circumscribed marine basin? and is not the variety which occurs among them at each period as great, though of a different character in each, as the variety which occurs at the present day? So that it can be fairly said, that at all periods, fishes have presented as great a variety of forms, and as numerous species, as under corresponding circumstances at the present day.

The class of Reptiles will allow similar conclusions, for though the giants of the class have chiefly been studied, do they not indicate an abundance, and a variety of these animals during the

upper secondary formations, as great as in any tropical region? and have we not sufficient indications among the tertiaries to be justified in expecting that they also will turn out to be more numerous than they are now known to be?

The class of Birds seems to form an exception in this view. But there seems to be particular reason why the bones of birds should be more liable to destruction and decomposition than those of other vertebrata. And whoever has traced the discoveries made recently among the fossils of this class, will certainly not insist upon a supposed scarcity of birds in former periods, but rather be inclined to admit that the limited number now known is to be ascribed to the deficiency of our knowledge rather than to a want of these animals in earlier formations, indications of their presence having been ascertained for several tertiary formations, for cretaceous deposits, and even for deposits belonging to periods older than the chalk.

Fossil Mammalia are comparatively too well known to call for many remarks, after what has been said above. Let us only remember that the number of fossil species found in Brazil alone equals the whole number of Mammalia known to live at present in that country; that the fossil Mammalia of New Holland compare already favorably with the living species of that continent; and that the locality of Montmartre alone has yielded as many large Mammalia as occur all over Europe, and the Mauvaises Terres in Nebraska as many as may be found in North America now. So that, if we grant simply that among vertebrata the diversity has been increasing with the successive introduction of their different classes, the number and diversity of these different classes at each period has been as great as it is at present.

These facts are of the utmost importance with reference to the great question of the order of succession and gradation of animals in the different geological periods. They cut away forever one of the arguments upon which the asserters of the development theory have insisted most emphatically. Before it could be granted that the great variety of types which occur at any later periods has arisen from a successive differentiation of a few still earlier types, it should be shown that in reality in former periods the types are fewer and less diversified; and we have now shown that this is so far from being the case, that in many instances the reverse is really true. I have already attempted elsewhere to show in outlines what is the real order of succession of the great types of the animal kingdom, I need not therefore repeat here what may be gathered from the diagram at the head of the Zoological Text Book I have published jointly with Dr. Gould. I shall limit myself to a few more general remarks upon the special difficulties involved in a more thorough investigation of the subject.

The study of the order of succession and gradation of the organized beings which have inhabited our globe at different periods, presents indeed difficulties of more than one kind. Unhappily these difficulties have seldom been all considered in their natural connection by those who have ventured to consider the subject in its whole extent; thus presenting certain results as general which would require various qualifications to be true. In comparing fossils of one and the same or of different geological formations, it is in reality not enough to ascertain their true geological horizon, which we may call the *chronological element* of the enquiry; it is equally important that the differences or resemblances arising from the geographical distribution over the wide expanse of the whole surface of the globe, which we may call the *topographic element* of the question, should be also considered, for it is already known that within certain limits the same differences and resemblances which are observed at present between the animals inhabiting different parts of the globe existed already in former geological periods. We must therefore become acquainted with the *general biological character* of the epoch as well as with the *local faunæ* of each period. The tertiary faunæ of New Holland and the Brazils for instance, resemble more closely the living faunæ of those parts of the world than they resemble one another. Our lists of fossils teem with chronological errors of the worst kind, arising partly from false identifications of strata, which in reality belong to different periods, but the fossils of which are thus represented as having inhabited our globe simultaneously, when in reality they may have been separated by long periods of time, and existed upon earth under very different physical conditions. This chronological confusion is further increased by the too extensive limits frequently assigned by geologists to the successive groups of rocks forming the crust of our globe. For instance, when the cretaceous or the oolitic formations are considered respectively as indivisible natural groups, and the fossils of all their subdivisions are enumerated in one single list as the inhabitants of a long period, an infinitude of anachronisms are presented to the mind, which no special mention of localities can rectify; and until the fossils of each of the natural subdivisions of these formations shall have been grouped together and compared carefully, as I have attempted to do it in my Monographs of the *Trigoniæ* and of the *Myæ* of Switzerland and the adjoining countries, or as Al. d'Orbigny has done it upon a much larger scale in his Paléontologie Françoise, no correct ideas can be formed respecting the succession of animals and plants characteristic of these long successive periods. I do not believe there is a single palæontologist, whose opinion is worth having, who can suppose, at this day, that any of the animals, the remains of which are buried in the lias, lived simulta-

neously with those of the inferior oolite, or these with those of the Oxford clay, or these with those of the upper division of the so-called oolitic formation. The same may be said of the different natural subdivisions of the cretaceous formation, and of the subdivisions introduced of late among the palæozoic rocks, by Sir Roderick Murchison, and Professor Sedgwick, and in America, by Professor J. Hall.

But even after this separation of the fossils, the synchronism of which may be fully established, our task is only fairly laid open, for then must begin the zoological identification of all the species, which has to be correct in every respect before general conclusions can be drawn from it.

In the first place the *specific identity* of organic remains is not so easily ascertained as many geologists would seem to suppose, if we judge from their statements; but unless the validity of a species is sanctioned by a practiced Zoölogist, it can not be taken as a basis for sound generalizations in reference to questions of a purely zoological character. The number of false identifications which have been accumulated in geological works is truly frightful. It would be however very unjust to accuse geologists in general of inaccuracy for this, the fault is mostly to be traced to other parties from which the names were obtained. It should only be understood that the materials thus accumulated are no longer fit to be used for the discussion of the questions which have been raised with the modern progress of geology, and that a thorough revision of *all* the identifications made in former years is imperatively demanded by the modern progress of palæontology. It would be however sometimes amusing, were it not actually distressing, to see the manner in which some geologists deal with fossils, considering them simply as the characteristics of certain rocks, and hardly yet dreaming that there may be such a thing as a special zoology of the different geological periods, and that during each, local faunæ may have existed with peculiar animals, &c. The ideas about characteristic fossils are still very crude, and nothing is more absurd than the complaints about unnecessary multiplication of genera and species; as if both genera and species had not a natural existence, independent of the estimates of naturalists. It would be just as reasonable for astronomers to complain of the great number of stars, as for geologists to object to the investigations of zoologists, on the ground that they lead to the "making" of "*too many species.*"

The difficulty with reference to the identification of species is three-fold: 1, different species may be considered as identical, 2, specimens of the same species in different states of preservation, or of different age, or sex, &c., may be considered as distinct species, or 3, the same species may have been described by different authors under different names, and their identity afterwards over-

looked by later writers. Who does not see what amount of error may accrue from the indiscriminate use of materials which are not first submitted to a very critical revision in these different respects, not to speak of the general difficulty of agreeing upon the limits of specific differences. With regard to this last point, however, I would say that whosoever would only use in discussing general questions materials revised candidly with the same principles, could not fail to obtain at least uniform results. And when the results of investigations made upon materials corrected in different ways by different authors are compared with one another, if these differences are kept in view, the disagreement in the results would not be found so great as it might otherwise seem. The astronomers and physicists have long learned to correct their observations before using them, and to take into consideration what they call the personal equation of different observers; are we never to learn from them a lesson in the estimation of our respective investigations, and shall our facts for ever be used without being first corrected for all the possible causes of error and disagreement? As long as there are differences of opinion respecting the natural limits of genera and species, is it not absolutely necessary to reduce or expand the scale applied to the investigations of different authors, when using them for the same purposes, exactly in the same manner as thermometric observations made with the scales of Reaumur or Celsius or Fahrenheit are reduced to the same standard, before being compared.

In the second place, *species must be referred to genera circumscribed within the same limits*, before they can fairly be compared or at least lead to trustworthy general results. As long as certain bivalve shells of the carboniferous and oolitic series were referred to the genus *Unio*, it could appear that the family of *Naiades* began its existence at a very early period; but since the oolitic species of this kind have been ascertained to differ essentially from our freshwater shells, and to constitute by themseles a natural genus more closely allied to Crassatella than to Unio, nobody thinks any longer of looking for Unios in *marine deposits*. As long as certain fossil fishes of the Zechstein and Lias were referred to the genera Esox and Cyprinus, the families of which these genera are the types could be supposed to have extended their range far beyond the tertiary formations, before which however no one of their representatives is to be found. Before the Spatangoids were divided into natural genera, the genus Spatangus was mentioned among the fossils of the oolitic as well as the cretaceous and tertiary formations; now it is restricted to the last among the fossils and found also among the living. I do not believe that a single genuine species of Gorgonia is found among the fossil Polypi, and yet that genus appears in the lists of fossils from the palæozoic period to the present time.

Since it is not my intention to enter here upon a special criticism of the innumerable errors of this kind, still to be found in even modern lists of fossils, I shall not multiply my examples. These may be sufficient to show how important a correct *generic* identification of the fossils may be in the estimation of the order of succession of organized beings; and I cannot but lament the utter want of consideration evinced even by many distinguished palæontologists in this respect, who seem to think that the knowledge of species is sufficient in itself to a proper appreciation of the order of creation, and that genera are arbitrary divisions established by naturalists merely for the sake of facilitating the study of species, as if the more general relations of living beings to one another were not as definitely regulated in all their degrees by the same thinking mind, as the ultimate relations of individuals to one another.

In the third place *the natural affinities of genera should be ascertained.* Unless the genera are referred to the families to which they truly belong, unless the rank of these families in their respective classes is positively determined, unless the peculiarities of structure which characterizes them is taken as the foundation of such an arrangement and further corroborated by the mode of development of their respective types, it would be a hopeless task to attempt to determine the order of succession of the fossils in different geological formations. Before the Crinoids which Lamark placed along the Polyps had been referred to the class of Echinoderms, nobody could have understood the beautiful gradation so fully ascertained now, which may be traced through all geological formations among these animals. Before it was ascertained that the little animal described by Thompson under the name of Pentacrinus europeus, as a living Crinoid, for which DeBlainville established the genus Phytocrinus, is in reality the young of a Comatula, nobody could have suspected the wonderful relations which exist between the changes animals now living undergo during their growth, and the order of succession of entire classes of animals during successive geological ages. As long as the natural position of Trilobites remained doubtful in the animal kingdom, the characters of the prototypes of the class of Crustacea could not be appreciated. Who does not see how impossible it was for those who classified the Trilobites with the Chitons to arrive at any sound results respecting the gradation and order of succession of these animals? Whilst now they are beautifully linked to the Macrura of the Trias, by the gigantic Entomostraca of the Devonian and Carboniferous periods. Again, the knowledge of the embryology of Crustacea gives us a key to a correct appreciation of the early appearance of the Macrura and the late introduction of the Brachyura. The removal of the Bryozoa from among Polypi to the class of Mollusks, will entirely

change the aspect and relations of the faunæ of the paleozoic rocks. How different, again, would the order of succession of Mollusks appear, were we to adhere to Cuvier's view of separating the Brachiopods, as a class, from the other Acephala, to which they are now more correctly referred The vexed question of the period of appearance of Dicotyledonous plants in the geological series would have been settled long ago, had it been placed upon its real foundation. It is not in reality to be argued upon palæontological evidence chiefly, for it resolves itself in the main into a botanical question, and the definite answer must depend upon the position finally assigned by botanists to the families of Coniferæ and Cycadeæ. If these natural orders of plants are really allied to the Dicotyledonæ, then this type begins with the palæozoic rocks in the Devonian system, and there is no gradation in the order of succession of plants during geological times. But if the view of Brongniart is more correct, if the Coniferæ and Cycadeæ have to be separated from the Dicotyledonæ as Gymnospermæ, and if moreover these latter should prove, as I believe they are, inferior even to the Monocotyledoneæ, then we may at once recognize in the vegetable kingdom a similar gradation of types as among animals. These examples may suffice to show what is required for a proper investigation of the order of succession of organized beings in the course of time, and how little confidence the investigations in this field deserve, which have not been made with due reference to all the points mentioned above. It is indeed only in the classes, the structure and embryology of which is equally well understood, we are able to discover the laws regulating the succession of animals and plants in geological formations, and our knowledge is at present still too imperfect to carry the investigation into all families of the animal kingdom. And yet enough is known to leave no doubt as to the final result; we may confidently await the time when the glory of the wonderful order of creation shall be fully revealed to us, and this may stimulate us to renewed efforts, since the success depends entirely upon our own exertions.

The geographical distribution of animals began only to be studied long after systematic zoology had made considerable progress, but even to this day the limits of the faunæ are nowhere circumscribed with any kind of precision, the principles upon which they might be determined are in many respects questionable, and a large number of animals are daily described without any reference to their natural distribution upon the earth; though much has already been done since Buffon to place this branch of our knowledge upon a better foundation, and especially to ascertain the laws regulating the geographical distribution of certain classes and families considered isolately. The point which requires now particular attention, is the combination of these differ-

ent types within definite regions, and their common circumscription within natural zoological provinces. This study would be particularly important with reference to the comparison of the local faunæ of former geological periods with those of the present creation. But since the latter even are comparatively little known, we must be satisfied to wait for the time when thorough comparisons shall be possible between the local faunæ of each and all geological periods *inter se*, and with those of other periods.

In closing this digression, I may sum up my criticism upon palæontological investigations, by saying that any generalization respecting the succession of organized beings which is not based upon materials in which the synchronism and succession of species and their geographical distribution is not duly considered, and in which the identification of species is not made with reference to sound zoological principles, with due regard to the equal limitation of genera, and also with reference to our improved classifications in zoology, is not fit to be trusted. All species taken into consideration should undergo a revision with reference to their chronology, their topography and their zoology, and in the last point of view the range and natural limitation as well as identity of the species, their generic affinities and their zoological classification should be equally tested.

Returning now to the main subject of this paper, I have further to say that the very fact that certain stratified rocks, even among the oldest formations, are almost entirely made up of fragments of organized beings, should long ago have satisfied the most skeptical that both *animal and vegetable life was as active and profusely scattered upon the whole globe, at all times and during all geological periods, as it is now.* No coral reef in the Pacific contains a larger amount of organic debris than some of the limestone deposits of the tertiary, of the cretaceous, or of the oolitic, nay even of the palæozoic periods, and the whole vegetable carpet covering the present surface of the globe, even if we were to consider only the most luxurious vegetation of the tropics, and leave entirely out of consideration the whole expanse of the ocean, as well as those tracks of land where under less favorable circumstances the growth of plants is more reduced, would not form one single seam of workable coal to be compared to the many thick beds contained in the rocks of the Carboniferous period alone.

ART. XXXIII.—*New Localities of Meteoric Iron;* by CHARLES UPHAM SHEPARD, M.D.

1. *Tazewell, Claiborne county, Tennessee.*

FOR the specimens of the highly interesting mass here described I am indebted to Prof. J. B. Mitchell, of the East Tennessee University, at Knoxville.* Though but a fragment of three $\frac{1}{1}\frac{3}{6}$ths pounds, (having been detached from a mass originally weighing sixty pounds,) it nevertheless has much the appearance of an independent meteorite. Its shape is that of an elongated three-sided pyramid, whose axis is slightly oblique, and whose upper edges are obscurely truncated, so as to resemble an imperfectly formed six-sided pyramid of corundum. The height of the mass is four inches. The base is triangular and nearly smooth, presenting however a cleavage surface, partially coated by brown oxyd of iron. By this face, it was originally connected with the larger mass, of which it doubtless formed one of the sharpest extremities. It is certainly very remarkable that the cleavage should have been effected without leaving any hackly projections, the more so as the mass itself by no means yields to a similar cleavage in any direction whatever. Possibly the cleavage was occasioned by the interposition of a seam of pyrites, in the direction in which it took place. At any rate, its occurrence shows that these lumps, though generally composed of very tough and strongly coherent materials, are nevertheless susceptible of cleavage in certain directions, and that they may occasionally explode or subdivide into numerous fragments, without the necessity of any very considerable force.

The upper planes of the pyramid are indented, and somewhat undulating, as is usual in meteoric irons; but there is no thick

* The following is an abstract from Prof. Mitchell's letter in reference to its discovery: "This meteorite was found April, 1853, about ten miles west of Tazewell, Claiborne county, Tennessee. It was discovered by a son of William Rogers, while ploughing in clayey ground on the side of a hill, where the soil had been much washed away by rains. His attention was arrested by the resistance and noise produced, when the mass was struck by the plough. The lump weighed about sixty pounds. It was very irregular in its shape. The period of its fall is of course unknown. On account of its weight and lustre, it was regarded as silver; and it was with no small difficulty that the finder was induced to part with it, even by my paying him what appeared to be an equivalent, and then agreeing to give him its weight in silver, provided it should prove to be that metal, when properly examined. For my first information of the iron, I am indebted to J. C. Ramsay, Esq., a gentleman who, not limiting his researches to the mere details of his profession, loses no opportunity of contributing to several branches of natural history. I retain a fragment of about six ounces which he first gave me for examination. The remainder of a mass was broken when I saw it into two pieces, one of which, weighing perhaps three and a half pounds, I send to you. The largest portion, I transferred as I informed you, to an acquaintance for examining and reporting upon the same. These three embrace all the pieces into which this meteorite has been divided."

incrustation of peroxyd: on the contrary, it merely possesses for a coating a thin, brownish-black pellicle, which is much covered also by firmly adhering clay.

The iron is highly crystalline in its texture; a fact which may be seen in a few spots upon the surface, even through the coating itself. It is exceedingly tough, breaking with the greatest difficulty, and having a hackly surface, in which no crystallization is apparent. The fresh surface is much whiter than pure iron; and it retains its color and lustre apparently without change from ordinary exposure to the air. Its specific gravity = 7·30.

A part of the broad cleavage surface (or base of the pyramid) above described, was polished, and acted upon by dilute hydrochloric acid. The corrosion was very partial; but it revealed a perfectly crystalline structure in the iron. The subsequent application of nitric acid rendered it still more striking. The Widmannstättian figures are somewhat peculiar. While there is a general ground subdivided by innumerable thin and perfectly straight lines, into small equilateral triangles, and oblique-angled parallelograms of similar areas in size, presenting a picture on the whole closely resembling the Guildford (North Carolina) iron, there are also irregularly disposed veins, or interrupted seams, of a shining, white metal, $\frac{1}{30}$th of an inch in thickness, and each from $\frac{1}{2}$ to $\frac{3}{4}$ of an inch long. These occur on the whole pretty near together, and impart a singular aspect to the surface, inasmuch as the veins do not coincide in direction with the fine lines above mentioned; nor do they follow any parallelism with one another.

Neither of the acids employed attack this substance in the slightest degree, any more than they do the thin lines producing the small and regular areas. But closely associated with the larger veins are noticeable small particles of magnetic pyrites, which as usual are decomposed by the acid.

Having separated a few grains of this metal or ore forming the seams, and heated it with acids, I convinced myself that it is identical with the substance which I discovered as entering into the composition of the Seneca Falls (New York) meteoric iron, and which I denominated Partschite.

A fragment of the iron was treated with hydrochloric acid. The solution went on very slowly, and unattended by the extrication of any sulphuretted hydrogen. The solution proceeded so slowly that it required nearly three days to dissolve 26·5 grs. of the iron, although the process of digestion was several times hastened by the application of a gentle heat. The acid left behind 1·16 grs. of undissolved matter, in the form of innumerable brilliant crystalline scales of an iron-gray color, and a high metallic lustre. When washed and dried, they were found to be flexible, highly magnetic, and insoluble in hydrochloric acid;

but were readily attacked by hot nitric acid, though still leaving undissolved a few particles of another metallic species, supposed to be the Partschite, and which were finally taken up by digestion in warm aqua regia. The thin, crystalline scales, undoubtedly consist of the schreibersite (of Patera).

From the hydrochloric solution a precipitate was obtained (by means of a stream of sulphuretted hydrogen), which, after washing and reduction before the blowpipe, yielded metallic copper. A solution of the perchlorid was precipitated by ammonia, and the peroxyd of iron thus obtained was ignited with nitrate of potassa; when its solution gave decisive evidence of the presence of chromic acid.

The proportion of nickel obtained from the iron (without including the schreibersite and partschite) was 12·10 to 13·05 p. c. —thus placing the present meteorite, as regards the high proportion of nickel, in the rank of the following rather small number of meteoric irons, viz., that of Caille, which has 17·37, of Claiborne, Alabama, which has 12·66, of Greene county, Tennessee 14·7, of Krasnojarsk 10·7, of Bitburg 11·9, and of Cape Colony, Africa 12·27.

I have abstained from a complete analysis of the present iron, as Prof. J. Lawrence Smith is, as I understand, about to publish full results of this nature.

2. *Haywood county, North Carolina.*

This specimen, which weighed scarcely ⅛th of an ounce, was sent to me by Hon. T. L. Clingman, accompanied by the following remark: "It was given me by a person in Haywood county, whose father had obtained it in that region, but without his being able to designate the locality. It is evidently meteoric iron, but is perhaps from some mass already known."

The fragment is highly crystalline, and somewhat tetrahedral in form. One side was polished and etched. It displayed a marked character; and one which has no analogue among our meteoric irons. It is irregularly veined by a black ore, which was not acted upon by the acids; and which, when separated and submitted to examination, presented all the properties of magnetite. The general ground of the etched pattern is almost identical in character with that of the Hauptmannsdorf (Braunau) iron, it being characterized by this, that instead of brilliant projecting lines, it has fine, depressed lines, or grooves, which however are bright and glimmering in a strong light, and meet each other, mostly *at right angles*, parting off the ground into squares and rectangles, which are also to some extent, diagonally streaked, though always in a much fainter manner.

Specific gravity = 7·419. It dissolves in hydrochloric acid without the odor of sulphuretted hydrogen. The solution, when

perchloridized, gave with ammonia a precipitate which afforded distinct evidence of the presence of phosphorus and chromium: and the ammoniacal liquor was rich in nickel.

The small quantity of the iron at my disposal prevented a fuller examination of its properties. It is much to be desired that the present notice may lead to the discovery of the original mass.

3. *Union county, Georgia.*

For my specimen of this iron also, and for my chief information respecting its discovery, I am indebted to Hon. T. L. Clingman, as will appear from the following extract from his letter, dated Washington, Nov. 16, 1853. "Not long before I left home, I was at the copper mines of Ducktown, Polk county, Tennessee; and while there, in looking over some specimens of Mr. S. Congden, I found this ore, and told him that I was satisfied it was meteoric iron. He had taken it to be merely a rich iron-ore, and informed me that some weeks previously, it had been brought to him by a person, who had picked up a large lump of it in his field, and who had broken off this piece with a view to having it tested. The discovery was made in the edge of Georgia, but in what county Mr. Congden could not learn."

I have more recently ascertained from Mr. B. R. Dickey of Habersham county, Georgia, that the mass was found by a Mr. Freeman, in Union county of that state; and that its weight when picked up was about fifteen pounds.

The specimen sent to me by Mr. Clingman weighs one pound and one and three quarter ounces; and appears to have formed a portion of a somewhat tabular mass about two inches in thickness. It is coated on three sides, with a thin, scaly covering of brownish black hydrated peroxyd of iron. The other two sides present the appearance of a fresh fracture, but are nevertheless destitute of metallic lustre,—the surfaces being very irregular and dependant in form, upon the peculiar, concretionary character of the mass, which is strongly analogous to that of a very coarse-grained colophonite garnet, or the coccolite variety of pyroxene. It is, however, more or less traversed by cylindrical, or almond-shaped masses of meteoric pyrites, some of which are above an inch in length and one-third of an inch in diameter.

When polished, it approaches more nearly to a silver-white color, than any other meteoric iron. When acted upon by acids, it does not give the Widmannstättian figures; but only develops a series of web-like meshes, or, at most, a mottled, map-like delineation.

Its specific gravity $= 7{\cdot}07$. A fragment, as nearly as possible free from pyrites, was found to contain 3·32 p. c. of nickel. It is rich in chromium, and contains traces of phosphorus, cobalt, magnesium and calcium.

4. *Meteoric Iron? from Long creek, Jefferson Co., Tennessee.*

This iron was forwarded to me by Judge J. Peck of Oakland, East Tennessee, (to whom we owe the discovery of the Cosby Creek iron, and that from Green county in the same state) with the following remarks respecting its discovery. "It was found on Long creek, Jefferson county, a few miles north of the mouth of Chucky creek. Before I got possession of it (which was accidental) it had fallen into the hands of a young vulcan, who would try its metal, as you see. I regret its being disfigured: but I have saved all I could. Near the same place, another piece was picked up, of which, however, I possess a mere fragment; but it appears to be quite identical with the specimen I send. The pieces are described to me as being of about the same size and not unlike in figure. The family who made the discovery, affirmed to me, when I visited the locality, that there was a vein of it, and that wagon loads could be picked up. But the creek where the vein was said to be, was too high. I made a liberal offer for all they could find and bring to me, in an unaltered condition; but I hear of no more. Feb. 7, 1853."

The mass sent, weighed about two and a half pounds, and had an oval flattened form, much like a thick, freshwater bivalve shell. It is somewhat flattened on one side, and presents a broad fractured surface on one side, as if it had been broken from a larger mass. The natural outside is somewhat undulating and pitted, and bears some marks of having been heated and hammered. But it exhibits a very marked peculiarity in being coated almost uniformly by a layer one-twentieth of an inch thick, of specular iron, which has a botryoidal surface, and a concentric, lamellar structure. This coating moreover, often penetrates by little irregular veins, for half an inch or more, into the substance of the mass.

It breaks with difficulty; at first, slightly flattening under the hammer. The fresh fracture shows no bright metallic points. It presents a granular surface, much resembling certain rather fine grained blackish chlorites, or some varieties of graphite. Here and there throughout its mass, are noticeable rounded globules of metal from $\frac{1}{16}$th to $\frac{1}{8}$th of an inch in diameter. These are quite smooth externally; and when detached from their bedding, leave behind a smooth corresponding cavity. The material of the globules does not differ sensibly from the rest of the mass, except in being rather finer grained.

Specific gravity $=7{\cdot}43$. When polished, it presents a dark iron-grey color, and an imperfectly metallic lustre. Acids do not develop any crystalline texture with this iron.

It dissolves easily in hydrochloric acid, with no perceptible odor of sulphuretted hydrogen, but nevertheless slightly discoloring a

paper wet with a solution of acetate of lead, when held over the solution. A mineral resembling graphite rapidly separates as the solution goes on, and floats through the liquid in small shining black scales of perfectly uniform size. When no heat is employed in the process, their form is very definite, and on being washed and dried, they gave in one instance a specific gravity of 3·30. Deflagrated with nitrate of potassa they gave decided traces of titanic and silicic acids. Where heat and aqua regia were employed in acting upon the iron, the graphite-like substance was more acted upon, and had a specific gravity of but 2·20. This when deflagrated with nitre also afforded traces of silicic and titanic acids. The proportion of the heavy graphite in the iron, obtained in the first process was 4·5 p. c.; but by the latter process it was 3·3 p. c.

A portion of the iron was heated with hydrochloric acid, and the solution saturated with sulphuretted hydrogen. A yellowish brown precipitate was formed. It was separated and treated with concentrated nitric acid. A heavy, white powder was formed, which was not reduced in quantity by further digestion in the acid. It was separated, washed, dried and ignited, and amounted to 0·4 gr. or 5·50 grs. of the iron.

A portion of the nitric acid solution, on being treated with sulphuretted hydrogen, gave a brown precipitate of sulphuret of molybdenum. Another portion gave with tannic acid, a yellow brown precipitate; and, another still, gave with proto-nitrate of mercury, a yellow precipitate of molybdate of mercury, which was dissolved by nitric acid. The iron affords decisive indications of the presence of phosphorus and of chromium, when subjected to the usual tests for these elements; but does not contain nickel, cobalt, magnesium or calcium. Besides the specular iron of the crust, therefore, it contains,

Iron,	95·575
Carbon,	3·30
Chromium, Tin, Molybdenum,	1·125
Silicon, Titanium, Phosphorus, Sulphur,	*traces*
	100·00

ART. XXXIV.—*Description of Meteoric Iron from Putnam County, Georgia;* by J. E. WILLET, Professor in Mercer University, Geo.

THIS interesting meteoric iron, the first that has been found in Georgia, was presented to Mercer University by John A. Cogburn, Esq., in the fall of 1852.

The circumstances of its discovery, as detailed by Mr. Cogburn, are briefly these. The iron was first observed by his overseer, in 1839, in a field which had been cultivated for several years; but was supposed to be the common black rock of that region. Mr. Cogburn first noticed it March, 1840, and, attempting to raise it from the ground, found it so heavy that he carried it to his blacksmith shop to have it broken. Its weight, at that time, was 72 pounds, and the mass was coated deeply with a brown, scaly crust. He attempted to break it upon an anvil, but could remove only the outside crust, including a large blister, the place of which is now indicated by a deep fissure. Finding it so untractable, he threw it out into his yard, where it lay neglected; until a knowledge of the fact led me to request him to send it to the University for examination. He states further, that he supposes it to have been originally buried, and brought to the surface of the earth by cultivation and the action of rains; that there is no tradition of its fall; and that no similar pieces have been found in the neighborhood.

Its weight, when it was brought to the University, was about sixty pounds. In shape, it represents a rude triangular pyramid, with its base and edges rounded, and its faces exposing many knobs and depressions.

Most of the crust has been removed by the rough handling which it has received. The outer layers of what remains separate in thin scales of no regular shape; the inner portions break into rhombohedral masses, which, under the influence of a magnet, become permanently magnetic; showing that the iron has here been converted into magnetic oxyd. The mass of iron exhibits no magnetism.

In removing a slab, the iron was found to be remarkably tough and compact. The torn edges oxydized rapidly and developed the crystalline structure, before the application of acid; the oxydation proceeding inwardly from the edges and following the lines of cleavage first, and afterwards spreading over the inclosed areas. The sawn surfaces, after a few days exposure, were found bedewed with drops of a liquid, supposed to be chlorid of iron. After longer exposure, the exudation has ceased; a point of striking similarity with the Texas iron. The polished surface is uniform, without markings, and with few flaws.

Hydrochloric acid, applied to the heated slab, attacks it, with a rapid evolution of hydrogen bubbles, but develops only a few

of the larger bars; and the crystalline structure of the mass might be overlooked with the action of this acid alone. Nitric acid, however, brings out the Widmanstättian figures most beautifully. The etched surface is a perfect miniature copy of the Texan iron; the largest bars of the Putnam county iron corresponding with those of medium size in the Texas iron, and thence diminishing to bundles of striæ hardly visible to the naked eye. The triangles and parallelograms are proportionally small. Query: Are the crystalline figures of meteoric irons, in any degree, proportional to the meteoric masses? If so, may we not infer from the size of them, whether the iron be an entire mass, or a fragment of a large one?

Neutral sulphate of copper produces no precipitate of metal on the iron; the slightest addition of acid causes the deposit of copper. Moreover, I find, that if the film of copper be wiped off as soon as formed, the sulphuric acid has etched out the figures superficially but very imperfectly. Liquid sulphuric acid, when cold, has no effect upon the surface.

In addition to the above description, I subjoin an interesting note from Prof. Shepard, containing an analysis of the iron, which he has very kindly furnished at my request.

"Charleston, S. C., February 10, 1854.

My Dear Sir—In comparing the Putnam county meteoric iron with specimens from other localities, I notice a striking similarity in its structure to that of the Texan mass. Like it, your iron is compact, nearly free from pyrites and but slightly disposed to rust on exposure to the air. But the resemblance between the two is seen to the greatest advantage, when etched samples are compared with one another. The Putnam county iron exhibits figures of the same shape and size as the Texas, viz., triangles and oblique-angled parallelograms, bounded by slightly raised edges, which are often wavy, and sometimes not continuously of the same thickness, but, here and there, bulging out into beads or knobs. The pyrites in my specimens is scarcely to be recognized, except in one or two very limited patches, which are irregular and vein-like.

The iron appears to have suffered a very remarkable disintegration to the depth of half an inch or more below the thick, scaly crust with which the mass was coated; in consequence of which it cleaves very regularly, like the Cocke county, Tennessee iron, into tetrahedral and rhomboidal fragments.

The specific gravity of the fresh internal portions of the mass is 7·69. A single analysis gave me the following result:

Iron, ...	89·52
Nickel, with traces of cobalt, ...	8·82
Tin, phosphorus, sulphur, magnesium and calcium,	1·66
	100·00

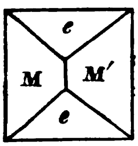
Very truly, yours,

CHARLES UPHAM SHEPARD."

Art. XXXV.—*On Conistonite, a new Mineral Species;* communicated by R. P. Greg, F.G.S., of Norcliffe Hall, near Manchester.

Mr. B. Wright, a mineral dealer in Liverpool, forwarded me some months since, a specimen, found at the copper mine near Coniston in Cumberland, by Mr. Marrat, a teacher of Natural History in Liverpool. The specimen was of a purplish-red color, somewhat resembling earthy cobalt-bloom, and implanted on it were a few small crystals resembling calcite. Thinking however the form of these rather peculiar, on trial I found the usual cleavages of that mineral wanting; and its peculiar behavior before the blowpipe soon convinced me that it must be a new species. I give the following general description:·

Largest crystals not more than one-eighth of an inch across, and in general form not unlike the double four-sided crystals of Edingtonite. Primitive form probably a right rhombic prism. No cleavage observable. Fracture small conchoidal, uneven. Lustre vitreous. Transparent to translucent; colorless. Slightly sectile. H. = 2·2. Sp. gr. = 2·05.



M : M′ = 97·05
M : *e* = 122·50
e : *e* = 94·00

The faces M brighter and more distinct than *e*.

Does not effervesce in acids. Before the blowpipe becomes white and opaque, expanding into seven or eight times its original bulk. After exposure to heat almost instantaneously dissipated in acids, with strong effervescence.

As yet only two specimens of this interesting mineral have been found. I have called it Conistonite from its having been first discovered at Coniston.

It is very probable that the matrix in which the crystals of Conistonite are imbedded, will itself prove to be a new mineral species. Mr. Heddle supposes it to be oxalate of cobalt.

I transmitted to my friend, Mr. M. Forster Heddle, of Edinburgh, a few crystals of the Conistonite for analysis, which he kindly undertook to make; and he has sent me the following particulars respecting it.

a. "Sp. gr., determined on 3·666 grs., found to be 2·052.

b. Powdered mineral absorbs ·23 per cent. of moisture.

c. Soluble without effervescence in hydrochloric and nitric acids, and on addition of ammonia a precipitate is thrown down.

d. When heated gives off water and carbonic oxyd, and is converted into carbonates; and *then* effervesces in acids.

e. Qualitative analysis made on a very minute quantity, gave indications of lime, magnesia, water and oxalic acid.

f. Quantitative analysis made on 3·83 grs.:

Carbonate of lime,	87·597	which corresponds to	Lime,	21·055	21·877
Water, and Carbonic oxyd,	60·052		Soda and Magnesia,	·822	
Carbonates of soda and magnesia in nearly equal quantities,	1·400		Oxalic acid,	28·017	
			Water,	49·155	
	99·049			99·049	

g. Uniting the soda and magnesia with the lime to calculate the formula, we get,

			Atom.
Lime,	21·877 =	625 =	1
Oxalic acid,	28·017 =	622 =	1
Water,	49·155 =	4369 =	7

which gives the formula $CaO, C_2O_3, 7HO$; being an oxalate of lime with 7 atoms of water instead of *one;* or more probably oxalate of lime with 6 atoms of water of crystallization, thus $CaO, C_2O_3, HO+6HO$; or $\dot{C}a\,\dddot{C}+7\dot{H}$.

h. The great interest of the mineral lies in the fact of the water of crystallization, rendering oxalate of lime dimorphous, the Whewellite of Brooke being oblique.

It is curious that the specific gravity of Conistonite should be greater than Whewellite; we should have expected that the additional HO would have lowered the gravity."

Art. XXXVI.—*Introductory Essay, in Dr. Hooker's Flora of New Zealand.*

(Concluded from p. 252.)

Hybridization has been supposed by many to be an important element in confusing and masking species. Botanists of one class are apt to refer to its agency the unwelcome appearance of a specimen which combines two nominal species, founded on inconstant characters. Another class of naturalists appeal to the repeated occurrence of fertile hybrids, to negative the inference, otherwise unavoidable, that the production between two individuals of inherently fertile offspring, is a good reason for pronouncing them identical in species. Any continued effects from hybridization in uncontrolled nature seem to be thoroughly guarded against in two ways; first by the constitutional debility, if not by the invariable sterility of the hybrid offspring, rendering it of transient duration; and secondly, by the fact that, when prolific at all, they usually become so through fertilization by one or the

other of the parents, when the offspring reverts to that specific type. Dr. Hooker justly remarks, that,

"As a general rule, the genera most easily hybridized in gardens are not those in which the species present the greatest difficulties. With regard to the facility with which hybrids are produced, the prevalent ideas on the subject are extremely erroneous. Gærtner, the most recent and careful experimenter, who appears to have pursued his inquiries in a truly philosophical spirit, says that 10,000 experiments upon 700 species produced only 250 true hybrids. It would have been most interesting had he added how many of them produced seeds, and how many of the latter were fertile, and for how many generations they were propagated. The most satisfactory proof we can adduce, of hybridization being powerless as an agent in producing species (however much it may combine them), are the facts that no hybrid has ever afforded a character foreign to that of its parents, and that hybrids are generally constitutionally weak, and almost invariably barren. Unisexual trees must offer many facilities for the natural production of hybrids, which nevertheless have never been proved to occur; nor are such trees more variable than hermaphrodite ones."

May we not even say that they are less variable *because* unisexual, since their progeny is most likely to be originated from the conjunction of different trees, and individual peculiarities must needs be thereby blended and obliterated. When we consider how often it must happen that the ovules of one tree in an oak or pine forest are fertilized by the pollen of another; when we take into view the great number of unisexual plants, and consider also how many apparently hermaphrodite blossoms (far more than is generally supposed) are really submonœcious or polygamous, we may find reason to suspect in this a more general provision of nature for limiting congenital or induced individual forms than would at first appear. That some such controlling or amalgamating agencies operate in nature may be inferred from a comparison of the general homogeneity of an indigenous species, over even a large area, with the ready development of marked varieties or races in the case of every cultivated plant which is multiplied from seed, and their perpetuation from generation to generation, which is almost always ensured merely by cultivating each stock by itself. For the general law of nature is, not merely that *species* are true to their characters, these characters being rightly apprehended, but that the *individual* progeny inherits and transmits the special peculiarities of the parent or parents. Hence, that an isolated race retains certain characteristics so long as kept separate is no proof that it constitutes a species. Many a variety of recent and known origin does this. But, on the other hand, whatever individuals, however distinguished by minor differences when separate, are found to blend into a fertile race when associated, must on sound principles be regarded as belonging to one species. Perhaps if zoologists would contemplate the wide varia-

tions presented by many plants of indubitably one and the same species, and the still wider diversities of long cultivated races from an original stock, they would find more than one instructive parallel to the case of the longest-domesticated of all species, *man.*

Let it also be especially noted, that varieties are not always, not even generally, the result of external agencies, at least of such as we are able to detect. Certain varieties of plants are so originated: these are generally as transient as the cause that produces them, and under altered circumstances often disappear even during the life of the individual: the plant may outgrow them. But others arise, we know not how. It is past the wit of man to trace a connection between the diverse forms of the foliage, &c. of a polymorphous plant such as a *Coprosma*, *Metrosideros* or *Alseuosmia* in New Zealand, and any difference of circumstances attributable to station; still more so when these diversities occur side by side. Yet such are the varieties which ordinarily exhibit the greatest persistency, i. e. when kept from intermingling by mutual fecundation. What external circumstances can in the least account for the origin of the race of Dorking fowls, or Manx cats, or indeed of almost any of our domesticated races which were not produced by cross-breeding, that is by mingling the characters of two such races already in existence. Yet how certainly may we continue any of these races, under favorable circumstances, merely by maintaining the needful isolation; and how soon would they mingle and disappear if they were left to themselves. Is not this equally true of the human races?

From such facts, from the study of species remarkable for their polymorphous vegetation, as well as from the wide differences between certain domesticated races and their original stock,—differences not to have been expected on any known principles, and often far greater than those which are reckoned satisfactorily to mark the limits between other species,—the systematic botanist, at least, learns the lesson, that the amount of variation of which any one specific type is susceptible is only to be ascertained from observation and experience.

Upon the third proposition, that "Species are more widely diffused than is usually supposed," our author's statements are brief, but very decided and remarkable. He has already declared in the previous section "that it is by far the smaller half of the vegetable kingdom that is confined to narrow geographical or climatic areas, and that very few plants indeed are absolutely local; whilst the operations of the gardener and agriculturist prove that a vast proportion of the plants of the two temperate zones are capable of growing in any moderate climate." "I do not think," he continues, "that those who argue for narrow limits to the distribution and variation of species, can have considered a garden in a philosophical spirit, or have weighed such facts as

that there have been cultivated, within the last seventy years in the open air of England (at Kew), upwards of 20,000 species of plants from all quarters of the globe, and this within a space that, had it been left to nature, would not have contained 200 indigenous species. The fact that an overwhelming proportion of these have come up true to their parent, and have continued so under every possible disadvantage of transportation and transplantation, of altered seasons, and amount and distribution of temperature and humidity, of unsuitable soil and exposure, and of the multitude of errors which unavoidable ignorance of their natural locality and habit engenders: such appears to me the most forcible argument in favor of the power of plants to retain their original characters under altered circumstances."

The validity of the last inference is unquestionable and valuable. As long as the plants survived, they doubtless retained their characters. But the value of the general statement, as respects its bearing upon the natural diffusion of species, depends much on the answer to the question, how long did the majority of the species survive. We should like to know how large a proportion continued beyond the first two or three years, even with all the advantages of being looked after by the gardener.

But, returning to the general question, we remark, that there is little or no reason to expect a similarity between plants and animals in this respect. From the nature of the case we should suppose that the latter, at least that land-animals, would be much more local than plants.

We give to our readers the whole of the third section; as some of the statements, especially those that relate to the probable number of vegetable species known, are likely to excite some surprise.

"This is a point upon which my own views differ materially from those of many of my fellow botanists, and which, if borne out by facts, leads to a widely different estimate of the number and variety of the members of the vegetable kingdom than that which is at present entertained. As with the affinities and variation of species, so is it with their distribution: an extensive knowledge of the subject is only to be obtained by actual observation over large areas, and many of them, or by the study and comparison of the contents of many museums. It has been my singular good fortune to have visited many regions of the globe, and to have entered into some details upon the dispersion of living species, which has always been a favorite pursuit of mine. I have further had the advantage of collating my results with the largest and best-named botanical collections in the world, and have received a greater amount of assistance from my fellow naturalists than has fallen to the lot of most: facts which in ordinary cases are the result of long study and much consultation have been placed at my disposal rather than worked out by myself. A very extended examination of these materials has only tended to confirm the view which originated in my

personal experience, viz., that the estimate of the number of species known to botanists is a greatly exaggerated one,* and the prevalent ideas regarding their distribution no less contracted.

Many more plants are common to most countries than is supposed; I have found 60 New Zealand Flowering plants and 9 Ferns to be European ones, besides inhabiting various intermediate countries; and amongst the lower Orders we find a greatly increased proportion of species common to all countries: thus of Mosses alone 50 are found in New Zealand and Europe;† of Hepaticæ 13; of Algæ 45 are also natives of European seas; of Fungi nearly 60; and of Lichens 100.

So long ago as 1814 Mr. Brown‡ drew attention to the importance of such considerations, and gave a list of 150 European plants common to Australia. The identity of many of these has repeatedly been called in question, but almost invariably erroneously; added to which more modern collectors have greatly increased the list.

The too prevalent idea that the plants of newly discovered, isolated, or little visited localities must necessarily be new, has been a fertile source of the undue multiplication of species. There are very many cases of naturalists having been so impressed with this idea, that they have not thought it worth while to consult either books or herbaria before describing the plants from such spots. The New Zealand Flora presents several instances of this; two conspicuous ones occur in the genus *Oxalis;* one, *O. corniculata*, is amongst the most widely diffused and variable plants in the world; of its varieties no less than seven or eight species have been made, most of them supposed to be peculiar to New Zealand; not only is *O. corniculata* hence excluded from the flora, but, in the descriptions of these its varieties, no allusion is made to that plant.§ In the case of the other species the error is more ex-

* According to the loose estimate of compilers, 100,000 is the commonly received number of known plants: from a multiplicity of data I can come to no other conclusion but that half that number is much nearer the truth. This may well be conceived, when it is notorious that nineteen species have been made of the common Potato, and many more of *Solanum nigrum* alone. *Pteris aquilina* has given rise to numerous book species; *Vernonia cinerea* of India to fifteen at least. Many of the commonest European plants have several names in Europe, others in India, and still others in America, besides a host of garden names for themselves, their hybrids and varieties, all of which are catalogued as species in the ordinary works of reference whence such estimates are compiled.

† In fact the distribution of some Cryptogams is so wide, that I have visited a spot in a high southern latitude, nearly all whose plants are not only identical with those of Great Britain, but inhabit many intermediate temperate and tropical countries. Cockburn Island, in lat. 64° 12′ S. and long. 64° 49′ W., nearly fulfills this condition; I thereon collected nineteen plants, of which three-fourths are natives of England.

‡ Appendix to Flinders's Voyage, vol. ii, p. 592.

§ I have stated very confidently in the body of this work that eight of Cunningham's and Richard's species of this genus are all referable to one. This view will probably not meet the approbation of the local botanists, who will point to the constancy with which some of the states retain their characters under varied conditions. I value such facts very highly, and attach great weight to them, and did these varieties occur only in New Zealand I should perhaps have withheld so strong an opinion on the subject; but such is not the case. *O. corniculata* varies as much in numerous other parts of the world; and admitting, as every one must, that varieties are known to retain their characters with more or less constancy for certain periods, some other evidence is necessary to shake the opinion of the botanist who grounds his views on an examination of the plant from all quarters of the globe.

cusable, and may be still open to question;* it is that of *O. Magellanica*, originally discovered in Fuegia, and imperfectly described by Forster, whose very indifferent specimens of it are in the British Museum. When re-found in New Zealand it was described as new, and called *O. cataractæ*, and when found a third time in Tasmania, was called by still a third name, *O. lactea*. In this case a more important fact was smothered than that of the distribution of *O. corniculata*, namely, that of a very peculiar plant of the south temperate zone being common to these three widely sundered localities.

Many similar instances might be added, for there are several New Zealand plants (as *Pteris aquilina*) that have a different name in almost every country in the world: and, partly from changes in nomenclature, partly from the reduction of species, I have found myself obliged to quote 1500 names for the 720 New Zealand Flowering plants described; and I believe I might have doubled the number had my limits not obliged me to reduce the synonymy as much as possible; in many cases too much, I fear, for the requirements of working botanists in Europe."

One thing is clear, and important to be enforced: namely, that if determinations of species are to be of any value, especially in their bearing on general questions, they must rest solely upon observed characters of admitted value, irrespective of all theory. We pronounce such and such individuals, from a certain habitat, to belong to a distinct species, only because we find them possessed of certain adequate distinctive marks. If we at length ascertain that particular species are peculiar to particular stations or parts of the world, we have a sound and valuable deduction. But to assume that certain plants, or certain animals, from widely sundered localities belong to different species, notwithstanding their resemblance, until the contrary is proved, and even to announce this as a principle, for general adoption, as has been done, is surely a gross instance of reasoning in a vicious circle.

We cannot venture to condense, and therefore reproduce the whole of § 4, viz.:

" *The distribution of species has been effected by natural causes, but these are not necessarily the same as those to which they are now exposed.*

Of all the branches of Botany there is none whose elucidation demands so much preparatory study, or so extensive an acquaintance with plants and their affinities, as that of their geographical distribution. Nothing is easier than to explain away all obscure phenomena of dispersion by several speculations on the origin of species, so plausible that the superficial naturalist may accept any of them; and to test their soundness demands a comprehensive knowledge of facts, which more-

* As no identification is proved till all the organs of the plants to be compared have been studied, there is yet a possibility of these three species proving distinct, but I do not at all expect it; the only difference I can find is a greater obliquity and emargination of the petals of the New Zealand species, but that character varies so much both in this plant and in others of the genus that it loses all specific value.

over run great risk of distortion in the hands of those who do not know the value of the evidence they afford. I have endeavored to enumerate the principal facts that appear to militate against the probability of the same species having originated in more places (or centres) than one; but in so doing I have only partially met the strongest argument of all in favor of a plurality of centres, viz., the difficulty of otherwise accounting for the presence in two widely sundered localities of rare local species, whose seeds cannot have been transported from one to the other by natural causes now in operation. To take an instance: how does it happen that *Edwardsia grandiflora* inhabits both New Zealand and South America? or *Oxalis Magellanica* both these localities and Tasmania? The idea of transportation by aerial or oceanic currents cannot be entertained, as the seeds of neither could stand exposure to the salt water, and they are too heavy to be borne in the air. Were these the only plants common to these widely-sundered localities, the possibility of some exceptional mode of transport might be admitted by those disinclined to receive the doctrine of double centres; but the elucidation of the New Zealand Flora has brought up many similar instances equally difficult to account for, and has developed innumerable collateral phenomena of equal importance, though not of so evident appreciation. These, which all bear upon the same point, may be arranged as follows:—

1. Seventy-seven plants are common to the three great south temperate masses of land, Tasmania, New Zealand, and South America.

2. Comparatively few of these are universally distributed species, the greater part being peculiar to the south temperate zone.

3. There are upwards of 100 genera, subgenera, or other well-marked groups of plants entirely or nearly confined to New Zealand, Australia, and extra-tropical South America. These are represented by one or more species in two or more of these countries, and they thus effect a botanical relationship or affinity between them all, which every botanist appreciates.

4. These three peculiarities are shared by all the islands in the south temperate zone (including even Tristan d'Acunha, though placed so close to Africa), between which islands the transportation of seeds is even more unlikely than between the larger masses of land.

5. The plants of the Antarctic islands which are equally natives of New Zealand, Tasmania, and Australia, are almost invariably found only on the lofty mountains of these countries.

Now as not only individual species, but groups of these, whether orders, genera, or their subdivisions, are to a great degree distributed within certain limits or areas, it follows that the flora of every island or archipelago presents peculiarities of its own. Though an insular climate may favor the relative abundance of individuals, and even species of certain Natural Orders, there is nothing in the climate, or in any other attribute of insularity, which indicates the nature of the peculiarity of endemic species. The islands of each ocean contain certain botanically allied forms in common, which are more or less abundant in them, and rarely or never found on the neighboring continents; thus there are curious genera peculiar to the North Atlantic islands, others to the North Pacific islands, others to those of the South Pacific and others again to the Malayan Archipelago; just as there are still

others peculiar to the Antarctic islands; and many to New Zealand, Fuegia, and Tasmania.

Each group of islands hence forms a botanical region, more or less definable by its plants as well as by its oceanic boundaries; precisely as a continuous area like Australia or South Africa does. There is however this difference, that whereas the Natural Orders that give a botanical character to a continuous area of a continent or to a large island (as the *Proteaceæ* in South Africa or in New Holland, and *Coprosma* in New Zealand) are numerous in species and often uniformly spread,—in clusters of small islands, distant from continents, they are few in species, and the individuals are scattered, appearing like the vestiges of a flora which belonged to another epoch, and which is passing away. This is perhaps a fanciful idea, but one which I believe to contain the germ of truth; for no Botanist can reflect upon the destruction of peculiar species on small islands (such as is now going on in St. Helena amongst others), without feeling that, as each disappears, a gap remains, which may never be botanically refilled; that not only are those links breaking by which he connects the present flora with the past, but also those by which he binds the different members of the vegetable kingdom one to another. It is not true in every sense that all existing nature appears to the naturalist as an harmonious whole; each species combines by its own peculiarities two or more others more closely, and reveals their affinities more clearly, than any other does; just as the flora of an intermediate spot of land connects those of two adjacent areas better than any other locality does. It is often by one or a very few species that two large Natural Orders are seen to be related; just as by a few Chilian plants the whole flora of New Zealand is connected with that of South America. The destruction of a species must hence create an hiatus in our systems, and I believe that it is mainly through such losses that natural orders, genera, and species become isolated, that is, peculiar, in a naturalist's eyes.

To return to the distribution of existing species, I cannot think that those who, arguing for unlimited powers of migration in plants, think existing means ample for ubiquitous dispersion, sufficiently appreciate the difficulties in the way of the necessary transport. During my voyages amongst the Antarctic islands, I was led, by the constant recurrence of familiar plants in the most inaccessible spots, to reflect much on the subject of their possible transport; and the conviction was soon forced upon me, that, putting aside the almost insuperable obstacles to trans-oceanic migration between such islands as Fuegia and Kerguelen's Land, for instance (which have plants in common, not found elsewhere), there were such peculiarities in the plants so circumstanced, as rendered many of them the least likely of all to have availed themselves of what possible chances of transport there may have been. As species they were either not so abundant in individuals, or not prolific enough to have been the first to offer themselves for chance transport, or their seeds presented no facilities for migration,* or were singularly perishable from feeble vitality, soft or brittle integuments, the presence of oil that soon became rancid, or from having a fleshy albumen that

* Thus of the *Compositæ*, common to Lord Auckland's Group, Fuegia, and Kerguelen's Land, none have any pappus (or seed-down) at all! Of the many species *with* pappus, none are common to two of these islands!

quickly decayed.* Added to the fact that of all the plants in the respective floras of the Antarctic islands those common to any two of them were the most unlikely of all to emigrate, and that there were plenty of species possessing unusual facilities, which had not availed themselves of them, there was another important point, namely, the little chance there was of the seeds growing at all, after transport. Though thousands of seeds are annually shed in those bleak regions, few indeed vegetate, and of these fewer still arrive at maturity. There is no annual plant in Kerguelen's Land, and seedlings are extremely rare there; the seeds, if not eaten by birds, either rot on the ground or are washed away; and the conclusion is evident, that if such mortality attends them in their own island, the chances must be small indeed for a solitary individual, after being transported perhaps thousands of miles, to some spot where the available soil is pre-occupied.

Beyond the bare fact of the difficulty of accounting by any other means for the presence of the same species in two of the islands, there appeared nothing in the botany of the Antarctic regions to support or even to favor the assumption of the double creation, and I hence dismissed it as a mere speculation which, till it gained some support on philosophical principles, could only be regarded as shelving a difficulty; whilst the unstable doctrine, that would account for the creation of each species on each island by progressive development on the spot, was contradicted by every fact.

It was with these conclusions before me, that I was led to speculate on the possibility of the plants of the Southern Ocean being the remains of a flora that had once spread over a larger and more continuous tract of land than now exists in that ocean; and that the peculiar Antarctic genera and species may be the vestiges of a flora characterized by the predominance of plants which are now scattered throughout the southern islands. An allusion to these speculations was made in the 'Flora Antarctica' (pp. 210 and 368), where some circumstances connected with the distribution of the Antarctic islands were dwelt upon, and their resemblance to the summits of a submerged mountain chain was pointed out; but beyond the facts that the general features of the flora favored such a view, that the difficulties in the way of transport appeared to admit of no other solution, and that there are no limits assignable to the age of the species that would make their creation posterior to such a series of geological changes as should remove the intervening land, there was nothing in the shape of evidence by which my speculation could be supported. I am indebted to the invaluable labors of Lyell and Darwin,† for the facts that could alone have given

* Of the seeds sent to England from the Antarctic regions, or transported by myself between the several islands, almost all perished during transmission.

† See Darwin's 'Journal of a Naturalist,' and 'Essays on Volcanic Islands and Coral Islands.' The proofs of the coasts of Chili and Patagonia having been raised continuously, for several hundred miles, to elevations varying between 400 and 1300 feet, since the period of the creation of existing shells, will be found in the first-named of these admirable works, which should be in the hands of every New Zealand Naturalist, if only from its containing important observations on his own islands. The fact of this accomplished Naturalist and Geologist having preceded me in the investigation of the Natural History of the Southern Ocean, has materially influenced and greatly furthered my progress; and I feel it the more necessary to

countenance to such an hypothesis; the one showing that the necessary time and elevations and depressions of land need not be denied; and the other, that such risings and sinkings are in active progress over large portions of the continents and islands of the southern hemisphere. It is to the works of Lyell* that I must refer for all the necessary data as to the influence of climate in directing the migration of plants and animals, and for the evidence of the changes of climate being dependent on geological change. In the 'Principles of Geology' these laws are proved to be of universal application, and amply illustrated by their being applied to the elucidation of difficult problems in geographical distribution. It follows from what is there shown, that a change in the relative positions of sea and land has occurred to such an extent, since the creation of still existing species, that we have no right to assume that the plants and animals of two given areas, however isolated by ocean, may not have migrated over pre-existing land between them. This was illustrated by an examination of the natural history of Sicily (where land-shells, still existing in Italy, and which could not have crossed the Straits of Messina, are found imbedded on the flanks of Etna high above the sea-level), regarding which Sir Charles Lyell states that most of the plants and animals of that island are older than the mountains, plains, and rivers they now inhabit.†

It was reserved for Professor Edward Forbes, one of the most accomplished naturalists of his day, to extend and enlarge these views, and to illustrate by their means the natural history of an extensive area; which he did by applying a profound knowledge of geology and natural history to the materials he had collected during his arduous surveys of many of the shores of Europe and the Mediterranean. The result has been the enunciation of a theory, from which it follows that the greater part, if not all, of the animals and plants of the British Islands have immigrated at different periods, under very different climatic conditions; and that all have survived immense changes in the

mention this here, because Mr. Darwin not only directed my earliest studies in the subjects of the distribution and variation of species, but has discussed with me all the arguments, and drawn my attention to many of the facts which I have endeavored to illustrate in this Essay. I know of no other way in which I can acknowledge the extent of my obligation to him, than by adding that I should never have taken up the subject in its present form, but for the advantages I have derived from his friendship and encouragement.

* To Sir Charles Lyell's works, indeed, I am indebted for the enunciation of those principles that are essential to the progress of every naturalist and geologist; those, I mean, that affect the creation and extinction, dispersion and subsequent isolation of organic beings; and though botanists still differ in opinion as to the views he entertains on the most speculative of subjects (the origin and permanence of species), there is, I think, but one as to the soundness and originality of his observations on all that relates to the strict dependence of organic beings on physical conditions in the state of the earth's surface. I feel that I cannot over-estimate the labors of this great philosopher, when I reflect that without them the science of geographical distribution would have been with me little beyond a tabulation of important facts; and that I am indebted to them, not only for having given a direction to my studies in this department, but for an example of admirable reasoning on the facts he has collected regarding the distribution of plants and animals. I have no hesitation in recommending the 'Principles of Geology' to the New Zealand student of Nature, as the most important work he can study.

† See the Principles of Geology, ed. 9, p. 702, and Address to the Geological Society of London by the President (Leonard Horner, Esq.), in 1847, p. 66.

configuration of the land and seas of Northern Europe. The arguments which support this theory are based upon evidence derived from Zoology and Geology,* and they receive additional weight from the fact that the distribution of British plants is in accordance with its principal features.†

The geographical distribution of British plants has been the subject of the most rigorous investigation by one of our ablest British botanists, Mr. H. C. Watson, who first drew attention to the various botanical elements of which the flora is composed, and grouped the species into botanical provinces. These provinces were intended for 'showing the areas of plants, as facts in nature independent of all theoretical explanations and reasons.' (Cybele Britannica, vol. i, p. 18). An inspection of them shows the relations borne by the plants of England to those of certain parts of Europe and of the Arctic regions; and Professor Forbes, applying a modification of these botanical provinces to the illustration of his views of the original introduction of plants into the British Islands, proceeds to show that their migration took place at different periods, contemporary of course with the connection by land of each botanical region of Britain with that part of the continent which presents a similar association of plants.

To extend a theoretical application of these views to the New Zealand Flora, it is necessary to assume that there was at one time a land communication by which the Chilian plants were interchanged; that at the same or another epoch the Australian, at a third the Antarctic, and at a fourth the Pacific floras were added to the assemblage. It is not necessary to suppose that for this interchange there was a continuous connection between any two of these localities, for an intermediate land, peopled with some or all of the plants common to both, may have existed between New Zealand and Chili when neither of these countries was as yet above water.‡ To account, however, for the Antarctic

* For the contents of the Essay itself, I must refer to the Records of the Geological Survey of Great Britain, vol. i, p. 336. This is the most original and able essay that has ever appeared on this subject, and though I cannot subscribe to all its botanical details, I consider that the mode of reasoning adopted is sound, and of universal application. What I dissent from most strongly is, the origin of the gulf-weed, the peopling of Scotch mountains by iceberg transport of seeds, and the too great stress laid upon the west Irish flora, whose peculiarities appear to me to be considerably over-estimated.

† It may be well to state to the New Zealand student, that there are no reasons to suppose that Botany can ever be expected to give that direct proof of plants having survived geological changes of climate, sea, and land, which animals do; the cause is evident, for the bones of quadrupeds, shells of mollusca, and hard parts of many animals, afford an abundant means of specific identification, and such are preserved when the animals perish. In plants the case is widely different: their perishable organs of reproduction, which alone are available for systematic purposes, are seldom imbedded, even when other parts of the plants are.

‡ This disappearance of old land, and the migration of its flora and fauna to new, may be illustrated to a certain extent by the delta of any New Zealand river. A mud-bank on one shore, covered with mangroves, advances across the channel, the mangroves growing on the new land as it forms. The current changes, and the end of the bank (with its mangroves) is cut off, and becomes an island: another change of the river channel fills up that between the islet and the opposite shore, to which it hence becomes a peninsula, peopled by mangroves, whose parents grew on the opposite bank. Here, be it remarked, no subsidence is required, such as must have operated in the assumed isolation of New Zealand.

plants on the lofty mountains, a new set of influences is demanded; no land connection between these islands and New Zealand could have effected this, for the climate of the intermediate area must necessarily have prevented it. But changes of relation between sea and land induce changes of climate, and the presence of a large continent connecting the Antarctic islands would, under certain circumstances render New Zealand as cold as Britain was during the glacial epoch. Sir C. Lyell first demonstrated this, and showed what such conditions should be; and by consulting the 'Principles of Geology,' my reader will understand how such a climate would reign in the latitude of New Zealand, as that its flora should consist of what are now Antarctic forms of vegetation. The retirement of the plants to the summit of the New Zealand mountains,* would be the necessary consequence of the amelioration of climate that followed the isolation of New Zealand, and the replacement of the Antarctic continent by the present ocean.

The climate throughout the south temperate zone is so equable, and the isothermal lines are so parallel to those of latitude, that it is not easy for the New Zealand naturalist to realize the altered circumstances that would render the plains of his island suitable for the growth of plants that now inhabit its mountains only;† but if he glance at the map of the isothermal lines of the northern hemisphere, he will see how varied are the climates of regions in the same latitude; that London, with a mean temperature of 51°, is in the same latitude as Hudson's Bay, where the mean temperature is 30°, and the soil ever frozen: and he will further be able to understand by a little reflection, how a change in the relative positions of sea and land would, by isolating Labrador, raise its temperature 10°–15°, causing the destruction of all the native plants that did not retire to its mountain-tops, and favoring the immigration of the species of a more genial climate.

The first inference from such an hypothesis is that the Alpine plants of New Zealand, having survived the greatest changes, are its most ancient colonists; and it is a most important one in many respects, but especially when considered with reference to the mountain floras of the

* With regard to the British mountains, Professor Forbes imagines that they were islets in the glacial ocean, and received their plants by transportation of seeds with soil, on ice from the Arctic regions. This appears to me to want support, and there is much in the distribution of Arctic plants especially, wholly opposed to the idea of ice transport being an active agent in dispersion. A lowering of 10° of mean temperature would render the greater part of Britain suitable to the growth of Arctic plants; it would give it the climate of Labrador, situated in the same latitude on the opposite side of the Atlantic. Britain is the warmest spot in its latitude, and a very slight geological change would lower its mean temperature many degrees.

† The New Zealand naturalist has probably a very simple means of determining for himself whether his island has been subject to a geologically recent amelioration of climate; to do which, let him examine the fiord-like bays of the west coast of the Middle Island, for evidence of the glaciers which there exist in the mountains having formerly descended lower than they now do. Glaciers to this day descend to the level of the sea in South Chili, at the latitude of Dusky Bay; and if they have done so in the latter locality, they will have left memorials, in the shape of boulders, moraines, and scratched and polished rocks.

Pacific and southern hemisphere generally. These may be classed under three heads:*—

1. Those that contain identical or representative species of the Antarctic Flora, and none that are peculiarly Arctic; as the Tasmanian and New Zealand Alps.†

2. Those that contain, besides these, peculiarities of the Northern and Arctic Floras;‡ as the South American Alps.

3. Those that contain the peculiarities of neither; as the mountains of South Africa and the Pacific Islands.

We thus observe that the want of an Arctic or Antarctic Flora at all in the Pacific islands, and the presence of an Arctic one in the American Alps, are the prominent features; and I shall confine my remarks upon these to the fact that, with regard to the isolated islands of the Pacific, they are situated in too warm a latitude to have had their temperature cooled by changes in the relative position of land and ocean, so as to have harbored an Antarctic vegetation. With regard to the South American Alps, there is direct land communication along the Andes from Arctic to Antarctic regions; by which not only may the strictly Arctic genera and species have migrated to Cape Horn, but by which many Antarctic ones may have advanced northward to the equator.§

There is still another point in connection with the subject of the relative antiquity of plants, and in adducing it I must again refer to the 'Principles of Geology,' where it is said, 'As a general rule, species common to many distant provinces, or those now found to inhabit many distant parts of the globe, are to be regarded as the most ancient .... their wide diffusion shows that they have had a long time to spread themselves, and have been able to survive many important changes in Physical Geography.'‖ If this be true, it follows that, consistently with the theory of the antiquity of the Alpine flora of New Zealand, we should find amongst the plants common to New Zealand and the Antarctic islands, some of the most cosmopolitan; and we do so in *Montia*

* I need scarcely remind my reader that in thus sketching the characteristics of these Alpine floras, I make no allusion to exceptions that do not alter the main features. I am far from asserting that there are no peculiar Arctic or Antarctic forms in the Pacific Islands, nor any peculiarly Arctic ones in Tasmania and New Zealand: but if, on the one hand, future discoveries of such shall weaken the points of difference between these three mountain regions, on the other they might be very much strengthened by adducing the number of Arctic species common to the South American Alps, but not found in the others.

† These Antarctic forms are very numerous; familiar ones are *Acæna, Drapetes, Donatia, Gunnera, Oreomyrrhis, Lagenophora, Förstera, Ourisia, Fagus, Callixene, Astelia, Gaimardia, Alepyrum, Oreobolus, Carpha, Uncinia.*

‡ *Berberis, Sisymbrium, Thlaspi, Arabis, Draba, Sagina, Lychnis, Cerastium, Fragaria, Lathyrus, Vicia, Hippuris, Chrysoplenium, Ribes, Saxifraga, Valeriana, Aster, Hieracium, Stachys, Primula, Anagallis, Pinguicula, Statice, Empetrum, Phleum, Elymus, Hordeum.*

§ Why these Antarctic forms have not extended into North America, as the Arctic ones have into South America, is a curious problem, and the only hypothesis that suggests itself is derived from the fact that though the Panama Andes are not now sufficiently lofty for the transit of either, there is nothing to contradict the supposition that they may have had sufficient altitude at a former period, and that one which preceded the advance of the Antarctic species to so high a northern latitude.

‖ Principles of Geology, ed. 9, p. 702.

fontana, *Callitriche verna*, *Cardamine hirsuta*, *Epilobium tetragonum*, and many others.

On the other hand, it must be recollected that there are other causes besides antiquity and facility for migration, that determine the distribution of plants; these are their power, mentioned above, of invading and effecting a settlement in a country preoccupied with its own species, and their adaptability to various climates: with regard to the first of these points, it is of more importance than is generally assumed, and I have alluded to its effects under *Sonchus*, in the body of this work. As regards climates, the plants mentioned above seem wonderfully indifferent to its effects.*

Again, even though we may safely pronounce most species of ubiquitous plants to have outlived many geological changes, we may not reverse the position, and assume local species to be amongst the most recently created; for whether (as has been conjectured) species, like individuals, die out in the course of time, following some inscrutable law whose operations we have not yet traced, or whether (as in some instances we know to be the case) they are destroyed by natural causes (geological or others), they must in either case become scarce and local while they are in process of disappearance.

In the above speculative review of some of the causes which appear to affect the life and range of species in the vegetable kingdom, I have not touched upon one point, namely, that which concerns the original introduction of existing species of plants upon the earth. I have assumed that they have existed for ages in the forms they now retain, that assumption agreeing, in my opinion, with the facts elicited by a survey of all the phenomena they present, and, according to the most eminent zoologists, with those laws that govern animal life also; but there is nothing in what is assumed above, in favor of the antiquity of species and their wide distribution, that is inconsistent with any theory of their origin that the speculator may adopt. My object has not so much been to ascertain what may, or may not, have been the original condition of species, as to show that, granting more scope for variation than is generally allowed, still there are no unassailable grounds for concluding that they now vary so as to obliterate specific character; in other words, I have endeavored to show that they are, for all practical purposes of progress in botanical science, to be regarded as permanently distinct creations, which have survived great geological changes, and which will either die out, or be destroyed, with their distinctive marks unchanged. We have direct evidence of the impoverishment of the flora of the globe, in the extinction of many most peculiar insular species within the last century; but whether the balance of nature is kept up by the consequent increase of the remainder in individuals, or by the sudden creation of new ones, does not appear, nor have we any means of knowing: if the expression of an opinion be insisted on, I should be induced to follow the example of an eminent astronomer,

* Mr. Watson (Cybele Britannica) gives the range of *Callitriche* in Britain alone as including mean temperatures of 40° to 52°, and as ascending from the level of the sea to nearly 2000 feet in the East Highlands of Scotland. *Montia*, according to the same authority, enjoys a range of 36° to 52°, and ascends to 3300 feet; *Epilobium*, a temperature of 40° to 51°, and ascends to 2000 feet; *Cardamine*, a temperature of 37° to 52°, and ascends to 3000 feet.

who, when the question was put to him, as to whether the planets are inhabited, replied that the earth was so, and left his querist to argue from analogy. So with regard to species, we know that they perish suddenly or gradually, without varying into other forms to take their place as species, from which established premiss the speculator may draw his own conclusions. * * * * *

To those who may accuse me of giving way to hasty generalization or loose speculation on the antiquity and dispersion of plants over parts of the Southern Hemisphere, I may answer, that no speculation is idle or fruitless, that is not opposed to truth or to probability, and which, whilst it co-ordinates a body of well established facts, does so without violence to nature, and with a due regard to the possible results of future discoveries. I may add, that after twelve years' devotion to the laborious accumulation and arrangement of facts in the field and closet, untrammelled by any theories to combat or vindicate, I have thought that I might bring forward the conclusions to which my studies have led me, with less chance of incurring such a reproach, than those would, who with far better abilities and judgment, have not had my experience and opportunities."

The hypothesis that these insular floras are the remains of larger floras greatly reduced or verging to extinction, will be found remarkably applicable, if we mistake not to the case of the Sandwich Islands; many of the characteristic species of which appear to be represented by comparatively few individuals, as if waning to extinction along with the Hawaian race.

The general idea here so ably set forth by Dr. Hooker, in respect to the Antarctic and South Pacific floras, is a very important one, and worthy of the most extended and critical examination. Its establishment, moreover, by satisfactory evidence, would destroy one of the strongest grounds upon which the doctrine of the multiple origin of species (at least in the form maintained by Schouw) is supported. To complete the view, and fairly to exhibit the grounds upon which these theoretical conclusions are based, we should embrace in our extracts a large part of the remaining chapter; on the physiognomy and affinities of the New Zealand Flora, and on the variation of New Zealand species. But the necessary limits of our article forbid. A few facts regarding the more striking peculiarities of the New Zealand flora may be collected. The large proportion, both relatively and absolutely, of the Cryptogamia, and especially of Ferns, has already been noted. "A paucity of Grasses, and absence of Leguminosæ, and abundance of bushes and Ferns, and a want of annual plants, are the prevalent features of the open country; whilst the forests abound in Cryptogamia, in Phænogamic plants with obscure and green flowers, and very often of obscure and little known natural orders."

The number of natural orders of the Phænogamic plants is remarkably large in proportion to the genera, even for an insular flora; being 92 to 282, or about one to three: while the genera

are to the species as 282 to 730, each genus having on the average only 2½ species: so that the species average but eight for each order. This makes it one of the most difficult floras in the world for a beginner, who must know a natural order for every eight species. How recondite, vague, and unsatisfactory the natural system of Botany must appear to the New Zealand student!

Of the largest natural orders, as respects the number of species, the individuals are often so few that none of them form prominent features in the landscape. The *Coniferæ* prove to be the most prevalent family; but the majority of their species are not social but grow intermixed with the trees, so as to give no character to the landscape;—a case just opposite to what occurs in the northern hemisphere, at least in North America, where the species are few, but mostly social, and existing in a vast number of individuals, which often occupy considerable tracts almost exclusively, and thus strikingly impress their features upon the landscape.

The number of kinds of trees is very large in proportion to the herbaceous plants; as there are 113 Phænogamous trees, including shrubs above twenty feet high, or one-sixth of the flora, while in England there are not more than 35 native trees, in a much larger flora.

A remarkably large proportion of the Phænogamous flora of New Zealand consists of absolutely peculiar plants: of these there are 26 genera and 507 species; or more than two-thirds of the whole. The greater part of these are Exogens. Of the remaining third, consisting of plants common to New Zealand and other countries:

"193 species, or nearly one-fourth of the whole, are Australian.
89 species, or nearly one-eighth of the whole, are South American.
77 species, or nearly one-tenth of the whole, are common to both the above.
60 species, or nearly one-twelvth of the whole, are European.
50 species, or nearly one-sixteenth of the whole, are Antarctic Islands, Fuegian, &c."

These several elements in the New Zealand flora, whether as represented by identical or cognate species, are in turn subjected to a critical analysis. The following extract, respecting the Australian element, is interesting from its direct bearing on the question of transport by water.

"If the number of plants common to Australia and New Zealand is great, and quite unaccountable for by transport, the absence of certain very extensive groups of the former country is still more incompatible with the theory of extensive migration by oceanic or aerial currents. This absence is most conspicuous in the case of *Eucalypti*, and almost every other genus of *Myrtaceæ*, of the whole immense genus of *Acacia*,

and of its numerous Australian congeners, with the single exception of *Clianthus*, of which there are but two known species, one in Australia, and the other in New Zealand and Norfolk Island.

The rarity of *Proteaceæ*, *Rutaceæ*, and *Stylideæ*, and the absence of *Casuarina* and *Callitris*, of any *Goodeniæ* but *G. littoralis* (equally found in South America), of *Tremandreæ*, *Dilleniaceæ*, and of various genera of *Monocotyledones*, admit of no explanation consistent with migration over water having introduced more than a very few of the plants common to these tracts of land. Considering that *Eucalypti* form the most prevalent forest feature over the greater part of South and East Australia, rivalled by the *Leguminosæ* alone, and that both these Orders (the latter especially) are admirably adapted constitutionally for transport, and that the species are not particularly local or scarce, and grow well wherever sown, the fact of their absence from New Zealand cannot be too strongly pressed on the attention of the botanical geographer; for it is the main cause of the difference between the floras of these two great masses of land being much greater than that between any two equally large contiguous ones on the face of the globe. If no theory of transport will account for these facts, still less will any of variation; for of the three genera of *Leguminosæ* which do inhabit New Zealand, none favor such a theory; one, *Clianthus*, I have just mentioned; the second, *Edwardsia*, consists of one tree, identical with a Juan Fernandez and Chilian one, and unknown in New Holland; and the third genus (*Carmichælia*) is quite peculiar, and consists of a few species feebly allied to some New Holland plants, but exceedingly different in structure from any of that extensive Natural Order."

Dr. Hooker then appends a carefully prepared table of 228 phænogamous species which may be said to represent each other in two or all the three South temperate masses of land, viz., New Zealand (including Auckland and Campbell's Islands), Australia (including Tasmania), and extra-tropical South America, including the Falkland Islands; the list being confined to cases of real and usually very close botanical affinity, to the exclusion of analogical resemblances, however striking. The list is by no means overstrained, nor as full as it might be; since one or two more good instances have occurred to our memory while looking over its columns. On comparing together the Australian and New Zealand columns, we find only fourteen blanks, not filled by known representative species; a similar comparison of the New Zealand and South American columns shows forty-six blanks, sixteen of which are among the Endogens.

On fairly weighing all this testimony, the botanist will perhaps accede to our author's conclusion,—viz., that the floras of these three great areas of land in southern latitudes "exhibit a botanical relationship as strong as that which prevails throughout the lands within the Arctic northern temperate zones: and which is not to be accounted for by any theory of transport or of variation; but which is agreeable to the hypothesis of all being members of a once more extensive flora, which has been broken up by geological and climatic causes." A. G.

Art. XXXVII.—*Remarks on the Mineral species Algerite;* by T. S. Hunt.

In this Journal for July, 1849, I published a description by Mr. Alger, of a mineral from Franklin, N. J., together with an analysis of it by myself, from which I was led to consider it a new species, and to give it the name of Algerite. It was again analyzed by Mr. R. Crossley, whose results will be found in this Journal for July, 1850. The mineral had been described as having the form of an oblique rhombic prism, M : M = 94°, but Mr. Dana has satisfied himself that the crystal is really a square prism. The hardness is 3·0—3·5 (Alger, Crossley); density 2·697—2·712 (Hunt), 2·78 (Crossley). The mineral which is imbedded in a white coarse-grained crystalline limestone, is subject to decomposition in the weathered portions of the rock, but as I stated in my paper, "the crystals selected for analysis were hard, semi-translucent and undecomposed; their powder when elutriated and carefully dried, was of a buff color, which was not changed by ignition." Mr. Crossley remarks that in his case "many of the crystals were incrusted with idocrase, and in some instances penetrated so much that it required great caution to secure portions free from that mineral." He rejected all decomposed portions, and analyzed only pure honey-yellow fragments. The results of the two analyses are as follows:

	Hunt.	Crossley.
Silica,	49·82	49·96
Alumina,	24·91	24·41
Peroxyd of iron,	1·85	1·48
Magnesia,	1·15	5·18
Potash, Soda,	10·21	9·97
Carbonate of lime,	3·94	4·21
Water,	7·57	5·06
	99·45	100·27

A subsequent incomplete analysis gave me, Silica 49·42, alumina 25·67, peroxyd of iron 1·93, carbonate of lime 3·57, water 6·18, magnesia and alkalies by difference 13·23. My analysis differs from that of Mr. Crossley in the smaller amount of magnesia which is replaced by water, a case analogous to that of aspasiolite and iolite, or serpentine and chrysolite, which are found in the same localities and even in the same crystal. The question whether these differences are to be attributed to a subsequent alteration of the minerals, by removing magnesia and substituting water, or to the original association of homœomorphous, chemically homologous species, is as yet unsettled. The latter view is maintained by Scheerer, whose opinions will be found in this Journal [2] vols. v, p. 388, and vi, p. 199; and also cited with some further considerations to the same effect, in a paper by myself, published in the last volume of this Journal, of which see page 217.

From the square form of the prism Mr. Dana has suggested that Algerite may be a result of the alteration of scapolite from which species however it differs widely in composition; while the latter affords from 13 to 24 p. c. of lime, and from 4 to 7 of soda, Algerite contains neither lime nor soda, but 10 p. c. of potash, so that the change must have consisted in the removal of lime and soda, and the substitution of magnesia, water, and a large amount of potash, which reactions, especially the replacement of one alkali by another, seem very questionable. As scapolite has not yet been found with this mineral, it would have been better to regard algerite as derived from the alteration of the associated idocrase, a process which presents less difficulty to the chemist than the other. It may fairly be questioned whether the extreme views maintained by some, with regard to the alteration of mineral species, do not tend to discourage mineralogical chemistry, and to embarrass the science with groundless hypotheses.

In support of the view that Algerite is an altered scapolite, Mr. J. D. Whitney has given at page 296 of this volume, the results of his examination of the mineral. He observes of the material selected for analysis, that "after the ignition it was noticed that portions of the ignited mineral remained nearly unaltered in appearance, while the larger part acquired a brick-red color, and on examination with the microscope was seen to contain small silvery scales apparently of mica. As only a small quantity could be used for analysis, the results can be relied upon only as approximatively correct." p. 208. He gives the following as the result of his analysis: Silica 52·09, alumina 18·63, phosphate of lime 8·22, carbonate of lime 4·41, water 6·68, loss, potash and soda? 9·97=100·00.

Had the analysis of such a compound agreed with my own, I might have had reason to doubt the results of Mr. Crossley and myself. Since reading Mr. Whitney's remarks I have examined several crystals of Algerite for phosphate of lime, by digesting their powder with heated hydrochloric acid, and testing the solution with molybdate of ammonia; this delicate reagent indicated the presence of traces only of phosphoric acid. The apparently pure calcite in which the crystals are imbedded, gives a more decided reaction, and traces of this acid are seldom wanting even in feldspars and tourmalines. While, therefore, we do not question that Mr. Whitney's parti-colored mixture may have contained apatite as well as mica, we find in his confessedly approximate results, no reason for doubting that Algerite is a homogeneous mineral, or for supposing it to be produced by the alteration of scapolite. Further examinations of the mineral may throw more light upon the variations in the proportions of water and magnesia.

Montreal, C. E., March 7th, 1854.

ART. XXXVIII.—*Notice of a collection of Fishes from the southern bend of the Tennessee River, in the State of Alabama;* by L. AGASSIZ.

(Concluded from page 308.)

CYPRINODONTS, *Agass.*—Only two species of this family have thus far been discovered in the waters of the Tennessee River, and both of them have already been described by Dr. Storer under the names of *Pœcilia catenata* and *olivacea*, Synopsis, p. 178. Having made lately however, a thorough revision of the genera and species of this family found in the United States, I would remark that *Pœcilia catenata*, St., ought to be referred to the genus *Hydrargyra*, and that *Pœcilia olivacea* belongs to my newly established genus *Zygonectes*. These species ought therefore to stand in future in our systematic catalogues under the names of *Hydrargyra catenata*, and *Zygonectes olivaceus.**

CYPRINOIDS, *Cuv.*—This is one of the most interesting families of our freshwater fishes, both on account of the number of genera and species inhabiting our lakes and rivers, and of the diversity of their forms and habits.

CARPIODES, *Rafin.*—In the great French Ichthyology, Valenciennes has established a new genus under the name of *Sclerognathus*, for Lesueur's *Catostomus cyprinus*, and this genus has deservedly been acknowledged by subsequent writers. In con-

* The species of the genus Zygonectes may be arranged in two groups: 1, those in which there are several more or less distinctly dotted lines along the sides of the body, and in which a broad black band extends across the eye and cheek. To this group belong: *Z. Nottii*, Agass. The darker continuous longitudinal lines alternate with fainter interrupted ones. Males with distinct, transverse bands. Dark olive above, fading upon the sides, silvery below. Operculum, throat, and space in advance of the eye light orange color. Mobile, Alabama. Collected there with Dr. Nott. Mississippi: Col. Deas.—*Z. lineolatus*, Agass. Longitudinal lines broader and undulated or serrated, the transverse bands of the male very distinct and broader than the longitudinal ones. Olive colored, darker along the back and fading upon the sides, lower parts silvery Discovered by Dr. W. I Burnétt at Augusta, Ga.—*Z. guttatus*, Agass. A large dark spot upon the centre of each scale on the back and sides, forming longitudinal rows of disconnected dots. The transverse bars of the males are much narrower and nearer together than in Z. lineolatus. Dark olive above, fading upon the sides; abdomen silvery. Mobile, Alabama.—*Z. dispar*, Agass. Longitudinal lines of minute dots particularly distinct in the anterior part of the body, alternating backwards with continuous lines in the males, which are besides transversely barred, whilst the female has only continuous serrated lines upon the sides. Light olive above, silvery upon the sides and below. In small creeks near St. Louis, Mo., on the Illinois side of the Mississippi, and also in the Illinois River at Beardstown.—*Z. hieroglyphicus*, Agass. Anterior and upper part of the body irregularly sprinkled with dark spots, passing into longitudinal rows backwards. Light olive above, silvery upon the sides and below. Mobile, Alabama. 2. The second group includes species with one broad longitudinal black band extending from the tip of the lower jaw to the base of the tail, passing in a straight line through the eyes and along the sides of the body. To this group belongs the species mentioned above from the Tennessee River, and also *Z. lateralis*, Agass, which is a more elongated species from Mobile, Alabama; also dotted above the broad lateral band, and *Z. zonatus*, Agass., from St. Louis, Mo., which has no spots upon the sides of the back, and in which the outlines of the longitudinal band are serrated.

sidering this type of Cyprinoids as a distinct group among the Catostomi, Rafinesque has however the priority over the able professor of the Jardin des Plantes; for we find in his *Ichthyologia Ohiensis* that the third subgenus of Catostomus, which he calls *Carpiodes*, though not characterized with the precision with which Valenciennes has circumscribed his genus Sclerognathus, exactly corresponds to it. I do not hesitate therefore to adopt Rafinesque's name as the older; the more so, since this writer has at the same time wisely separated from the common Catostomi at that early day two other types of the same group, which are even now left among Catostomi by all ichthyologists. I allude to the subgenus *Ictiobus*, with *Catostomus Bubalus* as its type, and to the genus *Cycléptus* for the *Missouri sucker;* for though Rafinesque did not himself examine this latter fish, and ascribes to it two dorsals, it must be evident to any one who has had an opportunity of investigating this rare species that the few words with which it is mentioned apply to it, and that the indication of two dorsals is easily explained by the very form of that fin, the anterior part of which rises like a separate fin in advance of the following low part which extends uniformly far behind. I should add that *Catostomus elongatus* belongs also to this genus *Cycleptus*. As to *Ictiobus*, it resembles Carpiodes in external appearance, but is at once distinguished by its thin lips and more terminal mouth.* Nothing is to be more regretted for the progress of Natural History in this country, than that Rafinesque did not put up somewhere a collection of all the genera and species he has established, with well authenticated labels, or that his cotemporaries did not follow in his steps, or at least preserve the tradition of his doings, instead of decrying him and appealing to foreign authority against him. Tracing his course as a naturalist during his residence in this country, it is plain that he alarmed those with whom he had intercourse by his innovations and that they preferred to lean upon the authority of the great naturalists of the age then residing in Europe, who however knew little of the special Natural History of this country, than to trust the somewhat hasty man who was living among them, and who had collected a vast amount of information from all parts of the States, upon a variety of objects then entirely new to science. From what I can learn of Rafinesque, and from a careful study

* In connection with the genera mentioned above, I may remark here that Rafinesque has established another sub-genus under the name of *Moxostoma*, which fully deserves to be recognized as a distinct genus, as far as I am able to judge from the three species belonging to it, with which I am especially acquainted, which are *Catostomus anisurus* of the West, *C. gibbosus* or *tuberculatus* of the East, and *C. Suceti* of the South. After acknowledging these alterations of the genus Catostomus, as it is now generally understood by ichthyologists, there would still remain a group of species to constitute the genus Catostomus proper of which *C. hudsonius*, for which the name Catostomus was first proposed, may be considered as the type. Thus freed of all unjustifiable additions engrafted upon it in course of time, the genus Catostomus would be restored to its primitive natural circumscription.

of his works, I am satisfied that he was a better man than he appeared. His misfortune was his prurient desire for novelties, and his rashness in publishing them, and yet both in Europe and in America he has anticipated most of his cotemporaries in the discovery of new genera and species in those departments of science which he has cultivated most perseveringly, and it is but justice to restore them to him, whenever it can be done. Personal considerations should no longer be allowed to interfere with this late act of redress. May the example of Rafinesque not be lost for those naturalists in this country who describe new species without taking the least care to preserve the original specimens of their descriptions, or to circulate authentic ones among other naturalists.

Besides the well known type of the genus Carpiodes, the *C. Cyprinus*, and the other species described by Valenciennes and Rafinesque, I have ascertained the existence of five undescribed species, of which I give below short comparative descriptions. These species bear to one another similar relations as the species of Cyprinus described by Heckel; indeed they truly represent upon the Continent of North America the genus Cyprinus of the old world to which they bear the greatest resemblance in outward appearance, though they differ strikingly in their generic characters. I have applied to the new species here mentioned names reminding us of the common name of Buffalo applied to all of them throughout the country. The large number of specimens including all sizes, which I have been able to collect of some species of this genus, has enabled me to ascertain the range of variation in their characters.

1. *Carpiodes Urus*, Agass.—From the Tennessee River. It grows very large, weighing occasionally from 30 to 40 pounds. The body in this species is not so high as in C. Cyprinus, nor is it so compressed above; the scales are also not so high, but more angular behind, and the anterior portion of the dorsal is not so elongated. The gill cover is larger, and the distance from the hind border of the eye to the inferior angle of the subopercle, near the base of the pectorals, and the distance from the same point to the superior and posterior angle of the opercle, are nearly equal. In C. Cyprinus the distances differ by nearly one-third. The subopercle is not triangular, but its hind border is nearly regularly arched from the upper angle to the posterior angle of the interopercle. The anal has its posterior margin full, and not lunate; the caudal is not so deeply furcate as in C. Cyprinus. The ventrals do not reach the anal. All fins are of a dark color. I am indebted to Dr. Newman for this species.

2. *Carpiodes Taurus*, Agass.—From Mobile River, Alabama. The form of the body is intermediate between that of C. Cyprinus and C. Urus. The gill cover has the same form as in

C. Urus, but it is larger and more strongly arched behind. The hind margin of the scales is waving, owing to a somewhat prominent middle angle. The anterior rays of the dorsal equal in length two-thirds of that of the base of the fin. Anal not lunate behind. The ventrals do not reach to the anal opening. Caudal not so deeply furcate as in C. Cyprinus.

3. *Carpiodes Bison*, Agass.—From the Osage River, Missouri. This species is more elongated than C. Taurus. The head is smaller, the opercle also smaller, and the subopercle triangular. The dorsal has its anterior rays longer, hence its hinder border is more deeply emarginate. Anal more deeply lunate. Horizontal diameter of scales greater. I have received this species through the attention of Mr. George Stolley.

4. *Carpiodes Vitulus*, Agass.—From the Wabash River, Indiana. This seems to be a smaller species than the preceding ones. The form of the body resembles that of C. Taurus; but the eyes are smaller; the opercle is more broadly rounded behind; the subopercle has its posterior and free border regularly arched above and below, and not emarginate as in C. Taurus. The direction of the numerous water tubes on the head and cheeks also differ. The upper and lower borders of the scales are nearly straight. The dorsal does not extend quite so far forwards. I am indebted to Col. Richard Owen of New Harmony for this species.

5. *Carpiodes Vacca*, Agass.—From the Susquehannah River. This species resembles more closely C. Cyprinus than any other; the anterior rays of the dorsal are also very elongated, yet they do not reach beyond the base of the fin itself when bent backwards; the caudal is not so deeply furcate, and the scales have a greater horizontal diameter. I owe this species to the kindness of Professor S. S. Haldeman.

Catostomus, *Lesueur.*—The following species of this genus have been collected by Dr. Newman in the vicinity of Huntsville:

Catostomus communis, Lesueur.—Called Fine-scaled Sucker at Huntsville.

Catostomus nigricans, Lesueur.—Called Hog Sucker at Huntsville.

Catostomus Duquesnii, Lesueur.—Called May Sucker at Huntsville.

Catostomus melanops, Kirtl.—Also called May Sucker at Huntsville. This species agrees with Kirtland's description of C. melanops, except in having longer pectorals and in the reddish color along the sides. Rafinesque's description cannot apply to this fish. Having no specimens from the localities mentioned by Rafinesque and Kirtland, I do not venture to pursue further a comparison between these fishes.

Rhinichthys, *Agass.*—This genus was established by me in "Lake Superior," page 353. Several new species have been dis-

covered since by Prof. Baird and myself.* Dr. Newman has sent me another undescribed species, which I call

Rhinichthys obtusus, Agass.—Body cylindrical, slightly compressed, more blunt than in Rh. marmoratus. The mouth extends but little beyond the margin of the upper jaw; lower jaw strongly arched from side to side. Eyes rather large and nearer the end of the snout than the posterior angle of the opercle. Dorsal exactly intermediate between the ventrals and the anal, quadrangular, its last rays about two-thirds the length of the fish, so that when the fin is folded backwards their ends meet. Pectorals broadly rounded behind; do not reach the base of the ventrals. Caudal not very deeply furcate; its lobes are broad, rather than slender, the lower lobe is generally a little the longer. The color of the body is dark chocolate above, and of a silvery white below; these two colors are separated by a longitudinal band of a darker color than the back, extending from the end of the snout through the eye in a direct line along the sides to the middle of the base of the caudal. The whole dorsal region is mottled with black blotches, sometimes running together and forming large patches, and often descending to the lighter portion of the sides. Scales rather small. Called Minnow at Huntsville. Found in the Spring branch.

Chondrostoma, *Agass.*—This genus was established by me in 1834 in the Mémoires de la Société des Sc. Nat. de Neuchâtel, for the Cyprinus Nasus of Europe, and has been adopted with various modifications by subsequent writers. Thus far no representative of this type had been known to exist in North America, though the species I now refer to it here, has been described for sometime by Dr. D. H. Storer; but having been referred to the genus Leuciscus, to which C. Nasus was also referred formerly, it has not been distinguished from the ordinary Leucisci. I need only allude to it for the present.† Other species occur in the fresh waters of the Pacific coast of North America. ***Exoglossum dubium***, Kirtl., may belong to this genus.

Chondrostoma prolixum, Agass.—Leuciscus prolixus, *Storer*, Synops., page 165. Called Minnow at Huntsville. Found in the Spring branch.

* I am indebted for another new species of this genus to Dr. I. H. Rauch, of Burlington, Iowa, which I would call *R. Meleagris*, Ag. It is remarkably short and stout in comparison to its congeneric types, also smaller. The whole body is dotted with black upon a silvery ground, the dots partly confluent; the belly only is plain silvery white.

† I owe another entirely new species of this genus to Dr. I. H. Rauch, of Burlington, Iowa; which I inscribe as *Ch. pullum*, Ag. It is the smallest species of the genus; much broader than the others in comparison to its length; head especially small, almost indicating distinct generic peculiarities, into which I am however unable to enquire from want of a sufficient number of specimens. This pretty little fish is of a peculiar deep but dull green, darker above, passing into yellowish white below.

HYBOPSIS,* *Agass.*—So little attention has thus far been paid to the generic differences existing between the American Cyprinoids that it is not surprising to find several yet unnoticed. Among others I mention here a new type remarkable for its slender elongated form, its long head, its obtuse prominent snout, its inferior mouth and the advanced position of the anal. This genus is founded upon a small species from Huntsville. *Leuciscus Storerianus*, Kirtland, which I have however not examined in nature, may be another species.

Hybopsis gracilis, Agass.—Body much elongated and slightly compressed; head long, equalling nearly one-fourth the entire length of the fish. The snout is very short and broadly rounded; the nostrils are large, above the middle line of the eye and nearer the end of the snout than the centre of the eye. The eyes are very large in proportion to the size and width of the head; the horizontal diameter which is slightly the longest, equals one-third the length of the head, their upper edge is on a line with the top of the head, the lower edge with the anterior edge of the intermaxillaries and the extremity of the upper maxillaries reaches the line of their anterior border. The fins are all long and pointed. The pectorals are low down on the sides and reach the base of the ventrals. The hinder base of the dorsal is midway between the end of the snout and the extremity of the tail. The height of the dorsal is one-third greater than the length of the base; the second and the third rays longest; number total of rays 8, and two united as one for the last ray of the fin. The base of the ventrals is below the anterior part of the dorsal; their extremities reach nearly to the anal fin. The distance of the anal from the base of the tail is equal to twice the length of its own base. The anal is like the dorsal in form, but smaller, number of rays 7, with a last double ray. Caudal long, deeply furcate, the lobes being slender and pointed.

CHROSOMUS, *Rafin.*—The fish for which Rafinesque established this group in his genus Luxilus, well deserves to be considered as

* While these pages were setting in type, I have received another pretty species of this interesting genus, through the attention of Dr. I. H. Rauch, from Burlington, Iowa. The large number of specimens obtained enables me to make some additions to the characteristics of the genus: "The mouth is protractile downwards, after the fashion of Catostomus, so much so that had I not had ample opportunity to examine young Catostomi, and to study the changes they undergo with age, I might have supposed my Hybopsis to be the young of some species of that genus. Moreover the lips are not swollen nor thickened. The pharyngeal teeth differ also greatly from those of Catostomi, there being only four or five compressed and hooked ones in each main row, and one or two in a second row."

This new species differs from that of Huntsville, by its smaller size, its more pointed snout and the peculiar coloration. A deep black narrow band extends from the neck to the base of the caudal along the whole back, dividing in advance of the dorsal to encircle that fin, and uniting again behind it upon the middle line. General color olive, silvery upon the sides, the dorsal and caudal faintly tinged with rose color and a deeper rose-colored spot upon the base of the first ray of the dorsal. I shall call this species *H. dorsalis*, Ag.

a distinct genus, as it stands very isolated among the other American Cyprinoids. It may be considered as corresponding upon this continent to the genus *Phoxinus* of Europe, from which it differs however by the continuous lateral line and the shorter lower jaw. Rafinesque has given it a very appropriate specific name, calling it

Chrosomus erythrogaster, Raf.—It is one of the prettiest freshwater fishes of North America, varying greatly with age and at different periods of the year. It remains yet to be ascertained whether the specimens from the Tennessee River are strictly identical with those from the Ohio River. I have received specimens from the Osage River, from Mr. G. Stolley, which differ somewhat in having deeper colors and a somewhat elongated form.

Stilbe, *DeKay*.—In his Natural History of New York, DeKay has established this genus for the Cyprinus chrysoleucos of Mitchell. Without a thorough revision of the many new genera of Cyprinoids established by Heckel and Prince Canino, for which I have not the necessary materials on hand, I am unable to decide whether DeKay's genus may stand or not. So much however is certain, that Storer's *Leuciscus obesus* from Florence, Alabama, which has also been obtained in the vicinity of Huntsville by Dr. Newman, also belongs to this genus. *Abramis versicolor*, DeKay, must also probably be referred to it. I know several other undescribed species of this type from other parts of the United States. It is intermediate between Alburnus and Abramis, having the form of Abramis elongatus, and other elongated species of that genus with comparatively small anal, and the prominent lower jaw of Alburnus.

Stilbe obesus, Agass.—Leuciscus obesus, *Storer*, Synopsis, p. 166. Called Hickory or Gizzard Shad at Huntsville.

Hypsolepis, *Baird*.—This genus was established for those species of Leuciscus the body of which is compressed and covered with high short scales. *Leuciscus cornutus* may be considered as its type. My *Leuciscus frontalis* from Lake Superior, is another species of this genus. To it belongs also Dr. Storer's *Leuciscus gibbosus* from Florence, Alabama, which has also been found about Huntsville, by Dr. Newman.

Hypsolepis gibbosus, Agass.—Leuciscus gibbosus, *Storer*, Synop., page 166. Called Silver-sides at Huntsville.

Leuciscus, *Cuv*.—One species from Huntsville, the same which Dr. Storer has described from Florence, Alabama, under the name of

Leuciscus croceus, Stor., Synop., p. 165.

SAUROIDS, *Agass.*—Before I began to collect the materials for a monograph of the genus Lepidosteus, I had no idea of the wide geographical range of this type in North America. Indeed our ichthyological works mention only Lake Huron, Lake Erie and Lake Champlain in the North, the Ohio and Mississippi in the West, and S. Carolina and Florida in the South, as its home, and the whole number of species described, even including all those of Rafinesque without questioning the validity of any of them, does not exceed nine or ten. Yet I have now, in my own collection, not less than twenty-two well characterized species of the genus, and I have ascertained its existence in all the water systems of the South from Florida to Texas, in the Mississippi and all its larger tributaries up to the latitude of Lake Superior, where it does not however occur, in all the lower great Canadian Lakes, and in the St. Lawrence. Also in those river and Lakes of western New York which empty into the waters of the St. Lawrence; in those of western Pennsylvania emptying into the Ohio, and in all the Atlantic rivers, from the Chesapeake Bay to Florida; leaving only the New England States East of Lake Champlain without any of its representatives. Poey describes also one species from Cuba. It seems however to be wanting west of the Rocky Mountains and in Central America. The species sent me by Dr. Newman from Huntsville, agrees with Rafinesque's

Lepidosteus platostomus.—It differs however from the species described under the same name by DeKay from Florida, the original specimen of which I have examined myself. Its name at Huntsville is *Gar.*

The identification of species in this genus is extremely difficult owing to the great changes they undergo with age. Indeed the young differ so much in form and structure from the adult that Rafinesque has established a distinct genus for the young of his *Lepidosteus oxyurus* under the name of *Sarchirus vittatus.* In this immature state these fishes have the upper region of the caudal separate from the lower, as a distinct lobe, the body is scaleless and the pectorals consist of a membrane rising from a fleshy tubercle, hence the name Sarchirus of Rafinesque. Another peculiarity of the young lies in their coloration; they having mostly a broad longitudinal black band along the middle line. This has for a time led Rev. Z. Thompson to consider the young of Richardson's *Lepidosteus huronensis* as a distinct species which he has described as *L. lineatus.* DeKay's *L. Bison* is also the same species as L. huronensis; this differs however widely from the southern *L. osseus* and from Rafinesque's *L. oxyurus* from the Ohio River. I shall take an early opportunity of describing all the species I know of this genus and settling as far as possible their complicated synonymy.

CŒLACANTHS, *Agass.*—Until an extensive and minute comparison of all the representatives of the genus Amia from different parts of the United States can be made to ascertain the true value upon which the different species described by Richardson, DeKay and Valenciennes, are founded, it may be sufficient to mention here the existence of that genus in the waters of the Tennessee under the name of

Amia calva, L, which has long been considered and may in reality be the only one of the genus. It is known at Huntsville under the name of Scaly Cat and Carp. Found in Mill ponds.

SILUROIDS, *Cuv.*—Two species of this very natural family have been sent to me from Huntsville by Dr. Newman.

Pimelodus cœrulescens, Rafin.—Channel Cat. Grows very large and weighs occasionally over one hundred pounds.

Pimelodus Catus, Lin.—Several species are confounded under this name; but it is impossible to characterize them without entering into details which would be out of place in this short notice. Called Mud Cat at Huntsville.

STURIONES, *Cuv.*—Two species of Sturgeons occur in the Tennessee, specimens of which I have received from Dr. Newman.

Acipenser rubricundus, Lesueur.

Acipenser maculosus, Lesueur.

These two species have been considered as synonymous by some ichthyologists. It is true that the young A. rubicundus like all young Sturgeons are more or less maculate, and yet there are so many other differences between the two specimens I have before me, which are nearly of the same size, that I can hardly consider them as identical. The whole genus requires a thorough revision and would be an interesting subject for a monograph.

There are some genera of North American fresh-water fishes the absence of which surprises me in the collection sent by Dr. Newman, and mention them with the view of calling attention to them more particularly. *Lucioperca*, generally called *Salmon* in the West. Is it not possible that the specimens of Hyostoma described above were mistaken for young Lucioperca and sent as specimens of the Salmon? *Labrax*, known everywhere as White Perch. The presence of the genus Perca seems more doubtful. *Chatœssus*, generally known as Hickory or Gizzard Shad. I fancy that the *Stilbe obesus* mentioned above, was mistaken for a small specimen of this type. *Hyodon*, known as Toothed Herring. *Anguilla*, the Eel. *Lota*, known as Barbot or Eelpout. The genus *Pogostoma*, of Rafinesque, is evidently synonymous with Lota. *Polyodon*, known as Shovelbill, and *Petromyzon*, the Lamper-eel. I should also expect a long-billed

species of Lepidosteus, for the two types of this genus occur everywhere together in the West.

If the study of the geographical distribution of animals is ever to furnish us any indications respecting the circumstances under which organized beings were created, we must, in investigating it, turn our attention particularly to those facts which disclose differences of structure in connection with the special localization of the different representatives of each family within their natural boundaries. For years I have been collecting diligently all the data within my reach bearing upon this question, and from the results of this enquiry already in my possession, I am satisfied that the day is not far distant when we shall know with sufficient precision *where* all the living beings now existing upon earth have made their first appearance. This must of course be the first step towards a deeper insight into the *conditions* of that origin itself.

In connection with this train of thoughts it is interesting to notice how much different families of animals vary from each other in the most prominent features of their geographical distribution. There are those the representatives of which are almost uniformally distributed over the whole range of their natural arena. Such is the family of Salmonidæ proper. There are species of true Salmo, of Thymallus, of Coregonus, of Osmerus very equally scattered over Europe, Asia and North America. The same is the case with the family of Esoces, which has however a much greater number of species in the fresh waters of North America. So are also the Sturgeons, with this difference, that upon the continent of America two peculiar genera, Scaphirhynchus and Polyodon, are added, which have no representatives in the old world. The Percoids however present very different combinations: some types are common to North America, Europe and Northern Asia, as the genera Perca, Lucioperca and Labrax, with this difference however, that North America has many fresh water representatives of the genus Labrax which are wanting in the old world; other types are only to be found either in North America or in the old world,—for instance Grystes, Centrarchus, Pomoxis, Amploplites, Calliurus, Pomotis, have no representatives in Europe where we find in their stead the genera Aspro and Acerina; the balance being in favor of North America as far as the number and diversity of the fresh-water types of this family is concerned, whilst the old world has many more and more diversified marine representatives. The family of Cyprinoids agrees with that of the Percoids in the features of its geographical distribution; the types peculiar to each side of the Atlantic being however more equally distributed, for whilst in the old world we find the genera Cyprinus, Barbus, Tinca, Cobitis, Pelecus, Aspius, Rhodeus, Phoxinus, North America has its Car-

piodes, Ictiobus, Cycleptus, Catostomus, Rhinichthys, Chrosomus, Hypsolepis, Hybopsis which are foreign to the old world, and they share together the genera Alburnus, Chondrostoma, Leuciscus, &c., still, with this difference, that the true Leucisci are far more numerous in the old world than in North America. In the family of Cyprinodonts we find exactly the reverse, there being in North America a much greater diversity and a larger number of representatives of this type than in the old world. The case is still different with the family of the Etheostomoids; which are altogether peculiar to North America, not a single species being known in the old world. The family of Cœlacanths is also entirely foreign to the old world, whilst the Sauroids are represented by one genus, Polypterus in the old world and by another, Lepidosteus in America. The Sciænoids differ in another respect: whilst these fishes inhabit exclusively the sea in the old world, there are in North America besides many marine representatives, a number of fresh-water species constituting a distinct genus, Amblodon. Again the family of Siluroids, is represented by a great variety of species in North America, and only by a few in the old world. Similar facts might be mentioned of other families, but this may be sufficient to show how important it is to combine the study of the modifications of the structure of animals with that of their geographical distribution.

For it is not the presence here or there of this or that species of any genus, or family or higher group which I would particularly consider in the study of the geographical distribution of organized beings, but the localization upon certain parts of the surface of the globe of special modifications of definite types representing each a distinct idea, expressed in a variety of living forms and combined in various ways in time and space.

There is another point of view of equal interest in this connection; the mode of association of different families in different parts of the world. It is a fact for instance that the Goniodonts are limited to South America, and that this family, which is entirely wanting in the old world, has no nearer relative than that genus of Sturgeons peculiar to North America, the Scaphirhynchus. Again, whilst the families mentioned above as characteristic of the North American fresh-water fish fauna seem to be equally distributed over the surface of this vast continent, there is yet a special adaptation of some of their types to peculiar localities. The great similarity of their representatives throughout the Southern Atlantic States, the Gulf States and the Mississippi Valley, as high up as the Ohio, including even Lake Champlain, does not extend to the New England States, which although encircled by this uniform combination of fresh-water animals, have another zoological character, peculiar to itself, and approximating more to that of the old world under the same climatic conditions

than the western and southern parts of the Union. In this isolated region of North America, in this zoological island of New England, as we may well call it, we find neither Lepidosteus, nor Amia, nor Polyodon, nor Amblodon, nor Grystes, nor Centrarchus, nor Pomoxis, nor Ambloplites, nor Calliurus, nor Carpiodes, nor Hyodon, nor indeed any of the characteristic forms of North American fresh-water fishes, so common everywhere else, with the exception of two Pomotis, one Boleosoma, and a few Catostomus. The study of these features is of the greatest importance, inasmuch as it may eventually lead to a better understanding of the intentions implied in this seemingly arbitrary distribution of animal life.

Before closing this notice I would remark that there is still another very interesting problem respecting the geographical distribution of our fresh-water animals, which may be solved by the further investigation of the fishes of the Tennessee River. This water course, taking the Powells, Clinch and Holston Rivers as its head waters, arises from the mountains of Virginia in latitude 37°, it then flows S. W. to latitude 34°·25, when it turns W. and N. W., and finally empties into the Ohio under the same latitude as its sources in 37°. The question now is this: Are the fishes of this water system the same throughout its extent? in which case we should infer that water communication is the chief condition of the geographical distribution of our fresh-water fishes. Or do they differ in different stations along its course? and if so, are the differences mainly controlled by the elevation of the river above the level of the sea, or determined by climatic influences corresponding to differences of latitude? We should assume that the first alternative was true if the fishes of the upper course of the river differed from those of the middle and lower course in the same manner as in the Danube, from its source to Pesth, where this stream flows nearly for its whole length under the same parallel. We would on the contrary suppose the second alternative to be well founded, if marked differences were observed between the fish of such tracks of the river as do not materially differ in their elevation above the sea, but flow under different latitudes. Now a few collections from different stations along this river, like that sent me by Dr. Newman from the vicinity of Huntsville, would settle at once this question, not for the Tennessee River alone, but for most rivers flowing under similar circumstances upon the surface of the globe. Nothing, however, short of such collections, compared closely with one another, will furnish a reliable answer. I know already from a mere catalogue of the vernacular names of the fishes from the vicinity of Jonesboro, sent me by Dr. Cunningham, and from a few specimens collected by Prof. Erni, late of Knoxville, that the fishes of the upper and lower course of the Tennessee differ greatly from

each other, without being able to tell exactly how, from want of specimens. To set this question completely at rest, it would be best to obtain collections from the different tributaries of the Tennessee, as well as from the main stream, one from the Powells, one from the Clinch, one from the Holston, one from the French Broad, &c., and from the main river, one from the vicinity of Washington, Tenn., or from Chattanooga, another from Florence, (the Muscle Shoal being the point, as I am informed by Dr. Newman, above which fish do not migrate in the Tennessee,) and another anywhere above its junction with the Ohio, perhaps best about Reynoldsburg, at some distance from the Ohio. Whoever will accomplish this survey will have made a highly valuable contribution to our knowledge.

Appendix.—*Additional Notes on the Holconoti.*

Having lately received a large number of specimens of *Holconoti*, from California, through the kindness of my friend, T. G. Cary, Esq., of San Francisco, I avail myself of this opportunity to make several additions to my first notice of that remarkable family. As I had anticipated, the number of species belonging to it is rapidly increasing. I have now no less than six distinct species before me, presenting even a far wider range of differences than I was prepared to find among them, which has led me to establish several new genera, besides *Embiotoca*. Respecting the family characters, I have to add that there is *another space deprived of scales, extending along the middle line of the belly, from the sides of the ventrals to the base of the anal*, undoubtedly a provision to facilitate the dilatation of the abdominal cavity during the growth of the astonishingly large young of these fishes. It is rather surprising, however, that this scaleless space exists also in the males, and this might be considered an objection to the explanation just given, did we not find also tits and mammary glands in the males of Mammalia. Nevertheless *the males and females differ widely from one another*, in each of the four species of which I have thus far been able to obtain both sexes. This circumstance adds greatly to the difficulty of distinguishing and characterizing the species. The males are uniformly smaller than the females, contrary to what has been observed in the genus *Pœcilia*, in which the males (*Mollinesia*) and the females (*Pœcilia*) differ so much as to have been considered as distinct genera, but agreeing in this respect with my genus *Heterandria*, in which the males are also smaller than the females. The difference consists chiefly in the peculiar form of the anterior part of the anal in the males, which resembles somewhat that of the male of *Mallotus villosus*, being more rigid and more expanded than in the females. The jaws are more or less protractile. Air bladder large and simple. In

males the sexual aperture is at the summit of a projecting conical papilla. The genus *Embiotoca* as first established, does not require modifications; I have only to add a new species to it, and to mention some features by which it differs from the following genera: The spinous portion of the dorsal is uniformly low, so that the soft portion rises abruptly to a much greater height; the anterior *articulated* rays of the anal *simple* and *not branching* at their extremity. In the male the anterior articulated rays of the anal are swollen near their base, forming a continuous longitudinal ridge on each side of the fin. This ridge is variously modified in the different species. The jaws are moderately protractile; the lower lip is fixed by a frenum to the symphysis of the lower maxillaries, and not free and moveable all round the jaw. The young of the third new species of this genus resembles exactly those of the two formerly described, but differ remarkably from those of another species belonging to a new genus which I shall mention below, thus showing that there are generic modifications in the growth of the young, though the mode of reproduction is exactly the same in all. In Embiotoca proper, the young resemble most remarkably the mother, about the time of their escape from their confinement, except in color; in addition to the peculiarities described in my former paper, I would mention a large black diffused spot upon the anterior part of the soft portion of the dorsal and of the anal, which is found in the *young* of all three species of this genus, whilst *E. Caryi* alone shows signs of it *when full grown.* The male papilla is rather large.

Embiotoca Caryi.—I possess the most complete series of this species, for besides two pregnant females with young ready to escape, caught in July, I have males and females of various sizes caught in January; at this period the marsupial sac is reduced to a fusiform tube, extending from the sexual aperture to the anterior extremity of the air bladder, but the state of preservation of the intestines did not allow a minute examination of its structure. The male, which is more elongated than the female, has also much brighter colors: the longitudinal and transverse bands of the body are more distinct, the black specks upon the soft dorsal and the anal are more brilliant, and the cheeks, opercule, jaws and chin are adorned with bright blue blotches more or less confluent; the ground color of the body seems to vary from olive on the back to a yellow-orange upon the sides.

Embiotoca Jacksoni.—The form of the male does not differ quite as much from that of the female in this species. as in the preceding, though it is also slightly narrower. The color, as far as I can judge from alcoholic specimens, is of a deeper olive green, whilst the female is more yellowish.

Embiotoca lateralis, Agass.—Resembles closely E. Jacksoni in general form and appearance, but seems to bring forth its young

at an earlier period, for among several specimens caught in July, only one was full of young, and that was a younger specimen. The body is dark olive above; sides with alternate silver-gray and rusty bands; fins brown. In younger specimens the longitudinal bands are more yellow, and the fins also yellowish.

RHACOCHILUS, *Agass.*—In this genus the vertical fins have the same structure as in Embiotoca and the sexes differ in the same manner; but the jaws are very protractile, almost as in our southern Lachnolæmus, and the lips very fleshy, the lower lip especially broad, lobed and have their outer margin free from the jaw bone all round, and not attached by a frenum to the chin, as in Embiotoca and Amphistichus. Teeth few and only in front of the jaws, and none on the sides. The body is also more elongated. The young differ widely from those of the preceding genus: their form is more elongated, the caudal remarkably large and long and truncate at its extremity, whilst it is forked in Embiotoca; and the extremities of the dorsal and anal extend beyond the base of the caudal, whilst in Embiotoca they do not even reach it; finally there is no black speck upon either the dorsal or the anal.

Rhacochilus toxotes, Agass.—Color uniform olive above; sides silvery with light longitudinal bands; female darker than male; vertical fins and ventrals dark; male blackish upon opercule and cheeks. Female with mature young in July.

AMPHISTICHUS, *Agass.*—The spinous rays of the dorsal shorter than the soft rays, but gradually increasing in length, so that the soft portion of the fin does not rise abruptly higher than the spinous portion, though the anterior soft rays are the longest of the fin. Articulated rays of the anal *all divided*, and not simple in front as in Embiotoca, nevertheless the fin is separated into an anterior and a posterior portion, by the introduction in the *male* of a short flat-triangular ray, which produces a deep emargination in the outline of the fin, and in the *female* by the presence of two or three articulated rays of equal length with the others but much *stouter* and *oftener divided.* In the male the anterior rays are swollen as in Embiotoca and Rhacochilus. Papilla of the males very large. Jaws little protractile; with two rows of teeth above and below, lips thin, lower lip not free in the middle. The young have not been observed, the specimens obtained having been caught in January.

Amphistichus argenteus, Agass.—Bluish gray above, sides silvery with occasional indistinct and irregular transverse bands of olive color. Vertical fins yellowish.

HOLCONOTUS, *Agass.*—Dorsal long, and lowest behind, its spinous rays being the longest; the anterior and posterior parts of

this fin are not separated by a depression, but its outline descends regularly from the fourth or fifth anterior spinous rays to the posterior extremity. Structure of the anal the same as in Amphistichus but proportionally longer; the sexes differing also in the same manner. Young not known, the female obtained having been caught in January. Jaws very slightly protractile, lower jaw projecting; two rows of teeth in the upper jaw only. Lips not fleshy; lower lip free all round.

Holconotus rhodoterus, Agass.—Bluish gray above, silvery upon the sides with rose colored spots in irregular longitudinal lines; vertical fins, especially the caudal, reddish.

I have just been informed (February 28th) that the California Academy of Natural Sciences claims for Dr. W. P. Gibbons the discovery of the viviparous fishes upon which I had established the family *Holconoti* and the genus *Embiotoca;* but upon what ground I am not informed. This is a question in which I am entirely disinterested, having thus far been only the historian of the discovery and the biographer and godfather of the fishes. Dates and reference to other publications which may have been made in California, will easily settle the question of priority which as far as the discovery of the viviparity of these fishes is concerned, rests between Mr. Jackson and Dr. Gibbons, and not with me. I learn also, from the same quarter, that Dr. Gibbons has dedicated to me a new species of this family and that the California Academy has inscribed another species to him; but I have not yet seen descriptions of them. Should either of these species coincide with one or the other of those described above, I shall of course adopt, in the more elaborate paper, accompanied with figures, which I am now preparing upon this family, the names first established in accordance with the rules of our science.

The knowledge of this curious family is likely to lead to many other interesting disclosures. Dr. Thom. H. Webb, one of the scientific corps of the Mexican Boundary Line Commission, has sent me under date of Dec. 9th, 1853, the following abstract from his diary, dated San Diego, May 3, 1852: "Capt. Ottinger, of the U. S. Revenue service, caused his seine to be drawn for us to-day. Caught many Tiger and Shovel-nose sharks, two flounders, . . . also a number of small fish, about two or three inches long, *each of which contained ten or twelve living young*." He adds: "The viviparous progeny I exhibited to the Commissioner and several of the gentlemen of the Commission; and I also kept quite a number of them alive, in a vessel of water, for some days. In the mother they were not, so to speak, indiscriminately huddled together, but methodically arranged, and so placed in relation to each other as to form a compact series, without the loss of interstitial space, in other words, so disposed as to best accommodate the family. On leaving San Diego, I took extra pains to preserve

specimens of this fish, but these special efforts proved an injury," &c. We may therefore confidently look forward for some new type of viviparous fish from San Diego. Mr. Wm. Couper of Toronto, Canada, writes me also that an intelligent young man residing in Buffalo, New York, obtained some fish taken at Black Rock, in which a number of young were found enclosed in a pouch attached to or near the back bone, resembling the parent in form. May this not be some Cyprinodont? I am inclined to believe it, since I have of late ascertained that many of our representatives of that family, if not all, bring forth living young, though these are very small at the time of their birth.

That among our Sharks the Dogfish (*Acanthias americanus*, St.), is viviparous, has long been known. So is also *Mustelus Canis*, Mitch. But Mr. Thayer S. Abert, of the U. S. Engineers, informs me that the Stingray of the coast of North Carolina also brings forth living young. This would be, as far as I know, the first example of a viviparous species in the family of Rays.

Art. XXXIX.—*Observations on the Development of the "Surinam Toad" (Pipa Americana);* by Jeffries Wyman, M.D.

(Presented to the Boston Society of Natural History.)

The specimens upon which the following observations were made, were obtained by Dr. Francis W. Craigin, U. S. Consul, to whom the Society has been so frequently indebted for his generous and munificent contributions to its cabinet, of zoological collections from South America. The habits of these extraordinary animals during the reproductive season are well known. The eggs are transferred by the male to the back of the female, to which they adhere, and where they are impregnated; their presence excites increased activity in the skin, it thickens, is gradually built up around each egg which it at length nearly encloses in a well-defined pouch; this process of investment has been compared by J. Müller and others to the inclusion of the mammiferous ovum by the deciduous membrane of the uterus. The opening which is left after the pouch is formed, is at length closed up by an operculum, and thus the egg is shut off from all direct communication with the air.

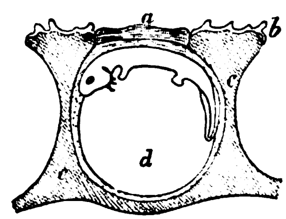
Of the eight specimens which I have examined, two were destitute of eggs in the back, and the skin of these presented a uniform surface throughout covered, as is usual, with conical papillæ. One of them I ascertained by dissection to be a female, the ovaries being well filled with eggs. In the backs of all of the others, ova existed in different stages of development, the number of egg-sacs varying in different specimens from forty to one hundred and

fourteen. The structure of the sac may be understood from an inspection of fig. 1, which represents a magnified vertical section through the whole thickness of the skin: *a* represents the operculum, *b* the epidermis, *c c* dermis or true skin, and *d* the yelk with its embryo. The sacs are at variable distances from each other, sometimes so closely approximated that the intervening integument is reduced to the thickness of a piece of paper. The operculum adheres to the circumference of the mouth, and there is usually found just beneath it a layer of gelatinous matter which is continuous, in some instances at least, around the whole circumference of the egg. The structure of the operculum as seen beneath the microscope was not homogeneous, but seemed to be composed of ill-defined fibres, not unlike those of the white element of areolar tissue, and there were intermixed with them granules of pigment. The interior of the pouch was covered by a layer of pavement epithelium, continuous at the orifice with the cuticle covering the surface of the body; it was easily detached, and its cells were nucleated and contained colored granules. Beneath the skin there exists over the whole back a large cavity, as in Frogs, but unlike the one in them no nerves were seen passing through it, in the region of the spine, to the integuments.

The eggs are quite remarkable when compared with those of other Batrachians for their great size, the yelk alone measuring one-fourth of an inch in diameter. In almost every instance, on removing the operculum, the embryo, however small, was found just beneath it, and thus occupying a position on the yelk which had the nearest proximity to the air.

In the earlier stages, as seen in fig. 2, the head is broad and flat, the cerebral vesicles are easily detected, the lateral portions not having united on the median line; the eyes were prominent

1.



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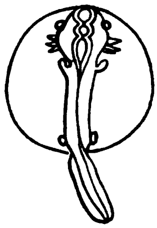

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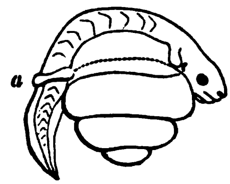


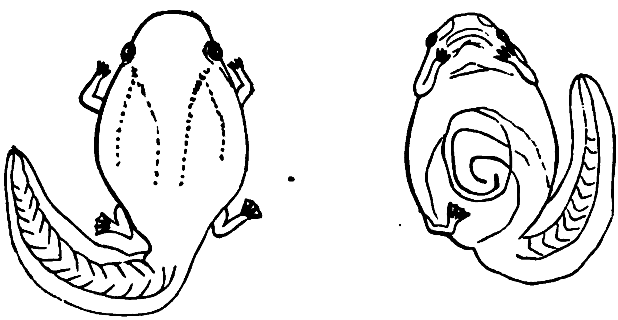
and black; the spinal canal was closed and the ventral laminæ were just beginning to extend upon the upper surface of the vitellus; the arms consisted of pyriform processes from anterior portion of the trunk, but the legs consisted of oval masses entirely disconnected with the parts surrounding the vertebral column, and seemed to have an independent centre of growth, and there-[illegible] did not bud out from the trunk. In all of the earlier speci-

mens three branchial appendages were visible on each side of the head. The general aspect of the embryo, as it lies extended on the surface of the yelk, reminds us of the larval condition of Salamanders and Tritons. The vitelline vessels communicated with the trunk by means of two afferrent vessels on each side of the head, and several efferrent ones on the sides of the trunk.

In a later stage as exhibited in another series of embryos (fig. 3), the external branchiæ had disappeared, the legs (*a*) now united with the trunk, were terminated by an expanded extremity, the rudiment of a foot; the ventral laminæ as represented by the dotted line extended farther down upon the yelk, but still this last was to a great extent uncovered; the nostrils were visible as round terminal depressions, but it was not ascertained if they communicated with the mouth. A small branchial fissure was detected on each side of the neck, and within this, as was shown by slitting open the mouth and œsophagus, there existed on each side a series of fringed branchial arches.

The most extraordinary feature however of this stage was the peculiar change going on in the yelk mass; the whole of the yelk substance was moulded into a spiral coil, fig. 3, and invested with a thin tunic, and thus converted at once into a spiral intestinal canal, the coils of which extend from the sides of the trunk to the most prominent portion of the yelk, and there changing direction and occupying the axis of the coil the intestine passes back again towards the trunk. The *whole* yelk-mass is therefore moulded into a spiral intestine.

4. 5.



In the most advanced stages which were examined (figs. 4 and 5) the ventral laminæ had almost entirely included the intestinal canal as seen in fig. 5; the papillæ and rows of tubercles upon the skin were developed, the intestine had increased in length, and the extremities had become elongated and were provided with well defined toes. The legs were folded against the sides of the body, usually one towards the back and the other towards the abdomen, and the tail which had become proportion-

ally much larger, was folded against the side and directed towards the head. The mouth as in the preceding instances, was not terminal, but receded a little on the under side of the head.

When the most advanced ova are compared with those which had made the least progress, as will be seen by reference to figs. 2 and 4, which are proportionally magnified representations, it is quite obvious that in the later periods, the mass of the embryo is much greater than that of the yelk and the embryo of the earlier. This increase as shown by weight was found as follows:— the embryo represented by fig. 2 weighed 2·95 grains, and that by fig. 5, 3·37 grains. It is not improbable that if earlier and later periods had been compared, the difference would have been still greater.

In none of the instances which fell under my notice had the final metamorphosis taken place. But Bonnet, Dumeril and others have observed that the tadpole remains in the dermal sac until its limbs are perfectly formed and the tail has been absorbed, until in truth it arrives at the same stage reached by the common toad, when having finished its aquatic life to which it is no longer adapted, it leaves the water and seeks its livelihood in a more congenial manner on the land.

Remarks.—In addition to the unusual circumstances under which these animals are developed, it will be seen that they are objects of especial interest in connection with the general subject of the development of Batrachian reptiles. The first peculiarity which may be noticed is as to the period at which the arms and legs make their appearance. The tadpoles of frogs and toads acquire a comparatively advanced stage of development before any traces whatever of limbs are seen; they leave the egg, increase in size, and after a period (variable in the different species) of weeks or months, the rudimentary arms and legs begin to be formed. In Pipa they appear before the external branchiæ disappear, and before even the ventral laminæ descend upon the sides of the vitellus. The development of the limbs, independently of the vertebral column, is an interesting morphological fact.

That the first trace of a leg is an oval mass, entirely free and detached from the vertebral axis, goes to show that whatever view we may adopt, with regard to the homology of legs, the pelvis included, they are something superadded to it and not evolved from it, or any of its processes. I have ascertained by direct observation, that even among Frogs, the legs which appear on each side of the tail, in the form of small papillæ, are primarily tegumentary growths, that beneath these there is developed a cartilaginous plate which gradually extends upwards on each side until it meets with the transverse processes of the vertebral column, with which it becomes permanently connected under the form of the pelvis, and at the same time the papillæ are de-

veloped into limbs with their contained bones; thus the pelvis which in the adult seems to be an appendage to the vertebral column, is in the embryo an independent structure, just as the tooth is primarily independent of the jaw. In this mode of the development of the legs, we have a temporary analogy to the permanent constitution of the same parts in Fishes, in which the ventral fins are never connected with the vertebral column by their pelvic bones, these being confined to the abdominal surface of the body.

The complete development of the tail, adapted to swimming, is under the circumstances worthy of attention. In the ordinary Ranidæ the phases of development are in accordance with the peculiar conditions under which the earlier periods of life are passed; their habits are not only wholly aquatic, but they have many of the anatomical and physiological characters of fishes, among which may be mentioned the existence of branchiæ, for aquatic respiration, and a broad and compressed tail for aquatic locomotion. The embryos of Pipæ differ from those of other allied genera, in passing through all of their embryonic phases in closed dermal sacks, where they neither breathe by the action of aquatic currents, nor are capable of executing the ordinary locomotive movements; yet the external branchiæ are developed, disappear and are replaced by internal branchiæ, and these in turn by lungs; the tail also acquires its full development with swimming adaptations, in the form of muscles and folds of skin, as in other tadpoles, and after having existed for a certain period, is removed by absorption, without having been even once made use of as a locomotive organ. It appears that in this particular instance the exigencies of embryonic life do not require the existence of a tail for the purposes of locomotion, and its presence seems to be accounted for only on the supposition of the existence of a preestablished plan according to which Batrachians generally are developed, and this plan is adhered to, although the organ may not be used or not used in the same way as in the other species. It is possible that the materials of the tail serve as a store of nutritive substance, though this seems scarcely probable; but even if this be the case, it is none the less a fact that the part assumes a structure, the adaptations of which have reference to a function wholly different.

As regards the existence of branchiæ, I have observed an analogous instance in the embryos of *Salamandra erythronota*, where these organs are developed externally, though the eggs are deposited under a log, and the animal is not aquatic at any period of life.

The only other subject to which it is proposed to refer, is the growth of the embryo, by which there is formed at the end of incubation a larger mass than existed in the egg when it commenced.

This increase in bulk could have been effected in no other way than by an absorption of materials furnished by the dermal sac, since the existence of an operculum would prevent the entrance of nutritive matter from without. The gelatinous matter which originally surrounded the egg, may have contributed something, but still there is growth after this has disappeared. It seems highly probable, therefore, that the walls of the pouch secrete the necessary additional quantity to supply all that is required for development. In so far as observation has extended, this is a solitary instance among Batrachians if not among Reptiles generally in which the embryo is nourished at the expense of materials derived from the parent.

Among fishes analogous facts exist. Dr. John Davy has shown that the undeveloped but mature egg of the Torpedo in June does not exceed 200 grs. in weight, whereas in the month of September the fœtuses weigh from 428 to 580 grs. The recently discovered genus, *Embiotoca*, Agass., exhibits to us not only the fact of a true ovarian gestation as in Pœcilia, but that the young continue to grow in the ovary till they acquire a very extraordinary size. In *Embiotoca Jacksoni*, for example, Professor Agassiz states that a specimen 10½ inches long and 4½ inches high, the young were nearly three inches long and one inch high. He was led to the belief that in this case the water entered the ovarian sac, since the gills were found so highly developed. If this supposition should prove correct, it still seems quite improbable that the young fish should acquire all the materials for its growth from whatever may be in this manner casually introduced.

Art. XL.—*Researches on the Development and intimate Structure of the Renal Organs of the four Classes of the Vertebrata;* by W. I. Burnett, M.D., Boston.

There are two facts strikingly indicative of the importance of the urinary organs in all the higher forms of animal life. These are: first, their widely distributed presence; and second, their early appearance in developing embryonic forms; I might, perhaps, add as a third fact, that their functional activity is usually in pretty exact ratio with the grade of organization.

Throughout the higher classes of the Invertebrata,* and in all the Vertebrata, these organs are present, and their functional relations quite prominent; it is only in the lower classes of the animal kingdom, where there is an absence or incompleteness of a

* Urinary organs are not found lower than the Cephalophora, in which, as also in the Cephalopoda, Crustacea, Arachnoidæ, and Insecta, they assume an important function, and are often of a complex structure. See Comparative Anatomy, by *Siebold* and *Stannius*, transl. &c. by *Burnett*, vol. i, §§ 223, 255, 288, 314, 345.

true circulatory system distinct by itself, that these structures are wanting. The kidneys are organs inseparably connected with an important blood-function, and in the Vertebrata where the conditions of organization rest upon an active and widely distributed circulation, they possess determinate anatomical and physiological characteristics, and may therefore form the subject of a distinct and complete investigation.

On this account, I limit myself in the present paper to an examination of these organs as observed in the four grand classes of Vertebrata: Fishes, Reptiles, Birds, and Mammals,—and by the terms *Urinary Organs*, I mean those both of a transient and a permanent nature—the Wolffian bodies of most embryos, as well as the true kidneys of all adults.

I have lately enjoyed excellent opportunities for the study of the development and intimate structure of these glandular organs; and their apparent peculiarities, as observed in the four different classes, require the detail I have given, in order to convey a clear idea of what may be called the *formula* of their organization. I say formula, for the intimate organic structure of a urinary organ wherever observed among the Vertebrata, is invariably the same; the variations being only apparent and extrinsic, and due to the many modes of combination of a single individual structure.

As before mentioned, the Renal organs appear under two forms, viz.: the Embryonic, or the so-called Wolffian Bodies, and the Permanent or true Kidneys.

I. *Wolffian Bodies.*

To the physiologist it is a beautiful and suggestive fact, not unfrequently observed in embryological studies, that nature sometimes puts up a temporary, provisional structure, for the performance of an important function, until the conditions of the general organization shall be so far advanced that there can be formed a permanent organ of a certain type, belonging to the animal as such, and which persists through its entire life. Such a fact is presented by the Wolffian bodies.

In the higher forms of organization, the blood, directly upon its active circulation, seems to require some means for the removal from it of certain effete particles, and to effect this a delicate, transient structure is erected, to remain only until a permanent one in the shape of a true kidney can be formed.

These temporary kidneys are found in the embryos of all the Vertebrata, excepting the Fishes and the Amphibian Reptiles; that is, in the true Reptiles, the Birds, and the Mammals. The length of time they persist is, in general, in an inverse ratio to the grade of the animal; in fact, this law of gradation seems so marked, that there would at first seem some ground for the opinion that in the Amphibia and the Fishes, which have only per-

manent renal organs, these last may be only persistent Wolffian bodies, but this point will receive our attention at a future time.

Wherever occurring, these Wolffian bodies present the same unvarying form and type of structure; their mode of development I have found to be equally invariable, whether occurring in Reptiles, Birds, or Mammals. They always make their appearance during the very earliest phases of development of the embryo, and, with the exception of the heart, are the first organs formed in the abdominal cavity.

As I have studied these phases of development in Birds more carefully than in the other classes, I will describe them as observed in the chick; and this description will include what belongs to these organs in Reptiles and Mammalia, in all their essential details.

In the Chick, there appears, at about the fiftieth (50th) hour of incubation, a line on each side of, and lying close to, the vertebral column; this line extends from near the region of the heart to the caudal vertebræ, and is composed of a collection of nucleated cells which soon become arranged so as to form a tube; this tube is the basis of the future Wolffian body. At this stage of development there is observed, then, a simple tube on each side of the vertebral column; but a few hours after, the surface of the tube becomes nodulated at regular intervals on its inner surface. These nodulations are the beginnings of a series of eversions from the main tube which soon therefore has a digitated appearance—each of the finger-like prolongations being a future uriniferous tube. These prolongations having been formed from without inwards, the original tube, which now has become a common duct, lies upon the external side, and the former overlap the vertebral column.

This formation of tubes by deverticula from a main one, constitutes the first phase in the development of this organ as a compound structure. The second phase is the formation of a direct connection between the blood-vessels and these newly-formed tubes, so that an eliminating function may be performed. This occurs at about the sixtieth (60th) or sixty-fifth (65th) hour of incubation. At this time, the free extremities of some, but not all, of the newly-formed tubes become enlarged and dilated into an infundibuliform body which, together with its attached tube, resembles a flask with a very long neck. *In* this dilated extremity of the tube, a knot of blood-vessels is formed anew from epithelial cells; this occurs in a manner so beautiful that I digress here for its more careful description.

This infundibuliform or bulged end of the tube is nothing but the tube at this point taking on a little larger growth; it is therefore lined, like the tube itself, with a layer of epithelial cells. These cells, by a more or less linear arrangement, form a minute

convoluted tube enclosing in its calibre smaller cells; this tube is the future blood-vessel which connects afterwards with the blood-vessels of the general circulation, and then the enclosed smaller epithelial cells become blood-corpuscles. This convoluted blood-vessel is the so-called Malpighian tuft, or the Glomerulus, and is the functional or active structure of the organ.

A brief but comprehensive description of the structure of the Wolffian body would then be: a straight, main duct into which empty many digitiform tubes, the capsular dilatations of the free ends of which contain each a knot of blood-vessels. In the same way, I might describe the method of its function, as: the straining off from the blood, through this knot of vessels, those effete particles which as a whole form the urinary excretion of the embryo. A structure more simple cannot easily be conceived of, yet its function is most effectual, for although thus quickly formed, we shall soon learn that both the structure and function of the complex permanent kidney rest upon precisely the same primitive types.

These temporary kidneys, thus formed, which have, as before remarked, exactly the same structure wherever found, have a duration varying in the different classes, which stands in an inverse ratio to the grade of the animal.

In the true Reptiles, and in Birds, they persist as active functional organs during a considerable portion of the embryonic life; in fact, their disappearance coincides with the appearance of the kidney as an active structure. Their structural remains, however, are often found long after the animal has passed out of its dependent, vitelline life; thus, in the chick the kidneys assume the urinary excretion at about the tenth (10th) day, yet the vestiges of those bodies may often be observed after hatching; and in the Alligator, I have seen, even four or five months after the animal had escaped the egg, the remains of these organs so well preserved that the Malpighian bodies were distinct in them. But in the Mammalia, where their existence as active parts is very brief, in fact so limited that it is difficult to observe them in a state of functional activity, they are correspondingly soon absorbed, and although these remains are observed more or less distinctly after birth in some species, yet, generally, they have mostly passed away by the latter half of the intra-uterine life.*

* *Müller*, (Physiologie, transl. by Jourdan, &c., Deux. éd. Paris, 1851, ii, p. 760, fig. 290. C. 5.) has given a figure representing the remains of the Wolffian body in a human fœtus of 3½ inches. As is well known, there may be observed during the last months of the human fœtus, or even after birth, peculiar canaliculi situated in the fold of the peritoneum and of the Fallopian tube. They form the so-called organ of *Rosenmüller* (De ovariis embryonum, Leipzig, 1801), and are probably the remains of the Wolffian body. In the Ruminantia, Solipeda, and Suina, there are likewise canaliculi (the canals of *Gærtner*), with thin walls, which extend from the large ligament of the uterus along the neck of this organ, pass between the mucous

As I have shown elsewhere,* the receptacle of this urinary excretion of the Wolffian bodies is the Allantois, which, as I have described in the paper referred to, is at first, properly nothing but their receptacular appendix—the excretory ducts terminating in it below.

In conclusion, I may remark that the Wolffian bodies are transient in all their relations; they subserve nothing for the formation of the permanent organs that succeed them—being every way distinct structures; and the testicles or ovaries which are first observed on their bodies, have no other relation with the part on which they rest except that of mere contact.

But before leaving this section of the subject, I think it necessary to explain one point which otherwise might seem assumed on too little authority. I refer to the statement I have already made that the Wolffian Bodies are absent in the Amphibian Reptiles. This, as is well known to physiologists, is directly opposed to the views of Müller, and therefore demands here a special reference.

Among Müller's earliest anatomical publications, upon the formation of glandular structures and particularly those of the genital apparatus, he pointed out and gave a careful description† of what he regarded as a new structure in the embryos of Amphibia. Up to this time, the so-called Wolffian bodies had not been observed by Meckel, and Rathke, who had specially studied them, in either the Fishes or the Amphibian Reptiles,—and these newly-discovered structures Müller regarded as a peculiar form of the Wolffian bodies. He described them as two organs situated, one on each side of the vertebral column, directly under the branchiæ of the larvæ of Batrachians, and from which proceeds a duct that runs along the side of the vertebral column and opens, finally, into the lower portion of the intestinal canal.

But the presence of these organs, thus carefully described and especially by so excellent an authority, has not been acceded to by all, although by most, subsequent microscopical anatomists;‡

membrane and the muscular tunic of the vagina, and open, at last, near the urethral orifice. These, *Jacobson* (Die Oken'schen Körper oder die primordial Nieren, Kopenhagen, 1830, p. 17) thinks are the remains of these same bodies; a view regarded as probable likewise by *Rathke*, *Valentin*, and *Gurlt*, but which certainly requires new proofs. See, in this connection, *Rathke*, in *Meckel's* Arch., 1832, p. 386; *Valentin*, Handbuch der Entwickelungsgeschichte, p. 393; *Gurlt*, Vergleich. Anat. &c., ii, p 115; and *Bischoff*, Entwickelungsgeschichte, &c., *Jourdan's* edit., p. 348.

* See a paper, *On the formation and function of the Allantois*, in the Proceed. Amer. Acad. of Arts and Sciences, Boston, for October, 1852.

† *J. Müller*, Bildungsgeschichte der Genitalien, Dusseldorff, 1830. See also his De Glandularum secernentium structura penitiori earumque prima formatione, 1830, p. 86, Tab. xii, and his Physiologie, transl. by *Jourdan*, &c., Deux. éd. Paris, 1851, ii, p. 758.

‡ Among those who have followed *Müller*, may be mentioned *H. Meckel*, (Zur Morphologie die Harn—und Geschlechtswerkzeuge der Wirbelthiere, Halle, 1848,) and *Reichert*, (Das Entwickelungsleben im Wirbelthier-Reich, 1840, p. 26).

at least that they are distinct organs as independent of the permanent kidneys or as the ordinary Wolffian bodies of Birds and Mammals.

This is a point to which I gave special attention in making these investigations, and after the most careful and repeated examinations of the larvæ of Batrachia, in all their stages, I have wholly failed to find a structure, such as Müller has described, distinct from the developing kidney, and which would correspond to the ordinary Wolffian body. On the other hand, all that I have observed in this respect is, that the common duct (future ureter) of the forming kidney extends quite high up towards the cardiac region, some distance beyond the upper limit of its branching out into uriniferous canals. The upper end of this free portion of the duct is convoluted and seems to have some direct connection with the blood-vessels, though I have never here observed any thing like Malpighian bodies. As the development of the kidney progresses, this free extremity of the tube gradually disappears, and towards the end of the larval state is observed only in remains. In brief, then, I have observed no distinct, temporary urinary organs in the undeveloped forms of Amphibia.

These observations, made for the most part during the summer of 1852, though repeated from time to time until quite recently, I was pleased to perceive confirmed by so excellent an observer as Wittich in a paper published in the autumn of the same year.* Wittich's investigations have been very extended, and as far at least as they relate to the renal organs, furnish results much like my own. He found nothing in the larvæ of the naked Amphibia which would correspond to Müller's Wolffian bodies, excepting the free prolongation upwards of the main duct or the ureter of the developing kidney.

The doctrine here insisted upon that the true Wolffian bodies, or fœtal kidneys, are present only in the embryos of the true Reptiles, the Birds and the Mammals, is so apposite with the results I have obtained from an investigation of the Allantois, that it may be urged with an added force. In the paper already alluded to, I have sought to show that the Allantois is, primitively, the vesicular expansion of the combined extremities of the ducts of the Wolffian bodies, and finally becomes the receptacular appendage of these organs—its contained liquid being properly urine.† But afterwards it subserves also another function—that of the aeration of the blood.

* *Wittich*, Beiträge zur morphologischen und histologischen Entwickelung der Harn—und Geschlechtswerkzeuge der nackten Amphibien, in *Siebold* and *Kölliker's* Zeitsch, f. wissensch. Zool, IV, 1852, p. 125; also, Harn- und Geschlechtsorgane von Discoglossus pictus und einiger anderer aussereuropäischer Batrachier, ibid, p. 168.

† *Jacobson*, as is well known, detected urea in the liquid contents of the Allantois of very young embryos. See loc. cit.

This view being correct, we could expect to find Wolffian bodies only where there is observed an Allantois, and *vice versa.* Thus, in the Fishes and Amphibian Reptiles, which truly have no Allantois, there would be no temporary kidneys or the Wolffian bodies; while in the other Vertebrata, which are allantoïdian, these last would be found. I scarcely need refer further to this parallelism as far as regards our doctrine in question.*

II. *Permanent or true Kidneys.*

These organs being apparently indispensable to the adult economy of all the Vertebrata, have a physiological importance of the highest character. Although the primitive essential type of their structure is precisely like that of their temporary analogues just described, when present, yet as they are permanent organs and sustain definite relations and connections with various parts of the general system, the study of their structure as organs is the more complex and interesting. It would be foreign to my subject to enter upon the details of the comparative Anatomy of these organs, describing those variations of external form belonging to each type;† I shall limit myself to those points that bear directly upon the subject of development and intimate structure.

As a leading fact I may mention that the idea or *formula* of the kidney, wherever observed in the vertebrate, is always the same; and thus finding the intimate structure essentially alike everywhere, we should naturally infer that there is throughout a single and invariable mode of development. This indeed, I have found to be the case, as I hope to show in the following details.

In the Fishes and Amphibian Reptiles, where there are no temporary urinary organs, the earlier phases of development of the kidneys seem to be a little less complicated than in the higher classes. In fact, the type of structural development here is quite allied to that of the Wolffian bodies, and by some anatomists

* I much regret that in preparing this account of the Wolffian bodies, I should not have had direct access to the work of *Follin*, (Recherches sur les corps de Wolff, Paris, 1850), which I have failed to get on the reputed ground of its being out of print. Considerable reference, however, is made to this work, by the copying of some of its figures, in *Littré's* edition of *Jourdan's* translation of *Müller's* Physiologie, Paris, 1851, ii, p. 761. But by the figures in question, I can judge nothing of the views of *M. Follin* upon this structure.

Besides the well known works of *Rathke, Meckel, Baer, Jacobson, Valentin, Bischoff*, already referred to, see especially for representations of these bodies, *Müller*, Bildungsgeschichte der Genitalien; his De Glandularum secernent. &c.; and his Physiologie, Transl. by *Jourdan*, Paris, 1851, ii, p. 757. For figures of the so-called Wolffian bodies of Amphibia, see *Wittich*, loc. cit., Taf. ix, figs. 1, 2, 3, 4, 5, 6, &c.

The general structure of these bodies has long been known, and I am not aware that I have added any new facts in this respect. My observations relate chiefly to the very first stages of formation of all the parts, and concerning this, have furnished results I have seen recorded by no investigator.

† For full details on this subject, with a copious reference to its literature, see the Comparative Anatomy of *Siebold* and *Stannius*, transl. &c. by *Burnett*, ii, §§ 49, 106, 153, 260.

these organs in Fishes have been regarded as merely permanent forms of the above mentioned bodies. This last, however, I do not think correct, for in this class the subsequent phases of development by which the organ is increased in size, are exactly like those of the general formation of these organs in Birds and Mammals.

The formula of development is the branching out indefinitely of a primitive tube which is the future ureter;—the terminal portions of these branches forming an intimate connection with the general vascular system, either by Malpighian bodies, or by a delicate net-work of vessels.

The mode of formation is therefore arborescent, and the analogy of the development and growth of this gland with that of vegetable forms is too striking not to be noticed. We shall see that the growth exactly resembles that of a tree even almost in its details.

The line of development being everywhere the same, I will, for the sake of uniformity, describe it as occurring in the Chick, where, moreover, I have carefully registered the successive phases.*

In the chick no traces of the kidney are perceptible, according to my own observations, until the end of the fourth or the beginning of the fifth day. The ureter, then, is the part first seen, and consists of a simple tube, the upper part of which sends off branches. Each of these branches then divides and subdivides,

* Here I may perhaps well allude to the views of other observers who have specially studied the development of the kidney. The authors best known in this respect are *Rathke*, *Müller*, *Valentin*, *Bischoff*, as well also as *Wittich* of later times. The works of these men have already been cited. According to *Rathke*, the first traces of the kidney consist of a number of small claviform protuberances appearing in an amorphous blastema situated in the place of the future kidney. These he says are pediculated, and these pedicles ultimately become attached to the ureter, which at this time is not yet formed. In these claviform bodies the uriniferous tubes subsequently appear, but in a manner not precisely determined.

Valentin's investigations agree in general with those of *Rathke*, but, from some observations made upon very small embryos of the hog, he concluded that the ureter, pelvis, and uriniferous tubes were primitively developed separately, being subsequently united. *Valentin's* view of the formation of the uriniferous tubes coincides with that of *Henle*, viz.: that it occurs by the condensation of a linear arrangement of the vesicular contents of the blastema.

Both *Müller* and *Bischoff* confirm, in general, the views of *Rathke;* but *Bischoff* differs not a little upon some points which deserve mention. He says, "I have never been able to convince myself that the ureter, pelvis, and canaliculi are developed separately, and I think that their rudiments are continuous throughout. They are not hollow at first, and their internal cavity is the result only of further development."

But of all, *Wittich's* results most closely approach my own. Speaking of these organs in the Amphibia, he says: "The further growth of the kidney occurs, it would appear, partly by a new deverticulation of the excretory duct (ureter), and partly by a widening, lengthening and branching of the primitive deverticula."

As will be seen in the text, the view I advance is the arborescent mode of development, more or less marked according to the class, of these organs—the formula being a more or less extended deverticulation of tubes from a main tube or duct.

and around these subdivisions are collected a mass of blastematous cells. These cells serve as the material for the growth and further division of the tubes—in fact for their ultimate ramification, until the whole structure is completed. There is observed, therefore, at an early period, a main stem (ureter) which has many branches, each of which is the foot-stalk of a leaf-like body; this last is one of the future lobules of the kidney. The whole plan of structure is here laid out—a branching ureter with lobules. The increase of the size of these lobules, and the formation of the ultimate uriniferous tubes takes place by one and the same process, viz.: by the branching of the original tubes in a plumate form from a main tube,—the whole resembling the plume of a feather. The tubes do not end, however, on the edge of the plume (so to speak), but here loop and return, and when near the shaft or main tube, they dilate into Malpighian bodies; this is, as far as I have observed, their invariable mode of termination, there being no anastomoses of the tubes as some have supposed.

The mode of formation of the Malpighian body of the true kidney, is precisely like that of the same structure belonging to the Wolffian body, already described; I need not therefore here repeat the description. I may remark, however, that the two blood vessels which compose the glomerulus, usually penetrate the capsular dilation of the tube at some point more or less near the opposite one of its insertion upon the tube, and rarely more laterally, or at least on the inner half of the capsule. In the chick, the Malpighian bodies begin to appear at about the tenth day; they are then very few in number, but they become more numerous exactly in proportion to the growth of the organ—not ceasing to be formed until the kidney has reached its full size.

Nothing could be simpler therefore than the mode of formation of the renal structure; it is clear and unmistakable—one tube gives rise to another by an eversion of its walls, and this last produces another in the same way, and so on.

Such is the mode of development as occurring in Birds, and my observations upon it as found in all the other classes, show that the formula is there invariably the same. There are, however, peculiarities of the combination of this formula in each class, which demand a special consideration.

In Fishes, these organs appear at an early period as two straight tubes, extending, one on each side of the vertebral column, from the region of the heart to the anus. These tubes soon give off from their inferior and inner surface, *deverticula*, exactly as with the Wolffian bodies just described, and for a time the organs have much the same appearance as embryonic kidneys. But afterwards, as the animal increases in size, a new development takes place. This consists in the branching, dichotomously, of the deverticular tubes,—a process which goes on indefinitely, giving

the whole organ much the same lobulated, arborescent structure as that of the chick—although the plumate arrangement of the terminal tubes is not here present. In both the Cartilaginous and the Osseous Fishes, the functional or secreting structure is, as far as I have observed, always the same; that is, the tubes invariably terminate each in a well-formed Malpighian body.

In the Amphibian Reptiles, the kidneys quite closely resemble those of Fishes, yet simulating even more closely than these, the general character of the Wolffian bodies. They present little tendency to an aborescent structure, the tubes of the main duct rarely dividing but rather forming convolutions, and ending invariably, according to my own observation, in Malpighian bodies.* These Malpighian bodies, together with their adjacent portion of the uriniferous tube, are lined with long lash-like cilia, the ever constant, rapid action of which, waving towards the outlet, presents a beautiful aspect. The use of these cilia is, evidently, to direct the course of the current out of the Malpighian body, therby keeping this last in a free, unobstructed state. In the true Reptiles, the development of the kidney is arborescent as in Birds, and in the Serpents the lobules thus formed remain more or less separate through life. This is also the case, but in a less degree, in the Chelonioidæ.

But the Ophidioidæ present this peculiarity, that all the uriniferous tubes do not end with a Malpighian body; in fact, I have been able to find but few of these bodies which are usually situated on the posterior portion of the organ. Many of the uriniferous tubes terminate in a simple cæcal extremity, and here the ultimate connection with the vascular system by means of a Malpighian body, is replaced by a large system of renal vessels, the indefinite ramifications of which permeate the whole organ and by very delicate anastomoses bring every such cæcal secreting tube within the influence of a blood vessel.†

In the Chelonioidæ, the kidneys quite closely resemble, in every point of view, those of Birds. The uriniferous tubes have the same form of distribution and terminate always, each, in a Malpighian body.

As for these organs in the Birds, I have described their peculiarities in speaking of the chick, and have only to add that throughout the entire class there seems but little or no variation.

In Mammalia, the course of development followed is like that observed in Birds, but the subsequent changes by which the organ becomes more compact, often entirely conceal the original forms. By observations upon very young embryos in the different fami-

* For representations of the intimate structure of the kidneys of Amphibia, see, especially, *Wittich*, loc. cit. Taf. ix.

† There is, as I have lately observed, a similar relation between the blood-vessels and the terminal cæca of the poison-gland of the Rattle-snake (*Crotalus*—a kind of *Rete mirabile.*

lies, it is evident that the development is arborescent, forming lobules,—and these lobules are at first exactly like those observed in Serpents and Birds. In some species, as is well known, these lobules remain distinct through life; this is well seen in the otter, bear, and whale, and in many of the Ruminantia, these remains are distinctly visible. It is only in the higher forms, that they have so coalesced as to be concealed.

This intimate combination of parts produces the greatest amount of secreting surface in the smallest space. Take, for instance, the kidney of Man; here the lobules are arranged in a half-circle around a common cavity or the pelvis. But they are so united as to become conical in shape (with the bases of the cones at the surface of the organ) thereby producing the so-called pyramids. These pyramids are composed in part of tubes that spread out in a fan-like manner from a common point (calyx), and the gradually increased size of the medullary portion is produced by the branching of the tubes in a dichotomous manner. The straight regular way in which these tubes run, seems to be due, in a measure at least, to the mechanical pressure to which they are subjected by the combination of the lobules,—for I have been unable to perceive it in very young embryos, and, moreover, it does not agree with the invariable arborescent conditions of early formation. The so-called Medullary portion of the kidney is made up of fasciculi of straight tubes which divide dichotomously, but have no Malpighian bodies; they continue directly to the Cortical portion which is composed of more or less convoluted tubes—the result of the dichotomous division. These tubes finally end, each, in a Malpighian body. These Malpighian bodies sometimes lie upon the surface, directly beneath the capsule, but often also the tubes run up to the surface, loop, return a short distance, and end in a Malpighian body deeper in the renal structure. From all I have observed, it appears that in Man the Malpighian body is the *only* termination of the final tubes, there being therefore as many of the former as of the latter. I have seen nothing like an anastomosis of the final tubes, as some have supposed is sometimes the case. The pelvis of the human kidney, as also that of other mammals, is formed by the dilatation of the main duct, or ureter, involving the primary branches which give off the straight tubes of the medullary substance. In this way, and by the union of the bases of the pyramids, the apices are left free, projecting into this pelvis or main cavity. These changes I have enjoyed an opportunity to trace in an embryo.

It will be seen, therefore, that the development of the kidney in Man and the higher mammals involves no new phases,—the differences of general structure being extrinsic and due to combinations which produce compactness and decreased size without a corresponding decrease of functional surface.

In the foregoing account, I have preferred not to burden the general text with a reference to the history and criticism of some of the most important as well as disputed points connected with this subject. These can better be discussed by themselves.

The most important of these points is the character, relations, and connections of the Malpighian body. This secreting organ of the kidney had long been recognized by both the older and the more modern observers, but its intimate structure and its connections with the other parts of the renal substance as the functional secreting organ, were first successfully made out and published by Bowman* in a memoir which has since become classic on this subject. I may here mention that it is quite a remarkable historical fact that more than fifty years before, Schumlasky† expressed the view that these bodies were the source of the urinary secretion and had direct connection with the uriniferous tubes. But this view was opposed by subsequent observers. In 1841, Müller‡ published a portion of his work on the anatomy of the Myxinoid Fishes, where he describes the kidney as consisting of sac-like deverticula from a main tube or ureter, terminating cæcally, and in the end of which is a small tuft or knot of blood-vessels. The value of this discovery was not appreciated until Bowman (ignorant, however, at this time of Müller's investigations on this point) published the next year his memoir. To Bowman, therefore, we seem indebted for the first full exposition of the secreting structure of the kidneys of Vertebrata.

Bowman's doctrine was, that the Malpighian body is the infundibuliform expansion of the uriniferous tube, and that the glomerulus or tuft of blood-vessels lies enclosed freely in this expansion, being composed of a tortuous loop, the two component vessels entering at the same point which is generally opposite or nearly so to the point of insertion of the uriniferous tube. The Malpighian body thus composed, Bowman maintained, is the exclusively secreting structure of the kidney. These are the essential features of the results advanced, and I need not enter into the details of a memoir so well known as this.

Results so valuable in physiology, were, of couse, examined by different investigators on every side. Reichert,§ in his report upon the progress of Microscpical Anatomy for 1842, enters into a critical discussion of this subject. He denies that the uriniferous tubes end in the capsule, and regards this last as a distinct and separate formation. He also denies the presence of ciliated epithelium in the Malpighian body, and the uriniferous tubes.

* *Bowman.* Philosophical Transactions, London, 1842, Pt. i, p. 57.
† *Schumlasky.* De structura renum. Strasburg, 1788.
‡ *Müller.* Vergleich. Anat. d. Myxinoiden, Dritte Fortsetzung. Berlin, 1841. p. 13.
§ *Reichert,* Bericht über die Fortschritte der mikroskopischen Anatomie in dem Jahre, 1842, in *Müller's* Arch., 1843, p. ccxvii.

On the other hand, he maintains that the glomerulus or knot of blood-vessels is enclosed in the Malpighian body. These views he has defended in some of his subsequent reports.*

In 1845, Gerlach† published a still different opinion upon this disputed point of the Malpighian body. He maintained that the uriniferous tubes do not end in flask-shaped cæca, but loop and pass into each other. The Malpighian bodies he declares to be globular deverticula from the tubes, and that they contain, as Bowman advocates, the glomerulus. This author has likewise reasserted his views in a subsequent paper.‡

The view of Bidder§ is even still different. According to this observer, the uriniferous tubes end in flask-shaped cæca, but these last do not enclose the glomeruli. On the contrary, the Malpighian bodies receive each a glomerulus in a kind of depression, and so the cavity of the Malpighian body is as disconnected from the glomerulus as is the cavity of the pleura from the lung.

These, as far as I am aware, are all of the dissimilar views maintained by recent observers. It is indeed remarkable that there should have occurred such a discrepancy. Most other investigators‖ of note, follow more or less completely the views of Bowman. Among these may be mentioned Kölliker,¶ Patruban,** Nicolucci,†† Maudl,‡‡ Victor Carus,§§ and Wittich.‖‖

As to my own views, they will appear sufficiently plain in the foregoing pages. I subscribe to Bowman's results very generally, as to the relations of the Malpighian body to the tubes and to the glomerulus. In tracing the successive phases of the development of these parts, this point enlisted my special attention from its disputed character. The Wolffian bodies are the best structures for study in reference to this point, and, having traced in these the budding out, as it were, of the uriniferous tubes,—their dilatation, at the extremity, into a flask-shaped body, and the formation, in this last, of the Malpighian tuft or glomerulus,—I can have no doubt that all these connections and relations really exist.

* *Reichert*, see Bericht, &c. for 1848, in *Müller's* Arch., 1849, p. 65; also that for 1849 in ibid, 1850, p. 67.

† *Gerlach*, Beiträge zur Structurlehre der Niere, in *Müller's* Arch., 1845, p. 378.

‡ *Gerlach*, Zur Anatomie der Niere, in *Müller's* Arch., 1848, p. 103.

§ *Bidder*, Ueber die Malpighischen Körper der Niere, in *Müller's* Arch., 1845, p. 508.

‖ *Hyrtl*, however, does not follow *Bowman*, but rather *Gerlach*, that is, that the uriniferous tube does not end in the Malpighian body. See Beiträge zur Physiologie der Harnsekretion, in the Zeitschrift der Gesellschaft der Aerzte zu Wien, ii, p. 381.

¶ *Kölliker*, Ueber Flimmerbegungen in den Primordialnieren, in *Müller's* Arch. 1845. p. 518.

** *Patruban*, Beiträge zur Anatomie der menschlechen Niere, in the Prager Vierteljahrsschrift. xv, p. 87.

†† *Nicolucci*, Sull' intima struttura dei reni in Filiatre-Sebezio. Feb., 1847.

‡‡ *Mandl*, Mémoire sur la structure intime des Organes urinaires, p. 18.

§§ *Victor Carus*, Ueber die Malpighi'schen Körper der Niere, in *Siebold* and *Kölliker's* Zeitsch. f. wissenschaftl. Zool., ii, 1850, p. 58.

‖‖ *Wittich*, Beiträge zur Anatomie der gesunden und kranken Niere, in *Virchow* and *Reinhard's* Arch., iii, p. 147.

ART. XLI.—*The Numerical Relation between the Atomic Weights, with some Thoughts on the Classification of the Chemical Elements;* by JOSIAH P. COOKE, Jr., A.M., Erving Professor of Chemistry in Harvard University.*

NUMERICAL relations between the atomic weights of the chemical elements have been very frequently noticed by chemists. One of the fullest expositions of these relations was that given by M. Dumas of Paris, before the British Association for the Advancement of Science, at the meeting of 1851. This distinguished chemist at that time pointed out the fact, that many of the elements might be grouped in triads, in which the atomic weight of one was the arithmetical mean of those of the other two. Thus the atomic weight of bromine is the mean between those of chlorine and iodine; that of selenium is the mean between those of sulphur and tellurium, and that of sodium, the mean between those of lithium and potassium. M. Dumas also spoke of the remarkable analogies between the properties of the members of these triads, comparing them with similar analogies observed in organic chemistry, and drew, as is well known, from these facts arguments to support the hypothesis of the compound nature of many of the now received elements. Similar views to those of Dumas have been advanced by other chemists.

The doctrine of triads is, however, as I hope to be able to show in the present memoir, a partial view of this subject, since these triads are only parts of series similar in all respects to the series of homologues of organic chemistry, in which the differences between the atomic weights of the members is a multiple of some whole number. All the elements may be classified into six series, in each of which this number is different, and may be said to characterize its series. In the first it is nine, in the second eight, in the third six, in the fourth five, in the fifth four, and in the last three. The discovery of this simple numerical relation, which includes all others that have ever been noticed, was the result of a classification of the chemical elements made for the purpose of exhibiting their analogies in the lecture-room. A short notice of this classification will, therefore, make a natural introduction to the subject.

Every teacher of chemistry must have felt the want of some system of classification like those which so greatly facilitate the acquisition of the natural-history sciences. In most elementary text-books on chemistry, the elements are grouped together with little regard to their analogies. Oxygen, hydrogen, and nitrogen are usually placed first, and therefore together, although there are hardly to be found three elements more dissimilar; again, phos-

* Communicated to the American Academy, Boston, Feb. 28, 1854.

phorus and sulphur, which are not chemically allied, are frequently placed consectively, while arsenic, antimony, and bismuth in spite of their close analogies with phosphorus, are described in a different part of the book. This confusion, which arises in part from retaining the artificial classification of the elements into metals and metalloids, is a source of great difficulty to the learner, since it obliges him to retain in his memory a large number of apparently disconnected facts. In order to meet this difficulty, a classification of the elements into six groups, differing but slightly from that given in the table accompanying this memoir, was made. The object of the classification was simply to facilitate the acquisition of chemistry, by bringing together such elements as were allied in their chemical relations considered collectively. As the classification has been in use for some time in the courses of lectures on chemistry given in Harvard University, I have had an opportunity for observing its value in teaching, and cannot but feel that the object for which it was made has been in a great measure attained. The series which is headed the Six Series will illustrate the advantage gained from the classification in a course of lectures, the elements which compose it being among those especially dwelt upon in lectures to medical students, and, generally, very widely separated in a text-book on the science. As chemistry is usually taught, the properties of the members of this series, nitrogen, phosphorus, arsenic, and antimony, as well as the composition and properties of their compounds, make up a large body of isolated facts, which, though without any assistance for his memory, the student is expected to retain. Certainly it cannot be wondered at, that he finds this a difficult task. The difficulty can, however, be in a great measure removed, if, after he has been taught that nitrogen forms two important acids with oxygen, NO_3 and NO_5, that it unites with sulphur and chlorine to form NS_3 and NCl_3, and also with three equivalents of hydrogen to form NH_3, he is also told, that, if in these symbols of the nitrogen compounds he replaces N by P, As, or Sb, he will obtain symbols of similar compounds of phosphorus, arsenic, and antimony; for he thus learns, once for all, the mode of combination of all four elements, so that when he comes to study the properties, in turn, of phosphorus, arsenic, and antimony, he has not to learn with each an entirely new set of facts, but finds the same repeated with only a few variations. Moreover, these very variations he will learn to predict, if he is shown that the elements are arranged in the series according to the strength of their electro-negative properties, or, in other words, that their affinities for oxygen, chlorine, sulphur, etc. increase, while those for hydrogen decrease, as we descend. He will then readily see why it is that, though nitrogen forms NO_3 and NO_5, it forms only NCl_3 and NS_3, and that this reason is correct he will be pleased to find

confirmed when he learns that phosphorus, which is more electro-positive than nitrogen, and has, therefore, a stronger affinity both for chlorine and sulphur, forms not only PCl_3 and PS_3, but also PCl_5 and PS_5. Again, he will not be surprised, after seeing the affinity of the elements for hydrogen growing constantly weaker as he descends in the series, to learn that a compound of bismuth and hydrogen is not certainly known. Should he inquire why, though NH_3 has basic properties, PH_3, AsH_3, and SbH_3 have not, he can be shown that the loss of basic properties in passing from NH_3 to PH_3 corresponds to a decrease in the strength of the affinity between the elements, and that if in PH_3, SbH_3, or AsH_3, atoms of methyle, ethyle, or other organic radicals analogous to hydrogen, are substituted for the hydrogen atoms, and more stable compounds thus obtained, strong bases are the result. The other series would afford similar illustrations, and, from my own experience, I am confident that no teacher who has once used a classification of the elements like that here proposed, would ever think of attempting to teach chemistry without its aid.

Classifications of the elements, more or less complete, have been given by many authors; but the fact that no one has been generally received, is sufficient to prove that they are all liable to objections, and would, indeed, also seem to show that a strictly scientific classification is hardly possible in the present state of the science. The difficulty with most of the classifications is, undoubtedly, that they are too one-sided, based upon one set of properties to the exclusion of others, and often on seeming, rather than real resemblances. This is the difficulty with the old classification into metals and metalloids, which separated phosphorus and arsenic, sulphur and selenium, because arsenic and selenium have a metallic lustre, while phosphorus and sulphur have not, though there could hardly be found another point of difference. For a zoölogist to separate the ostrich from the class of birds because it cannot fly, would not be more absurd, than it is for a chemist to separate two essentially allied elements, because one has a metallic lustre and the other has not. Yet it is surprising to see how persistently this classification is retained in every elementary work on the science; and if it is sometimes so far modified as to transfer elements analogous to selenium and arsenic to the class of metalloids, this is only acknowledging the worthlessness of the principle, without being willing to abandon it. If there were any fundamental property common to all the elements, the law of whose variation was known, this might serve as the basis of a correct classification. Chemistry, however, does not as yet present us with such a property, and we must, therefore, here, as in other sciences, base our classification on general analogies. The most fundamental of all chemical properties is, un-

doubtedly, crystalline form; but a classification of the elements based solely on the principles of isomorphism is defective in the same way as it is in mineralogy. It brings together, undoubtedly, allied elements, but it also groups with them those which resemble each other only in their crystalline form. The mode of combining seems to be also a fundamental property; but, like crystalline form, it would bring together in some instances elements differing very widely in their chemical properties. A classification of the elements which shall exhibit their natural affinities, must obviously pay regard to both of these properties. It must at the same time seek to group together isomorphous elements, and those which form analogous compounds. Moreover, in such a classification, other less fundamental properties must not be disregarded. There are many properties both physical and chemical, which, although they cannot be exactly measured, and are oftentimes difficult to define, (such properties as those by which a chemist recognizes a familiar substance, or a mineralogist a familiar mineral,) and which on account of their indefinite character cannot be used as a basis of classification, may, nevertheless, render important aid in tracing out analogies. Judging from such properties as these, chemists are generally agreed in grouping together carbon, boron, and silicon, although they cannot be proved to be isomorphous, and are not generally thought to form similar compounds.

It is, however, much easier to point out what a classification should be, than to make one which shall fulfil the required conditions. Indeed, as has been already said, past experience would seem to show that a perfect scientific classification of the elements is hardly possible in the present state of chemistry. At best, the task is attended with great difficulties, and it cannot be expected that these should be surmounted at once. The classification which is offered in this memoir will, undoubtedly, be found to contain many defects. If, however, it is but one step in advance of those which have preceded it, it will be of value to the science. It was originally made, as has already been said, simply for the purpose of teaching, and never would have been published had it not led to the discovery of the numerical relation between the atomic weights.

On turning to the table which accompanies this memoir, it will be seen that the elements have been grouped into six series. These correspond entirely to the series of homologues of organic chemistry. In the group of volatile acids, homologues of formic acid, for example, we have a series of compounds yielding similar derivatives, and producing similar reactions, and many of whose properties, such as boiling and melting points, specific gravity, etc., vary as we descend in the series according to a determinate law. From formic acid, a highly limpid, volatile, and corrosive fluid,

the acids become less and less volatile, less and less fluid, less and less corrosive; first oily, then fat-like, and finally hard, brittle solids, like wax. As is well known, the composition of these acids varies in the same way, and the variation follows a regular law, so that by means of a general symbol we can express the composition of the class. This symbol for the volatile acids may be written $(C_2H)O_3, HO+n(C_2H_2)$.

This description of the well-known series of the volatile acids, applies, word for word, *nominibus mutatis*, to each of the six series of chemical elements. The elements of any one series form similar compounds and produce similar reactions; moreover, they resemble each other in another respect in which the members of the organic series do not. Their crystalline forms are the same, or, in other words, they are isomorphous. Although this may be true of the volatile acids, yet it cannot be proved in the present state of our knowledge. Still further, many of their properties vary in a regular manner as we descend in the series. In one case, at least, the law of the variation is known, and can be expressed algebraically, though in most instances it cannot be determined. Finally, as one general symbol will express the composition of a whole organic series, so a simple algebraic formula will express the atomic weight, or, if you may please so to term it, the constitution of a series of elements.

These points may be illustrated with any of the series in the table; with the first, for example, which consists of oxygen, fluorine, cyanogen, chlorine, bromine, and iodine. All these elements form similar compounds, as will be seen by inspecting the symbols of their compounds given at the right hand of the list of names, where the similar or homologous compounds are arranged in upright columns. Moreover, they are all isomorphous, as may be seen by referring to the left-hand side of the list, where the similar compounds in each upright series are isomorphous, the numbers at the heads of the columns indicating the systems of crystallization, as described in the explanation accompanying the table. That the properties of these elements vary as we descend, can be easily shown. Oxygen is a permanent gas, as is also fluorine. Cyanogen is a gas, but may be condensed to a liquid. Chlorine, a gas also, can be condensed more easily than cyanogen. Bromine is a fluid at the ordinary temperature; and, finally, iodine is a solid. Moreover, starting from cyanogen, the solubility of these elements in water decreases as we descend in the series; and, again, the specific gravity of their vapors follows the inverse order of progression, gradually increasing from oxygen down. The atomic weights vary in the same order, and admit of a general expression, which is $8+n9$, or, in other words, the differences between the atomic weights of these elements are always a multiple of nine. This general formula may be said to repre-

sent the constitution of these elements, in the same way that the symbol $(C_2H)O_3$, $HO + n(C_2H_2)$ represents the composition of the volatile acids before mentioned. In the place of $(C_2H)O_3$, HO we have $8=O=$ the weight of one atom of oxygen, and in the place of C_2H_2 we have nine. What it is that weighs nine (for it must be remembered that those numbers are weights) we cannot at present say, but it is not impossible that this will be hereafter discovered. In order to bring the general symbol of the volatile acids into exact comparison with that of the Nine Series, we must reduce the symbols to weights, when the two formulæ become

$$46+n14, \quad \text{where } 46=(C_2H)O_3,HO \quad \text{and } 14=C_2H_2;$$
$$\text{and } 8+n\,9, \quad \text{where } 8=O \quad \text{and } 9=x.$$

The numbers 46 and 14 are known to represent the weights of aggregations of atoms. The number 8 represents the weight of one oxygen atom, but we cannot as yet say what the 9 represents. After this comparison, it does not seem bold theorizing to suppose that the atoms of the members of this series are formed of an atom of oxygen as a nucleus, to which have been added one or more groups of atoms, the weight of which equals nine, or perhaps one or more single atoms each weighing nine, to which the corresponding element has not yet been discovered. As it will be convenient to have names to denote the two terms of the formulæ which represent the constitution of the different series, we will call the first term, in accordance with this theory, the nucleus, and the number in the second term multiplied by n the common difference of the series.

From what has been said, it will be seen that the idea of the classification is that of the organic series. It is in this that the classification differs from those which have preceded it. Other authors, in grouping together the elements according to the principles of isomorphism, have obtained groups very similar to those here presented. Indeed, this could not be otherwise, since, as has been already said, the members of each series are isomorphous, while, as a general rule, to which, however, there are many exceptions, no isomorphism can be established between members of different series. These groups, however, have been merely groups of isomorphous elements, and not series of homologues like those in which the elements are here classed.

These general remarks will suffice to indicate the principles upon which the classification has been made, and the character of the numerical relation between the atomic weights which has been established. The details of the classification can be best studied by referring to the table, so that it will be only necessary to speak of those points which are of special interest, or which may require explanation, or in regard to which there may be doubt. The series I have named from their common differences.

The first I have called the Nine Series, the second the Eight Series, &c. Let us examine the doubtful points in each, commencing with the first.

The last five members of the Nine Series are connected by so many analogies, that they have been invariably grouped together in the elementary books. There can be no doubt, therefore, in regard to the propriety of placing them in the same series, on the ground of general analogies. Fluorine, it is true, presents some striking points of difference from the rest. Fluorid of calcium is almost insoluble in water, while the chlorid, bromid, and iodid of calcium are all very soluble. We must, however, remember that we have to do with series, and must not therefore expect to find close resemblances except between adjacent members. If, then, we consider that oxygen is one of the series, and that fluorine stands but one step removed from oxygen, while it is two steps removed from chlorine, the discrepancy in a measure vanishes, for lime CaO is but slightly soluble in water. Nevertheless, the difficulty does not entirely disappear; for CaFl is much less soluble than CaO, although it should be more soluble judging from the law of the series and the fact that CaCl is so much more soluble than CaFl.

The solubility of a series of homologous elements or compounds in water, may be regarded as a function of one or more variables. In the case of the elements there may be but one variable, but it is easy to see that in the case of compounds there must be several. One of these variables is probably the same which determines the common difference of the series to which the elements or compounds belong; (it will be hereafter shown that the atomic weights of the homologous compounds are related in the same way as those of the elements;) the other variables are perhaps the atomic forces which determine the hardness, density, &c. of the solid. We may, therefore, with justice, compare the relative solubilities of a series of homologues to a curve which should be the same function of the same variables, and what mathematics teaches we ought reasonably to expect in the case of this curve, we ought to expect also in the variations of solubility of these substances. Now every mathematician is familiar with the remarkably rapid changes which a curve undergoes that is a function of several variables, and we cannot be surprised that similarly rapid changes should be observed in the solubility of homologous substances in passing from one to the next in the series. In the curve which corresponds to the relative solubility of CaO, CaFl, CaCy, CaCl, CaBr, and CaI, it would seem that at CaFl there is a singular point where the curve, after rising for some distance above the axis, bends down again towards it. Several of the other series of compounds of these elements present similar anomalies; for example, KO, KFl, KCy, KCl, KBr, and KI.

Here the solubility diminishes until we come to KCl, which is less soluble than KCy; then it increases to the last. Here, of course, the singular point is at KCl. With the corresponding compounds of sodium, the solubility diminishes to NaFl, which is the least soluble of the series, and then increases constantly to the end.

These facts at least seem to show that apparent variations from the law of series in properties, which evidently are unknown functions of several variables, should not be allowed to outweigh strong analogies, and certainly the analogies between Fluorine and the other haloids are very marked. Fluorine itself possesses properties such as we should expect to find in a member of the series above chlorine. The strong and active affinities of fluorine might be indeed predicted, after seeing the rapid increase both in the strength and activity of the affinities in passing from iodine to chlorine. In passing from bromine to chlorine, we pass from a liquid to a gas, permanent under any natural conditions; and we should expect, therefore, in rising still higher in the series, to find in fluorine a gas less easily reduced to a liquid than chlorine. Now although, on account of its remarkably active affinities, this fact cannot be demonstrated on the gas itself, it can, nevertheless, be inferred with perfect certainty from its compounds. Finally, the isomorphism of fluorine and the other haloids may be urged as indicating close analogy. From these considerations, I cannot but think that those chemists who have questioned the propriety of classing fluorine with the other haloids will, on reviewing the facts, and regarding the haloids in the light of a series, and not simply as a group of elements possessing certain general properties, be led to change their opinion.

Cyanogen, though a compound radical, has been classed with the other haloids, not only from its atomic weight, but also from its other analogies. Its properties are in most cases those which we should expect from an element occupying its position in the series; but in others it presents remarkable variations, owing probably to the fact that it contains a radical which is easily decomposed. As is well known, it is perfectly isomorphous with chlorine.

The propriety of classing oxygen in this series seems to be placed beyond doubt by the discovery of ozone, which, although it does not seem to possess such energy as we should expect in an element higher in the series than fluorine, may, nevertheless, be found to fulfil all anticipations should it ever be obtained in a perfectly unmixed condition. The isomorphism of oxygen with chlorine, and therefore with the other haloids, seems sufficiently established by the determination both of Proust and Mitscherlich of the tetrahedral form of Cu_2Cl. It must, however, be admitted that oxygen presents as strong analogies with sulphur as it does

with chlorine; and since, not only from its analogies, but also from its atomic weight, it appears to be the nucleus in all the first three series, I have placed it at the head of each. It may be mentioned here, that in all cases the fact that the atomic weight of an element is included in the general formula of a series, is an argument for classifying it in that series, if the relation between the atomic weights pointed out in this memoir is admitted to be a law of nature; but as I wish to show that the relation is not that of a mere accidental group of numbers, but is connected with the most fundamental properties of the elements, and has, therefore, the claims of a law, I have endeavored to show the correctness of the classification which conforms to the law, and, indeed, in fact suggested the law on other grounds.

The atomic weights of the numbers of the Nine Series, as determined by experiment, present greater deviations from the numerical law already explained, than are to be found in any of the others. The weights which would exactly conform to the general formula $8+n9$ are given in the column of the table headed Theoretical, while in the next column at the right are given the weights of experiment. These for the most part (in this as well as in the other series), have been taken from the table of Atomic Weights given in the last volume of Liebig and Kopp, *Jahresbericht* for 1852, which was supposed to give the latest and most accurate results. In the few cases in which the numbers have not been taken from this table, the initial letter of the name of the observer has been annexed. It will be seen, on comparing the two columns that the greatest deviation from the law is in the case of fluorine, if we consider the care which was taken both by Berzelius and Louyet in the determination of the atomic weight of this element. It may, however, be remarked, that, as the processes used by both experimenters were essentially identical, any hidden constant source of error would produce the same effect on both results; so that the atomic weight of fluorine cannot be regarded as yet absolutely fixed. Nevertheless, it is not possible that so great a difference between the true and observed weights as two units could have escaped detection in the numberless analyses which have been made, by the most experienced chemists, of the fluorine compounds. It must, therefore, be admitted, and not only in the case of fluorine, but also in other instances, that there are deviations from the law; but these deviations are not greater than those from similar numerical laws in astronomy and other sciences, and indeed, judging from the analogy of these sciences, they ought to be expected.

Those who are not familiar with the amounts of probable error in the determination of the different atomic weights would judge, on comparing together the columns of theoretical and observed values, that the deviations from the law were much greater than

they are in reality. It should, therefore, be stated, that, in by far the larger number of instances, the deviations are within the limit of possible errors in the determinations, leaving only a few exceptional cases to be accounted for. It must be remembered that, other things being equal, the amount of probable error is the greater the greater the atomic weight, so that a difference of 1·9 in the case of iodine is not a greater actual deviation from the law than only 0·5 in the case of chlorine. Indeed, it is very possible that on more accurate determinations the atomic weight of iodine will be found to correspond to the law, which cannot be expected of that of chlorine. It is well known that many of the larger atomic weights, especially those of the rarer elements, cannot be regarded as fixed within several units.

I have calculated, as well as the data I have would permit, the amount of probable error in the determinations of many of the atomic weights, and by comparing the results from different processes, and by different experimenters, I have endeavored to detect the existence of constant errors, which seem to be the great errors in all these determinations, those accidental errors which are made in the repetitions of the same process by equally careful experimenters being comparatively insignificant. The results of this investigation will be published in a subsequent memoir. It is sufficient for the present purpose to state, that, while they show that, in the greater number of cases, the apparent variations from the law are within the limit of probable error, there are yet several instances, where, after allowing for all possible errors of observation, there is a residual difference. I do not therefore look alone to more accurate observations for a confirmation of the law, but, regarding the variations as ascertained facts, hope that future discovery will reveal the cause. Whether the variations will be found to be a secondary result of the very cause which has determined the distribution of the atomic weights according to a numerical law, as the perturbations in astronomy are a necessary consequence of the very law they seemed at first to invalidate, or whether they are due to independent causes, can of course, for the present, be only a matter of speculation. There are, however, facts which seem to indicate that the variations are not matters of chance, but correspond to variations in the properties of the elements.

From the beautiful discovery of Professor Schönbein we have learnt that oxygen has two allotropic modifications, and that besides its ordinary condition, it is capable of assuming another highly active state when its properties resemble those of chlorine. Cyanogen is known only in a quiescent state. The other haloids, fluorine, chlorine, bromine, and iodine, are known only in a highly active state. Now it will be seen on examining the table, that the atomic weights of the highly active elements, as determined

by experiment, exceed slightly the theoretical numbers, and that where the affinities are the most intense, in fluorine, the deviation is the greatest. A similar fact may be observed in the atomic weights of the members of the Six Series. Arsenic has been proved to be capable of existing in two allotropic modifications. In its ordinary state, it has a crystalline form belonging to the rhombic system. In the other condition, in which it may be obtained by sublimation at a low temperature, it crystallizes in regular octahedrons. The other members of this series are probably isodimorphs with arsenic. The ordinary condition of phosphorus is its monometric modification, while the rhombic state seems to be the normal condition of arsenic, antimony, and bismuth. Now the atomic weights of the last three are either equal to, or slightly exceed, the theoretical number, while that of the first falls short, perhaps even by a unit. Other facts, which also tend to show that the deviations are not matters of chance, may be found in the affiliations of the series. There are some elements which seem to be most remarkably double-faced, having certain properties which connect them closely with one series, and at the same time others which unite them nearly as closely to another. In such cases we find that the atomic weight either falls naturally into both series, or, not corresponding exactly with the theoretical number of the series to which the element properly belongs, it inclines towards that of the other, and sometimes equals it. Such is the case with chromium, manganese, and gold, as will be seen by referring to the affiliations at the bottom of the Nine Series. These various facts force upon me the conviction, that this relation between the atomic weights is not a matter of chance, but that it was a part of the grand plan of the Framer of the universe, and that in the very deviations from the law, there will, hereafter, be found fresh evidence of the wisdom and forethought of its Divine Author.

The general formulæ for the Eight Series are, $8+n8$ and $4+n8$. The two nuclei correspond to two different sets of elements, or sub-series, one consisting of oxygen, sulphur, selenium, and tellurium, the other of molybdenum, vanadium, tungsten, and tantalum. The atomic weights of the first are all equal to $8+n8$; those of the second to $4+n8$. The sub-series exhibit marked analogies, as well as certain differences. They resemble each other chiefly in that the members of both form analogous acids with oxygen, while they differ in that though the members of the first sub-series form compounds with hydrogen, those of the second do not. The isomorphism of the members of each sub-series among themselves, with the exception of vanadium, is complete; but there seems to be no proof of any isomorphism between the sub-series. Johnston attempted to establish the isomorphism of chromic and molybdic acids from the red variety of

molybdate of lead from Rezbanya, which he supposed to be a chromate; but the fact has been disproved by G. Rose, who has shown that the supposed chromate is a molybdate mixed with a small amount only of chromate.* There seems, nevertheless, to be some reason for believing that chromic acid may replace molybdic acid to a certain extent. If this is proved, it establishes another link of connection between the members of the two sub-series, since chromic acid is isomorphous with sulphuric acid. For the present, however, we must regard them as sub-series, related, but distinct, the second being in a measure supplementary to the first. They are distinguished in the table by printing the names of the second sub-series a little to the right of those of the first, and the fact that their atomic weights are intermediate to those of the first, I have indicated to the eye by giving to the names also an intermediate position.

The analogies between oxygen and sulphur are so numerous, that, were we to place oxygen in but one series, we should place it in this. HO and HS, HO_2 and HS_2, resemble each other very closely, as do also the oxygen salts the corresponding sulphur salts. Moreover, there can be no doubt in regard to the isomorphism of the two elements, since it has been established upon the authority both of Mitscherlich and Becquerel, and from two different compounds. The only doubtful case in the series was that of vanadium, which in some of its properties resembles arsenic more closely than it does molybdenum. The reasons for giving it the place which it occupies were the facts that its acids correspond to those of molybdenum, and that it forms remarkably highly colored oxyds which are repeated also in molybdenum. It is true that the properties of the element itself are not those we should expect from the position which it occupies in our table; yet, if it were placed in the Six Series, it would fall between phosphorus and arsenic, which on the whole it resembles less than it does molybdenum, for although it is combustible, yet neither it nor its oxyds are volatile. I consider it, therefore, as a member of the Eight Series, but affiliating very closely with the Six. Its atomic weight favors this hypothesis. Vanadate of lead has been considered isomorphous with the phosphate; but as this isomorphism does not rest on any measurement of angles, and as, moreover, the received symbols of the two minerals, vanadanite and pyromorphite, on whose crystalline forms the isomorphism was determined, show a very different constitution, I have not given much weight to this fact.* The observed atomic weights of the members of this series are almost precisely the same as the theoretical members, and with the exception, perhaps, of molybdenum, there appears to be no instance in which the difference is greater than the amount of possible error.

* See G. Rose's Mineral System.

The members of the Six Group form a well characterized family, so that, with the exception of oxygen, there can be no doubt in regard to the justice of classifying them together, and any discrepancies will disappear on considering the group in the light of a series. They form acids containing three and five atoms of oxygen which are completely homologous, and make two series parallel to that of the elements. They form also a remarkable series of compounds with three atoms of hydrogen. The idea which has been advanced by some authors, that NH_3 is the nitrid of hydrogen, while PH_3 is the hydruret of phosphorus, or, in other words, that hydrogen is electro-positive with reference to nitrogen and electro-negative with reference to phosphorus and those lower in the series, does not seem to me correct, since the remarkable bases which may be formed from PH_3, AsH_3, SbH_3, and BiH_3, by replacing the hydrogen atoms by organic radicals, seem to indicate that they have the same type as NH_3, and are therefore homologues of it.

The isomorphism of the four lower members of the series is perfect. It has been shown in the table, both by the crystalline forms of the elements themselves, as well as by those of their compounds. In the other series, wherever it was possible, the same double proof has been given. The doubt expressed by G. Rose in regard to the dimorphism of arsenic, as I hope to be able to show in a paper soon to be published, has been removed. In one state arsenic crystallizes in perfect octahedrons of the regular system, and is therefore isomorphous, not only with antimony and bismuth, but also, in its allotropic state, with phosphorus. Isomorphism, as is well known, is not absolute, except in forms of the regular system. The rhombic angles of the crystals of arsenic, antimony, and bismuth, are respectively, 85° 41′, 87° 35′, 87° 40′, and therefore conform to the general rule. It will be observed that the angle varies constantly in the same way as we descend in the series. Now, although these few instances do not afford sufficient ground for any general conclusion, yet they show that similar variations are possible in the other systems, and therefore that we cannot be expected to establish isomorphism in any case except between merely consecutive members.

The atomic weights of the members of this series, with the exception of phosphorus, do not present any important deviations from the theoretical numbers, taking into account always, of course, the amount of possible error. The deviation in the case of phosphorus has already been noticed. Oxygen, it must be admitted, is not connected with the series from any similarity of properties though the phosphids, arsenids, and antimonids, present certain analogies with the oxyds. As has already been said, oxygen was placed at the head of this, as well as of the last two series, because its atomic weight seemed to be the nucleus of all three.

The Five Series is the shortest of all, consisting of only three members, carbon, boron and silicon. Of these, the last two are as closely allied as are any two members of the other series, silicon having precisely the properties we should expect in a homologue of boron, which was lower in the series; and the same is also true of their compounds. The analogies, however, between these two elements and carbon are by no means so close, for not only carbon cannot be proved to be isomorphous with them, but it does not form similar compounds. Carbonic acid, it is true, presents some points of resemblance to boracic and silicic acid; like them it unites in a large variety of proportions with bases, its alkaline salts give a basic reaction, &c.; but according to the generally received opinion, its symbol is CO_2, while those of boron and silicon are BO_3 and SiO_3. In its uncombined state, however, carbon resembles boron and silicon, not only in its outward properties, but also in its action before the blowpipe. Two of the allotropic states of carbon, graphite and charcoal, are probably repeated in boron, and are known to be in silicon. The principle of exclusion would also seem to place carbon in this series, for it certainly presents no analogies with the members of any other. The correspondence of the atomic weights of the members of this series to the law is remarkably close.

The four series is by far the largest of all, including the greater number of what are generally known as the heavy metals. The members of the series resemble each other in the following respects. First, they are isomorphous; for although each member cannot be directly proved to be isomorphous with every other, yet isomorphism can be established between consecutive members, which, as has before been said, is all that can be expected. Second, the members of this series all form, by uniting with oxygen, either protoxyds or sesquioxyds, or both, which, as a general rule, are strong bases. Third, these oxyds are either insoluble, or nearly insoluble, in water. And finally, the elements of the series have all those physical properties which are known as metallic properties.

This series may be naturally divided into two sub-series. The first contains those elements whose protoxyd bases are their characteristic compounds, and which do not form acids with oxygen. The second contains those elements whose characteristic compounds are their sesquibases. They generally unite with two or more equivalents of oxygen, and form acids. These sub-series are distinguished in the table in the same way as those of the Six Series. Corresponding to these sub-series we have two sets of atomic weights, each having the same common difference, but differing in their starting-point or nucleus. The first set is expressed by the formula $4+n4$, the second by $2+n4$.

The sub-series affiliate with each other in a most remarkable manner. Manganese, for example, not only forms a strong protoxyd base, but also unites with a larger amount of oxygen, forming both a sesquibase and acids. Its atomic weight places it in the first group, and it has therefore been classed there, although by its properties it is equally allied to the second. Cobalt and nickel certainly resemble much more closely the members of the first than of the second sub-series, although their atomic weights place them in the second. With this exception, the subdivision of the series which the atomic weights require does not differ from that suggested by the properties of the elements. The members of this series may of course be still further subdivided into groups according to their special properties, as they are in all works on chemistry. They are placed together here because the atomic weights form but one numerical series.

The isomorphism of the members of this series will be found well established with the limitations before given. In order to establish the isomorphism of cobalt and nickel with iron, the isomorphism of one atom of arsenic with two atoms of sulphur has been assumed. This is generally admitted; but if it is not, no one can doubt in regard to the isomorphism of these three metals, as they constantly replace each other. Glucinum, zirconium, lanthanum, cerium, and thorium, cannot be shown to be isomorphous with the other metals by any of their compounds, but their oxyds are known to replace the analogous oxyds of the other metals. So also is ruthenium known to replace rhodium. There have been doubts expressed in regard to the existence of a monometric form of zinc; but as we have established its isomorphism with the other members of the series, not only by its own crystalline form, but also by those of its compounds, the fact is of no importance to the present question.

The atomic weights of the members of this series, as determined by observation, very nearly correspond with the theoretical numbers, which is the more remarkable, as the limit of error in the determination of the atomic weights of the greater number, especially of the rarer metals, is quite wide.

The Three and last Series is composed of hydrogen and the metals of the alkalies. The analogies between lithium, sodium and potassium, are very close, as is well known, and there can be no doubt in regard to the propriety of classing them together. It may be said however, in regard to hydrogen, that it resembles as closely some of the metals of the four series as it does those of the alkalies. Though this cannot be denied, yet the fact that the atomic weight of hydrogen is the nucleus of the series, and the great solubility of the alkalies in water, may be urged as reasons for placing it at the head of the Three Series.

The isomorphism of lithium, sodium, and potassium, is fully established; but I can find no data which prove hydrogen isomorphous either with them or with the metals of the other group.

The unit of the atomic weights which has been used thus far throughout the table, is the double atom of hydrogen; but the nucleus of the Three Series is the weight of the single atom, so that the unit in this series is one half of the unit of the weights in all the other series. This fact must be kept in mind in comparing the atomic weights of this with those of the other series. All the weights might have been made uniform by doubling them throughout; but as this would not have changed the relation, and would have been departing from the general custom, it was thought best to confine the doubling to the Three Series into which alone hydrogen enters. The general symbol of this series is $1+3n$, where of course the unit is one half of that of the symbols at the head of the other series. The observed atomic weights will be found to correspond very closely with the theoretical numbers; indeed, the two coincide, except in the case of potassium, where the difference is 0·6. This, however, it must be remembered, is 0·6 of the single hydrogen atom. Compared with the double atom, as the weight of potassium is generally given, the difference amounts to but 0·3.

One of the most remarkable points of the classification which has been now explained, is the affiliation of the series. We find in chemistry, as in other sciences, that Nature seems to abhor abrupt transitions, and shades off her bounding lines. Many of the elements, while they manifestly belong to one series, have properties which ally them to another. Several examples of this have already been noticed. In such cases, we find invariably, that there is a similar affiliation of the atomic weight. Of all the elements chromium and manganese are the most protean. Two atoms of these elements unite with seven atoms of oxygen and form acids analogous to perchloric acid, and, as has already been shown, the weight of two atoms of either element falls into the Nine Series. Moreover, one atom of chromium or of manganese, unites with three atoms of oxygen, to form chromic or manganic acid. Chromic acid is a strong oxydizing agent, and resembles closely nitrous acid, and the atomic weight of chromium falls into the Six Series just below that of nitrogen. Manganic acid, on the other hand, resembles sulphuric acid, with which it is isomorphous, and the atomic weight of manganese would place it in the Eight Series. In like manner osmium in many of its properties resembles platinum and the other metals with which it is associated in nature; but, unlike them, it forms a very remarkable volatile acid, whose insupportable and suffocating odor, as well as composition, reminds one of the acids of the Nine Series, and its atomic weight seems to justify the apparent analogy. Gold

likewise, though the noblest of metals, yet in some of its chemical relations resembles much more closely the members of the Nine than of the Four Series, and here again its accommodating atomic weight seems to account for its double-sided character. Several other examples of similar affiliations are given in the table, but do not need explanation.

In the description just concluded of the classification of the chemical elements, which is offered in this memoir, I have not entered into details, for to have done so would have been to write a treatise on chemistry. I have confined myself almost exclusively to general points, and referred only to those particulars which I thought might present doubts. I hope that I have been able to show, first, that the chemical elements may be classified in a few series similar to the series of homologues of organic chemistry; second, that in those series the properties of the elements follow a law of progression; and finally, that the atomic weights vary according to a similar law, which may be expressed by a simple algebraic formula. As already intimated, I have endeavored to prove the correctness of the classification on general grounds, in order that it might appear that the simple numerical relation which has been discovered between the atomic weights is not a matter of chance, but is connected with the most fundamental properties of the elements. I might leave the subject at this point, but the existence of the law which I wish to establish will be proved more conclusively if it can be shown, not simply that the general properties of the members of each series vary in a regular manner, but also if in one or more cases the exact law of the variation can be pointed out.

There are but few properties of the elements which are subjects of measurement, and which therefore can be compared numerically. Such are the specific gravity in which the three states of aggregation, the boiling and melting points, the capacity of heat, and a few others. It is easy to see that there are but few of these properties the law of whose variation in the series we could reasonably expect to discover in the present state of science. Most of them evidently depend upon molecular forces with which we are entirely unacquainted. Such in solids is undoubtedly the case with so simple and fundamental a property as specific gravity, and most, if not all, of the other properties of solids belong to the same category. It cannot therefore be expected that we should point out the laws by which these properties vary, although the remarkable investigations of Kopp, Dana, Filhol, Schröder, and others, on the relations between the density of substances and their atomic weights, and those of Kengott on the relation of hardness to atomic volume, give grounds for expecting that even they will before long be discovered. In liquids and gases, however, most of these molecular forces which produce the ap-

parent irregularities in solids have less influence, as we should naturally expect, probably because the atoms are removed out of the sphere of their action. We may therefore hope, on comparing together the properties of the liquid or gaseous states of the elements in any series, to discover some numerical relation between them. Unfortunately, however, we have not sufficient data for making such a comparison except in the case of one property, the specific gravity. The boiling point, which would be a very valuable property for the purpose, is known only in a few instances.

That the specific gravity of the elements in their gaseous state varies in each series according to a numerical law, follows necessarily from what is already known. It is a well-known fact, that the specific gravities of the gaseous states of the elements divided by their atomic weights give quotients which are either equal, or which stand in a very simple relation to each other. For any series, as far as we have data, this quotient is the same for all the elements with only a few exceptions. That is $\frac{\text{Sp. Gr.}}{\text{At. W.}}=p$. But we have found that At. W. may be expressed in general by $a+nb$, and substituting this for for At. W. in the above equation, it becomes $\frac{\text{Sp. Gr.}}{a+nb}=p$, or sp. gr. $=pa+npb$; so that $pa+npb$ is a general expression for the specific gravity of all the elements of any series, in the same way that $a+nb$ is for the atomic weight. The value of p will differ according as the specific gravities used are referred to hydrogen or air. Below will be found tables which give the calculated and observed specific gravities of the elements of the Nine and Six Series referred to hydrogen, which has been taken as the unit instead of air, as we thus in a great measure avoid fractions. In the Nine Series $p=1$, so that the numbers representing the specific gravities are the same as those representing the atomic weights. In the Six Series it equals two, so that the numbers representing the specific gravities are in this series twice as large as those representing the atomic weights. When the specific gravity has not been observed, the calculated number only is given. The observed numbers are taken from the "Table of Specific Gravity of Gases and Vapors," in Graham's *Elements of Chemistry*, which is a very complete collection of all known data. For the other series, we have only occasional data, so that no complete tables of their specific gravities are possible.

It is evident, then, that at least one property of the elements varies in the series according to an ascertained numerical law. But, it may be said, this proves nothing, for these specific gravities are connected so closely with the atomic weights that what is true of one must be to the same extent true of the other. It must be remembered, however, that the specific gravities are a distinct set of observed facts, and that the probability of a law is

in exact proportion to the number of facts which accord with it. Moreover, the closeness of the connection is unimportant. Whether the value of p be expressed by a single digit, or by a complicated algebraic formula, is evidently a matter of indifference so far as the confirmation of the law is concerned.

THE NINE SERIES. $\frac{\text{Sp. Gr.}}{\text{At. W.}}=1.$ Sp. Gr. $=8+n9$.			THE SIX SERIES. $\frac{\text{Sp. Gr.}}{\text{At. W.}}=2.$ Sp. Gr. $=16+n12$.		
Names.	SPECIFIC GRAVITIES.		Names.	SPECIFIC GRAVITIES.	
	Theoret.	Observed.		Theoret.	Observed.
Oxygen	8	16	Oxygen	16	16
Fluorine	17		Nitrogen	28	14
Cyanogen	26	26	Phosphorus	64	64
Chlorine	35	35·5	Arsenic	148	150
Bromine	80	78	Antimony	256	
Iodine	125	126	Bismuth	412	

I regret exceedingly that there are not sufficient data in the case of any of the other properties of the elements in the state of gas to allow comparison, as I feel confident that the law which governs their variation in the series might easily be discovered; but I look forward to the time when in the general formula $pa+npb$ the value of p shall be known, not only for the properties of the elements in their gaseous state, but for every property capable of numerical expression.

In this memoir I have confined myself entirely to the elements, but it is evident that the classification here offered, and the numerical law here explained, may be extended to all compounds. The elements of any one series, by combining, give rise to perfectly parallel series of homologous binaries, some of which are given in the table. The binaries of those series which have the greatest common difference are generally acids; and of those which have the smallest, they are generally bases. These acids and bases unite together and form series of homologous salts. As in organic chemistry, many of the series are very incomplete; but they are much more generally perfect than in that newer department of the science, and almost every day fills up some gap.

It will be seen, then, that not merely a plan has been given for classifying the elements, but one which will also embrace all inorganic compounds, and affiliate with the similar classification which has already been established in organic chemistry. We have not attempted to develop such a classification, since to do it would require a volume; nor is it necessary, as any one can develop it for himself.

That the atomic weights of the series of homologous compounds follow the same numerical law as those of the elements is easily shown. Take as an example the series of salts homologous with KO, NO_5, which may be expressed in general by,

KO, RO_5, where R is any member of the Six Series after oxygen, and whose atomic weight, therefore, equals $8+n6$. The atomic weight of KO, RO_5 must be necessarily $39{\cdot}5+48+(8+n6)$, or $95{\cdot}5+n6$. As this symbol differs from that of the Six Series only in the nucleus, the atomic weights of the salts which are represented by it must progress by the same differences as those of the corresponding elements.

The properties of these series of homologous compounds will also be found to vary in a regular manner, and the law of the progression of the specific gravities in the gaseous state can be easily expressed algebraically, since in each series the quotient of the specific gravity divided by the atomic weight is a constant quantity. As an illustration, we may take the series of binaries homologues of water given in the Nine Series of our table. It follows from what has been said, that the atomic weights of these compounds equals $9+n9$. With each $\frac{\text{Sp. Gr.}}{9+n9}=\frac{1}{2}$, therefore Sp. Gr. $=4{\cdot}5+n4{\cdot}5$. We give below a table of the observed or calculated specific gravities, not only of these compounds, but also of those homologues of NH_3 whose specific gravity has been observed.

Homologues of Water. $\frac{\text{Sp. Gr.}}{\text{At. W.}}=\frac{1}{2}$. Sp. Gr. $=4{\cdot}5+n4{\cdot}5$.			Homologues of Ammonia Gas. $\frac{\text{Sp. Gr.}}{\text{At. W.}}=\frac{1}{2}$. Sp. Gr. $=5{\cdot}5+n3$.		
Symbols.	Specific Gravities. Theoret.	Specific Gravities. Observed.	Symbols.	Specific Gravities. Theoret.	Specific Gravities. Observed.
HO	4·5	9	NH_3	8·5	8·5
HFl	9		PH_3	17·25	17·5
HCl	13·5	13·5	AsH_3	39	38·5
HBr	40·5	39·5			
HI	63	63·5			

As the series of compounds give a greater scope for investigating the relations of properties than is presented by those of the elements, we may expect that these relations will be first discovered in the former, and to my conceptions chemistry will then have become a perfect science, when all substances have been classed in series of homologues, and when we can make a table which shall contain, not only every known substance, but also every possible one, and when by means of a few general formulæ we shall be able to express all the properties of matter, so that when the series of a substance and its place in its series are given, we shall be able to calculate, nay, predict, its properties with absolute certainty; and when our chemical treatises shall have been reduced to tables of homologues, and our laws comprised in a few algebraic formulæ, then the dreams of the ancient alchemist will be realized, for the problem of the transmutation of the elements will have been theoretically, if not practically, solved.

EXPLANATION OF THE TABLE.

The formula at the head of each series is a general expression for the atomic weights of that series. The names of the series are derived from the "Common Differences," which are the numbers multiplied by n in the general formulæ. In the columns headed "Theoretical" are given the atomic weights calculated from these formulæ and the values of n given in the last columns at the right of each division of the table. In the columns headed "Observed" will be found the observed values of the same atomic weights. These have been taken from the table of atomic weights given in the last volume of Liebig and Kopp's *Jahresbericht* (for 1852), with the exception of those against which are placed the initials of the observers. The last were taken from Weber's *Atomgewichts Tabellen.* In some cases the atomic weight is taken at twice its received value, but it is then underlined. The compounds in any one column at the right of the names of the elements are homologous. In the same way, those in any one at the left are isomorphous. The numbers at the head of these last columns indicate crystalline systems as follows: 1. Monometric; 2. Dimetric; 3. Trimetric; 4. Monoclinic; 5. Triclinic; 6. Hexagonal. The data from which the table was compiled were drawn from numerous sources, but especially from the following works: Gmelin's *Handbook of Chemistry*, Graham's *Elements of Chemistry*, Phillips's *Mineralogy* by Brooke and Miller, and Gustav Rose's *Krystallo-chemische Mineralsystem.* References have been given only in a few cases, to avoid crowding the tables. For authorities in other cases, the author would refer to the above-mentioned works.

CORRESPONDENCE.

Extract from a letter from Dr. W. I. Burnett to Prof. J. D. Dana.

Magnolia, Florida, March 1, 1854.

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FLORIDA still remains almost a *terra incognita* to naturalists, although in geological structure, and Fauna and Flora, one of the most interesting States in the Union. Even from the limited survey I have made of its surface and general features, I am persuaded that some erroneous views have been entertained as to its formation and the exact nature of its peninsular relations. Naturalist after naturalist, from the time of the quaint old Bertram to the present day, have traversed its various sections, collecting specimens relating to their particular line of study, but no report has yet been made, in full, of the

geological structure, the agricultural resources, or the Fauna and Flora of the state.*

Gen. Bernard's report of a survey by a corps of topographical engineers for a practical object, contains the greatest number of details, as well as the most reliable, upon the general features of Florida, that I have seen. In making these notes I shall draw somewhat from this report, especially as to measurements. From the southern boundaries of Georgia to a line drawn from Cape Canaveral to Tampa the land is covered with pines, is much undulated, and in some places even hilly. The waters that run into the Atlantic Ocean and those that run into the Gulf of Mexico are divided by a ridge which extends slopingly from north to south, terminating at the above-mentioned line. The height of this ridge is certainly much greater than I had supposed, or than is generally understood even by those who have traversed the state, for the land rises very gradually and, without instruments, one is wholly deceived. The highest elevation is situated one mile west of Kinsley's pond and seven miles east of Sampson's pond, and is two hundred and thirty seven and one-half ($237\frac{1}{2}$) feet above low tide in the Atlantic Ocean. There are other points on this ridge between 150 and 200 feet high; and, distant only a few miles west of St. John's river, I have traversed elevations, at least one hundred feet above the stream that encircled their base and ran towards the sea. I am particular thus to allude to this point in order to correct the erroneous opinion entertained by many that the whole peninsula of Florida is a flat sandy country, and not elevated more than fifteen feet any where above the level of the sea. The soil is sandy, but the hummocks contain red, yellow, or black clay, mixed with sand. The upper stratum is about six feet of sand, and the substratum appears to be limestone mixed with much sand. Scattered over the surface are more or less conical depressions or *sinks*. Those that I have seen are of pretty regular form, though of variable size; they contain no water, and are probably due to the substratum of limestone crumbling or being undermined from some cause. Nowhere so far as I am aware, do these depressions appear to be fed by intermittent springs, like those of the pine region of South Carolina, which have already been described by Mr. Tuomey.

In this connection, I may mention that Bernard's levelling parties commenced, one division at the Gulf of Mexico, the other at the Atlantic Ocean, and, proceeding inwards, met at Alapaka. At this point it was found that they were at an elevation of ninety-five feet above the level of low water of the Atlantic, and ninety-one and a half feet above the level of the low water of the Gulf—making three and a half feet difference between the low water level of these two waters. The same result was obtained by a repetition of this same levelling. The fact is quite interesting in connection with the phenomena of the Gulf Stream.

* Buckingham Smith, Esq., is now engaged upon a detailed history of Florida, as I understand. The great pains he has taken to consult ancient documents relative to its primitive settlement, as well as his personal knowledge of the state physically, will ensure from him a most valuable work. But it is hoped that the legislature of this state will soon take measures for having a careful physical survey of Florida, by a competent geologist and naturalist.

But the physical features of the southern extremity of Florida are, as is well known, the most interesting. It is still land incompletely formed, and the conditions of its successive growth are yet visible. It is needless for me here to describe these conditions or to enter upon the details of this coral built promontory. Within a few years Agassiz has, as is well known, carefully studied not only the phases of the formation of the Keys, but also the coral-polyp marvels of the deep, which have led to these almost continental changes. Between the ridge above referred to and the everglades, is that remarkable collection of fresh water known as *Lake Okechobee.* Its average diameter is about thirty miles, and it is reputed to be quite deep. It contains a few islands, and is the reservoir of the neighboring streams, and of the Kissime river. South of this lake are the everglades which lie in a vast basin of limestone. They consist of a shallow lagoon—their waters varying from one to six feet, studded with islets, trees, and a rank growth of vegetation. Their waters are fresh and due to the copious autumnal rains, and as the rim of the basin is some twelve or fifteen feet above the ocean, the slow moving currents set southwards towards the sea. This section of Florida, as shown by Agassiz in his recent lectures on the Florida reefs, is of coral origin and of comparatively modern date. Thus recently formed, the geology of this promontory may be easily inferred. It is oolitic limestone filled with the shells and corals, and the fossil remains of species that still exist. The present state of the Indian affairs did not allow me to visit this southerly point, but from intelligent gentlemen connected with the army, I learned that, besides the curious geological features I have above alluded to, this land presents many wonders and objects most interesting to the naturalist who will have the intrepidity and hardiness to explore it.

High up on the St. John's river there are from point to point vast deposits of fluviatile shells. At Enterprise especially, these form even bluff-like elevations of forty to fifty feet, and of considerable extent. Hereabouts have been found also the fossil remains of Vertebrata that still exist. The whole physical structure of this region is of great geological interest, and would well repay the most careful examination.*

One of the most interesting features of this State is the St. John's River. It furnishes a key to the whole plan of formation of a large portion of Florida. As is well known, its course is northward, unlike that of other American rivers, and this fact alone would indicate that the conditions of its primitive formation are different from those of other streams. It is in fact a chain of lakes, as its Indian name signifies, (*Welaka*,) and it rises in the Cypress Swamp, situated near Indian River. Its similarity in appearance to an arm of the sea, is indeed more than apparent, for in its course of 300 miles, with often a width of six miles, there is a fall of only about twelve feet, and the tide is felt 100 miles above its mouth. But fed mostly by lateral infiltration chiefly on

* Our friend, Prof. Bailey, has been all about the travelled sections of Florida with his microscope, and his recently published results show that he had his eyes constantly and attentively upon the "invisible forms of life." He has also noticed, these grosser features (p. 23) which certainly are anything but microscopic. See **Microscopical Observations made in South Carolina, Georgia and Florida, by J. W. Bailey, &c., &c. Smithsonian Contributions to Knowledge, vol. ii, Art. 8.**

its western side, and receiving, besides, a few large tributaries, this majestic river, slow moving as it must be, discharges an enormous quantity of water into the sea. The segment of land included between its mouth and a line draw from its source to the sea, is at no point elevated above 12 or 14 feet, that is, higher than the fall of the river itself. It is therefore probable that this segment, like the whole southern extremity of the state, is of comparatively recent origin previous to which, of course, the St. John's River did not exist. This stream, consequently, is not properly a river, but rather an arm of the sea shut off from the ocean by alluvial sandy deposits. It receives the waters which arise on the easterly side of the ridge of the state, precisely as though it were the sea itself. Indian River presents the same phenomena, excepting that it is not wholly shut off from the ocean, but is open by inlets; and were it not for the influence of the Gulf Stream, the reef which forms its ocean boundary would be indefinitely extended until this river became truly an inland stream. By this view of the gradual geological changes that have supervened in this state, it would appear that ancient, primitive Florida, must have been of quite insignificant size,—indeed, limited to the grand dividing ridge which perhaps is a spur of the upheavals of the states that bound it on the north—a narrow promontory thus extending southward from the main land, and only about half the length of the present peninsula. But the conditions of the formation of what may well be called modern Florida, have an additional interest from their relations to the phenomena of the Gulf Stream. Agassiz has shown that the coral building is now at an end on the southern extremity of this promontory, since the depth of the Gulf Stream as it shoots round this point is incompatible with the conditions of life of the coral polyp; and so all hope of an annexation, physical at least, of this continent to Cuba, is in vain. It is a curious question, What were the phenomena of this stream when ancient Florida alone existed? Is it possible that with a current anything like the present, the slow-growing coral-formation could have made progress to the extent we well know it really has?

The Fauna and Flora of Florida seem much like those of Georgia and Alabama, as far as they go, for I have been surprised to find so great a deficiency in a land having so genial a climate. But this deficiency does not exist in one class—that of reptiles—at least, in the Saurians and Ophidians. The herpetologist will here find both venomous and non-venomous snakes which will meet his desires both as to numbers and size; and that prince of American reptiles, the alligator, is here seen in an abundance that is almost incredible to any but the beholder. The natural history, the anatomy and physiology of this kingly Saurian has yet to be written, and a pleasant as well as a most honorable task remains for some American anatomist to monograph this subject, which he could easily do by placing himself for a season on some convenient spot on the St. John's river. Aside from the special anatomy of the animal, I am persuaded that careful, well-conducted physiological experiments, especially upon its nervous system, would furnish results of great value in physiology. Here the whole relations of the so-called reflex actions of the nervous system are presented upon a most magnificent scale.

The experiments that I have already made have proved deeply interesting to me, and have modified somewhat the opinions I have hitherto entertained upon this class of nervous actions. But this point will have my special consideration at another time.

I cannot close this letter without alluding to a subject which, among others has engaged my attention during my sojourn in Florida this winter for my health. I refer here to the orange insect or *Coccus*, which has almost entirely ruined the prospects of an occupation, one of the most delightful, as also one of the most profitable that man could here engage in—the culture of the orange. The climate and soil, especially of east and south Florida, is excellently fitted for the growth, almost to perfection, of this most delicious fruit. All along the St. John's river there are groves of the wild-orange that salute the eye most pleasantly, although this fruit is too acid to be palatable except when used to make a beverage (orangeade).

The China orange (*Aurantium sinense*) is the species which has here been introduced for cultivation as a commodity. Here it grows most luxuriantly, and, for size and flavor, has always had the preference in the market, even to those produced in more tropical regions. Some fifteen years since when the orange-culture of this state was in its most favorable condition, the revenue from this business was indeed wonderful: 6,500 oranges have been yielded by a single tree, 2,000 being a not uncommon yield. At a small place called Mandarin, situated on the St. Johns river, thirty-five miles from its mouth, there were annually produced 1,500,000 oranges. At other places, such as St. Augustine, there was a yield of even a greater number.

The cold of the winter of 1835 seriously affected the vitality and prosperity of the trees throughout the peninsula. But the great drawback now to their successful culture, is undoubtedly, the orange insect or *Coccus*. This insect appeared so as to be felt about the year 1838, and its increase and distribution since has worked most lamentable effects. Section after section has yielded to its ravages, and those fine groves above mentioned have become nearly ruined. The trees seem blasted, and only yield a very small quantity of fruit. Some of the groves I visited are now completely deserted; all hopes of an extermination of the pest being relinquished. Some trees that I examined seemed completely covered, trunk, limbs, leaves, and fruit, with this insect, and it is not at all strange that the vitality of the trees yields to such a draft upon its juices.

Careful, from scientific reasons, not to lose so excellent an opportunity for the study of the animal, and desirous, from practical reasons, to find out, if possible, a means for its extermination,—this insect has received no little attention from me. It is exceedingly small and insignificant for so disastrous a worker, the males being only $\frac{1}{30}$ and the females about $\frac{1}{20}$ of an inch in length. On this account, its anatomy and physiology could be successfully studied only by the microscope; but the results afforded have well repaid the labor thus bestowed. These results are certainly sufficiently important for a special consideration, which I propose to give them, at some future time. It will suffice for me to say here, that the conditions of their reproduction are remarkable, and quite favorable for the prodigious multiplicity of this

animal. The life of the females is almost vegetative; with an outward form so undeveloped as to quite resemble a larva, she fastens herself to the leaf or bark, and, drawing the juices therefrom, develops her ovaries, successive litters of eggs being formed; and, as the conditions of her life would be wholly incompatible with the formation of eggs and their deposit, as in most other insects, these eggs undergo most of their development in the mother,—escaping from her just before they hatch. The orange Coccus is therefore viviparous. The male appears only at certain seasons, and is literally only a locomotive male apparatus, for his digestive organs are undeveloped, and as the testes, &c. are already fully formed at his first appearance, he lives only to impregnate the females. The means I shall recommend for a removal of this pest, will be based upon a consideration of the intimate economy of the animal.

Correspondence of M. Jerome Nicklès, dated Paris, February 27, 1854.

At one of its late sessions, the Academy of Sciences awarded the annual prizes to authors of works on different subjects proposed by it, and also "encouragements" to those who have distinguished themselves by researches in their own proper domain. As the list of prizes is long, we will only mention those of more special interest.

1. The Academy awarded the *prize in Mechanics* to Felix Franchot (1) for the invention of the lamp known under the name of "lampe à moderateur," brought forward by him in 1836 and 1837, the use of which has now become general; (2) for his experiments in the construction of motive engines with hot air, which he has pursued with unceasing perseverance since the year 1836. We have already spoken of the researches of M. Franchot and will not return to them.* The report upon them was made by the distinguished engineer and mechanician, M. Combes, at this moment President of the Academy of Sciences.

2. Prize Extraordinary on the Application of Steam to Navigation. In November, 1834, the government established a prize of 6000 francs for the work or memoir which should occasion the greatest advance in the application of steam to navigation and to naval force. Since this epoch, until 1848, great improvements have taken place, but they were not first put in practice in France; they were brought out in America and England. But now, by the construction and success of the "Napoleon," a vessel of the line with sails and steam, the end of the prize has been completely attained. The Academy has consequently assigned the prize (1) to M. Dupuy de Lôme, officer of the naval Engineer corps, for devising and constructing the ship Napoleon with sails and steam having a screw propeller, which unites in a remarkable degree rapid sailing and excellent sea qualities. (2) A prize to M. Moll, officer of the Naval Engineer Corps, sub-director of naval construction at the great manufacturing establishment of Indret, for having calculated the theory of, and constructed, the mechanism of the Napoleon, and for having, in

* In awarding the prize to M. Franchot, the Academy does not touch the question of priority as to hot-air engines, it being well known that M. Franchot in this respect has others before him.

connection with M. Bourgois, made the experiments with the screw propeller, the results of which are now the rule with engineers. (3.) To M. Bourgois, Captain in the Navy ("de fregate") for his labors on the screw propeller, and for his views on the progressive transformation of the existing naval marine into a mixed marine of sails and steam. The Report made by M. Charles Dupin, is extremely interesting, as respects the history of steam navigation, and we regret that we cannot find place for it here.

The question proposed in 1852 for the physical sciences was the following: "*To discover by direct observations and experiment the mode of development of intestinal worms and that of their transmission from one animal to another, and to apply the anatomical and physiological facts thus brought out to determining their affinities.*" In pursuance of the remarkable Report of M. de Quatrefages, the Academy awarded the prize to M. van Beneden, Professor at the University of Louvain (Belgium), and made honorable mention of M. Küchenmeister of Zittau (Saxony).

The prize in Experimental Physiology has been awarded to M. Claude Bernard for his discovery, with regard to the influence, which the cervical portion of the Great Sympathetic Nerve exerts on the temperature of the parts, to which its threads are distributed, accompanying the arterial vessels.

Another prize has been given for the *Traité de Chimie Anatomique et Physiologique* of MM. Robin and Verdeil, on account of the new facts which it contains and the thorough manner in which chemical facts in their relation to medicine are presented.

Among the prizes, there are also, the medals awarded to MM. de Gasparis, Chacornac, Luther and Hind for the discovery of five new planets; a prize to M. Fontaine and M. Machecourt for a parachute for the use of miners; an "encouragement" to M. Chuard for his attempts towards perfecting the miner's safety lamp. Other prizes for works on Statistics, Medicine, Surgery, and a grand prize to M. Hervé Mangon for his "Etudes sur le drainage au point de vue pratique et administratif."

Among the subjects of prizes offered we mention only those pertaining to the physical sciences.

1. For 1856.—A rigorous and methodical investigation into the metamorphoses and reproduction of the Infusoria properly so called, (the Polygastrica of M. Ehrenberg).

2. For 1855.—An exposition of the laws governing the distribution of fossils in the different sedimentary strata in their order of superposition; and a discussion of the question of their appearance or disappearance, successive or simultaneous.

A research into the nature of the relations existing between the present and past states of the organic kingdom.

Another for 1856.—The determination, through the study of the development of the embryo in two species, one taken from the class of Vertebrata, and the other either from the Mollusca or Articulata, of the proper foundation for comparative embryology.

The prizes for either of the above is a gold medal of 3000 francs value.

A medal of gold, of 800 francs, is decreed each year to the work, printed or in manuscript, which appears to have contributed the most to *the Progress of Experimental Physiology.*

A gold medal of the value of 2500 francs, is offered for 1856, for the best work on the *mode of fecundation* of eggs, and the structure of the organs of generation, in the principal natural groups of the class of polyps, or of that of Acalephs.

A large number of prizes are offered in mathematics, medicine and physiology, and NOTHING for physics, chemistry or mineralogy; it would seem as if these sciences had no existence. It is true that the Academy of Sciences of Paris has little to show among the great discoveries which physics and chemistry have accomplished in these later years; certainly such discoveries have not been called forth by any action on the part of the Academy.

Deaths and Academic Elections.—The Botanical Section has lost this year three of its members: A. DE JUSSIEU, President of the Academy, AUGUSTE DE ST. HILAIRE, and more lately GAUDICHAUD; and in the preceding year it lost RICHARD, Professor in the Faculty of Medicine. It has been recently occupied with filling these vacancies. MONTAGNE and TULASNE replace MM. Richard and de Jussieu; and A. de St, Hilaire is succeeded by M. MOQUIN-TANDON, the celebrated author on Vegetable Teratology, at present Professor in the Faculty of Medicine.

Zoological Society for the Acclimation of Animals.—Since we are in the midst of Natural History, we hasten to announce the formation of a Society whose end is to apply the Science of Zoology to the Acclimation and Domestication of valuable animals from different regions of the globe. Its founder and President is M. Isidore Geoffroy Saint Hilaire, son of the illustrious zoologist, and who should not be confounded with the botanist, M. Auguste de St. Hilaire, to whom we have above alluded.

M. Is. G. St. Hilaire is Professor of Zoology at the Jardin des Plantes, and for a long time he has prepared himself for the noble work which, aided by several other savants and the principal great proprietors of France, he is about to undertake. The end of the Society is to promote (1) the introduction, acclimation and domestication of species of animals either useful or ornamental. (2.) The perfection and multiplication of new varieties, introduced or domesticated. The number of members of the Society is not limited, and foreigners may unite in it. And among the latter, there are already Prince Demidoff, MM. Graëlls, Ramon de la Sagra, Vilanova of Madrid, and a large number of Belgians, Swiss and Germans, and some from the United States.

Artificial Production of Pleochroism in Crystallized Substances.—The influence which small quantities of a foreign substance chemically inert, exert upon the physical properties of bodies, as their density, index of refraction and angle of polarization, has long been known. Some years since,* I showed that these causes act also on the angles of crystals, and at times produce modifications as great as a change of the type and system of crystallization, and thus may give rise to dimorphism.† Since then M. Hugard has observed facts of a similar

* Comptes Rendus de l'Acad. des Sci., 1848.

† Comptes Rendus de Laurent, etc., 1850, and Annalen de Ch. et de Phys., 1853.

kind in sulphate of strontian, and M. G. Rose, in tetradymite; according to the latter, foreign matters cause the rhombohedron of tetradymite to vary nearly 3 degrees from that of the pure metals.

In view of such facts, my opinion appears less exaggerated than at first. The two forms of dimorphous bodies are in general approximate forms, whose prisms, although pertaining to distinct systems, differ between them only a few degrees in angle. I have inferred that a foreign substance present, may at times modify the molecular force which presides in crystallization, so as to give the molecules what is needed to cause them to pass from one system to another.

Should a new planet suddenly appear in our system, all the other planets would feel it as much as if a planet were withdrawn. A crystal is in my view a planetary system formed of atomic planets in motion; foreign molecules or planets abruptly introduced, bring in their own attraction and proper movements, and impress on the other atoms another condition of equilibrium.

Thus it may be with foreign matter which is chemically inert, as in my observation on the bimalate of ammonia, in which the foreign substance was the coloring material that was eliminated entirely by some crystallizations in pure water. If on the contrary the substance is active chemically, the form may change either totally or partially. These last are what I have called examples of *hemimorphism*, which I have illustrated by glycocoll and its combinations, oxalate and chlorhydrate of methylammine, etc.

It is not my intention at this time to touch on these questions. They have been called up by the reading of an important paper by M. Senarmont on the artificial production of pleochroism;—a property which he communicates to certain salts by introducing into them through crystallization, foreign substances that will not unite with them chemically, and which may be eliminated spontaneously by some solutions followed by crystallization in distilled water. He has thus communicated pleochroism to the nitrate of strontia, by crystallizing it in water colored with log-wood, changed to purple by some drops of ammonia.

Derivatives of nitrotartaric acid.—M. Dessaignes, who devotes his leisure hours to science, has brought out several new facts of interest. He has observed that the nitrotartaric acid which he has obtained, decomposes spontaneously in water, producing an acid which he calls *tartronic acid*, $C_6H_4O_{10}$. Heated to 160° C., it loses carbonic acid and leaves a residue which appears to correspond to $C_4H_2O_4$; this in contact with potassa affords a salt, the acid of which has the composition $C_4H_4O_6$ and is identical with glycollic acid, extracted from the sugar of gelatine. This acid forms an *amid* which is not the sugar of gelatine although like it in composition. Glycollamid is an isomere of glycocoll, just as lactamid is an isomere of alanine. M. Dessaignes considers the insoluble substance $C_4H_2O_4$ as having the same relation to glycollic acid as lactid to lactic acid; and he hence calls it glycollid.

On the gluten of wheat.—M. Millon, compelled by his high military position to rather a nomadic life, has for some years suspended the fine researches which he had undertaken—researches on the oxydized compounds of nitrogen, chlorine, mercury, nitric ether, also on vegetable physiology, etc., which had given him a high rank among men of science. Removed from his laboratory and sent to Africa, for political

reasons, he has found the means of carrying on some important investigations without a chemical laboratory, and he has just now brought before the Academy a series of papers which he proposes to present, containing the results of some researches on wheat.

In his first memoir, he brings out the important fact that there are some kinds of wheat, of good appearance, *that contain no gluten.* His attention was called to the subject by the wheat of Guyotville (Algeria), which although appearing well, was nearly destitute of this important ingredient. He was thus led to examine a quantity of the wheat poor in gluten, and he found it to be a mixture of rich grains with others containing none of this albuminoid substance. Dough made from the wheat of Guyotville without gluten is worked with more difficulty than ordinary dough, and the bread is swallowed with some difficulty, like that which is dry or stale. The nitrogenized substance of this wheat is soluble in water.

In a second memoir, M. Millon takes up the chemical composition of different varieties of wheat, and he deduces from his results a distribution of the wheats—using terms already in use—into *tender wheat*, and *hard wheat*, the characters of which are as follows:

Tender wheat: Fracture white, opaque, and farinaceous, the starch escaping more or less abundantly; a more or less complete replacement of the gluten by a soluble albuminoid principle varying widely in the proportion of nitrogen.

Hard wheat: Fracture horny, semi-translucent, without a starch-like appearance; all the nitrogen existing under the form of gluten and the weight of it always a little superior to the quantity of albuminoid principal represented by the nitrogen; only small variations in the proportion of nitrogen, the amount of which is large. This last characteristic does not serve to distinguish the hard wheat, since it is not rare to meet with tender wheat containing as much nitrogen as the hard wheat, or even more.

Wheat intermediate between these two varieties, M. Millon names *semi-hard* wheat, which he describes as follows:

Fracture close and less horny than in hard wheat; whitish when crushed; a proportion of gluten mixed with the albuminoid principle; a large proportion of nitrogen, and this nearly constant.

These descriptions are completed by a mention of the external characters, taken from the volume, color, integuments, etc. His facts are derived mainly from the wheat of Algeria and those of the north of France, and it remains to make the results general, and applicable to wheat of whatever origin.

Natural History of Lupulin.—This proximate principle of Hops, was first examined by Dr. A. W. Ives, of New York; MM. Raspail, Payen, Chevallier and others, have since been occupied with it, without fully establishing its chemical constitution. M. Personne, Assistant at the School of Pharmacy at Paris, has taken up the subject, and given it a satisfactory solution.

He has found that when acted upon by boiling water, lupulin affords two groups of substances; one obtained by distillation with water and the other by means of steam.

The former consist of *valeric acid*, and of *valeral* or *valeric-aldehyde.* The matters which are volatilized only through the action of steam,

consist of an organic and a nitrogenized substance, which the author has now under examination.

Various Communications.—In the name of a Commission consisting of MM. Thenard, Balard and General Piobert, M. Balard read a very favorable Report on the researches of M. Violette on the carbonization of wood by means of over-heated steam, of which we gave an account in the number for September, 1853.—M. Payen read a memoir, demonstrating the presence of carbonate of lime in vegetables.—A naval officer, M. Tramblay, read a paper on a new apparatus for saving property at sea.—M. Thomas, of Colmar, presented an arithmometer upon which he had worked for thirty years, which, as appears, resolves completely the problem of calculating machines: we shall return again to it.—At each session of the Academy, communications flow in from the four quarters of the globe both on the subject of the Bréant legacy (100,000 francs, to the person who shall discover the cause and cure of cholera), and that of the prize proposed respecting the disease of the vine. The largest number of communications come from Germany. This earnestness is no doubt praiseworthy, and yet may excite a smile; for each of the authors pretends to have resolved the problem better than all others, and the number of panaceas discovered or announced now amounts to thousands.

In the year X. of the first Republic, the French Government founded a prize of 60,000 francs, to be given to the person who should through experiment and discovery, make a step in electrical science comparable with that of Franklin and Volta, to be adjudged by the Academy of Sciences. The fact of this offer has recently been called to mind by Madame Œrsted, who claims the prize in the name of her late husband, M. Œrsted. A commission consisting of MM. Pouillet, Becquerel, Despretz, Thenard and Regnault, have been charged with the examination of this demand. They find much embarrassment, since without contesting the merit of Œrsted's discovery, his is not the only important one made since the year 1801. The discoveries of Davy, Ampère, Arago, Faraday, Ohm, Morse, Wheatstone, Jacobi, the pile with a constant current discovered by M. Becquerel, the thermo-electric currents discovered by Seebeck,—all show that it would have been far easier to have awarded the prize in 1820 than at the present time.

Industrial Photometry.—An instrument by Mr. Babinet for exact photometric measurements, has for a long time been used, which is based on the neutralization of the tints of polarized light, as employed in photometry by Arago. We have not space for a figure and complete description of this small apparatus, which the ingenious Duboscq has rendered very portable and convenient for use. It consists essentially of a tube having a ground glass at one end, and at the other end an analysing prism of Iceland spar. A pile of plates of glass, serving as a polarizer is fixed to the tube so as to form with it an angle of 35°, the angle of polarization of the glass. The light diffused upon the ground glass, reaches the eye only after having passed across the pile of glass, and is consequently polarized by refraction perpendicularly to the plane of incidence of the rays.

The ground glass is for receiving the illuminations for comparison. It is successively illuminated by the lights to be compared. On traversing the pile of glass plates, the rays are brought into a condition to

color the four semicircles of a Soleil's polariscope, which is furnished with plates, having the property of double rotation. By means of a third source of light, a second ground glass is illuminated, which is placed obliquely so as to neutralize the colors produced by one of the luminous sources under comparison; then leaving this light fixed with relation to the instrument, the photometer is moved towards or from the second source of light, until the colors disappear anew. The relation of the square of the distances give, then, the relation between the intensities of the two lights. Different precautions are necessary when it is not convenient to vary the distance of the photometer from each of the sources of light, as, when they are two jets of gas, or of electric light, not easy of access. But it is impossible here to enter into these details.

On forming vessels of gold by the aid of phosphorus.—The property of phosphorus, of precipitating certain metals from their solution has long been known; and gold is among the number. M. Levol has used this process in forming gold vessels useful in chemical research. He takes the perchlorid of gold, and places in it, at the ordinary temperature, some phosphorus, moulded of a form convenient to serve as a nucleus for the vessel of gold. To give the phosphorus the desired shape, it is melted in a water-bath near 60° C. in temperature, within a vessel of glass having the form required. After cooling it, the phosphorus is taken out solid, from its envelop, breaking it, if it be necessary. The precipitation of the gold or the construction of the vessel is then begun; and it finally remains only to remove the phosphorus by re-melting it and washing by the aid of boiling nitric acid until the last traces are removed.

Gilding of Silk, &c.—Commerce has furnished for some time a kind of silk treated by a galvanoplastic method; the threads produce a costly fabric of wonderful solidity. The author of the process is M. Pouilly. He first metallises the silk, then covers it with a thin layer of copper and finally applies the gold by the aid of the galvanic battery.

Similar trials have been made within a few years by Madame Foa. The gold is dissolved in aqua regia, then dried by evaporation, then it is dissolved again in water, which is diluted with a large quantity of distilled water. The fabric under treatment was placed in this bath; the salt of gold fixes itself upon the ligneous fabric, and the compound is reduced by an exposure of the tissue to the action of hydrogen. This process has been abandoned, as it proved to be not uniformly successful, though without reason, as success once is enough to insure constant success after the causes of failure are studied out.

Local anesthesis.—The process invented by Mr. Harris enables the surgeon to render insensible the part of the body to be operated on, without affecting the rest of it. MM. Nélaton and Paul Dubois, two skillful surgeons of Paris have made with the apparatus the following experiments. After having for five minutes directed a jet of the vapor of chloroform on an abscess, Dr. Nélaton was enabled to make an incision into the hollow of the foot without the least sign of pain on the part of the person operated on. The following fact observed by Dr. Dubois is still more remarkable. A young woman had an abscess in the armpit and an ulcer on the wrist. She suffered much from both and could not use her arm. The first application of chloroform was made on the armpit. The tumor, although so painful that she could

not bear to have it touched, became so completely insensible, that it could be handled without pain, and the woman could raise her arm; the insensibility continued for three hours. The operation was not performed that day. When the abscess was at its head, the chloroformic fumigation was renewed, and almost immediately Dr. Dubois plunged the knife into the abscess without any sensation on the part of the patient; he then turned her attention from it, and after this there was no more feeling of pain in the region. Afterwards the small place on the hand, fumigated in the same way, also remained completely insensible.

These facts are strongly affirmed by two honorable physicians who also cite the witnesses to the operations. Other doctors have attempted to repeat the experiment; but, as is singular, *without success.* More study of the conditions necessary may lead to new and complete success; and we hope to announce such in our next communication.

Influence of bismuth on the ductility of Copper.—There has recently arrived from Australia a black copper in ingots possessing some peculiar properties. Although of high per-centage the color is bronze; it is but little ductile; the fracture is loose and crystalline, which may be removed by refining according to the ordinary methods.

M. Levol, Assayer at the Mint of Paris, has analyzed this copper before and after refining, with the following results:

	Crude.	Refined.
Copper,	99·400000	99·480000
Sulphur,	0·314000	
Lead,		0·362000
Silver,	0·100000	0·100000
Bismuth,	0·144000	0·048000
Gold,	0·000833	0·000833
Tin, Antimony,	*trace*	*trace*
Arsenic,		
Loss,	0·041167	0·008917

The lead and trace of arsenic proceeded from the process of refining; and it is found by experiment that the small proportion of antimony, arsenic, gold, silver and lead do not explain the want of ductility of the copper. The bismuth, then, only one-third of which had resisted oxydation, is the sole cause of the loss of ductility. M. Levol has proved the correctness of this conclusion by preparing different alloys. It is remarkable that bismuth which has so many points of resemblance to lead, should be so different in the above respect. It is important to examine for bismuth the coppers of commerce, in order to search out the cause of the peculiar mechanical and chemical qualities often found, even in copper of excellent appearance.

On the bronze employed in sheathing ships.—This subject has been alluded to in the November number of this Journal. Some new facts have been brought out by M. Bobbierre, confirming the results before announced; and he has established them by experiments. For the purpose of experiment, he has made with metals either pure or impure, ingots of bronze of a cylindrical form by castings in sand, having a height of 40 centimeters and weighing 25 kilograms. Portions for analysis were taken from different parts of the ingots, both from the

surface and interior. The central parts in all cases contained less tin than the surface. For example, in the alloy of 97 of copper and 3 of tin, the richness in tin for the two parts had the ratio of 1 to 3·97. On adding to the alloy 1 p. c. of zinc, the homogeneity was much increased, the ratio becoming 1 : 1·45.

Under Louis XIV, the cannon were of better quality than those of the present time; zinc was mixed with the metal, in the condition of *brass*. The trials made in our time have failed because the zinc was introduced directly into the alloy while in fusion, in which case the zinc is burnt off and forms no combination with the fused metal.

Traité des Poisons, ou Toxicologie appliquée à la Medicine legale, à la Physiologie et à la Therapeutique, par le Docteur CH. FLANDIN. 3 vols. in 8vo. Paris, chez Mallet-Bachelier.—The first volume of this work has been some time out and has been favorably received. The author does not admit of poisons in the sense ordinarily understood. Poisonous substances are not, as has been said, irritants, etc., but substances not capable of being assimilated, which penetrate into the organism by absorption and become an obstacle to chemical or physiological action on which life depends. Thus the pathological and therapeutic department of poisoning are distinct. These views characterize the work. All the processes for detecting poisons before and after inhumation are given with details, among which are some that have given a just reputation to M. Flandin, their discoverer.

Traité de l'Electricité théorique et appliquée; par AUG. DE LA RIVE. 2 vols. in 8vo. Paris. Baillière.—The first volume has just appeared and it announces that the second will treat particularly of the *applications of electricity*. This work is beyond doubt the most complete that we have on the subject, and the name of the author, de la Rive, is sufficient to assure us that the facts have been experimented upon by himself, and that nothing is given without good authority.

Mécanique Analytique par LAGRANGE, 3d edit., revue et annotée par BERTRAND. 2 vols. Paris, chez Mallet-Bachelier.—Lagrange published the first edition of his work in 1788. In 1811, the principles and general applications, contained in the first edition, were extended and completed. Some obscure points remained, which one of our youngest geometricians, Joseph Bertrand, Examiner at the Polytechnic School, has elucidated with skill, profiting by the progress which the science had made since that period. The edition of 1811 having been exhausted, the new edition has been received with much favor by geometricians and mechanicians.

Leçons sur la Theorie mathematique de l'élasticité des corps solides, par LAMÉ; 1 vol. 8vo, avec planches. Paris, chez Mallet-Bachelier.—M. Lamé, one of the most learned geometricians of the Academy of Sciences, has succeeded in rendering the mathematical theory of elasticity as exact and as rigorous as rational mechanics. This work is addressed to engineers, physicists, geologists, and especially to practical men in these arts, who will find in it, rigorous laws on the action of elastic forces, and on the best plans for constructions.

La règle à calcul expliquée, ou guide du calculateur à l'aide de la règle logarithmique à tiroir, par BENAIT. 1 vol. in 12mo, avec planches. Paris. Ches Mallet-Bachelier.—This work treats of the methods of constructing the sliding computing scale, and of its use in calculations.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Artificial production of polychroism in crystallized substances.*—Senarmont has communicated to the Academy of Sciences the results of experiments upon this subject which are very unexpected and important. The capital fact which the author has discovered is, in his own words, expressed as follows: "A coloring matter disseminated continuously in the interior of a crystal, between its layers of increment but absolutely foreign to its substance, chemically inert and capable of being spontaneously eliminated by successive solutions and crystallizations in pure water, may nevertheless communicate to it in the highest degree the properties of polychroism, and an energy of absorbing action comparable if not superior to that of substances naturally colored in which it shows itself in the most decided manner." As a proof of the correctness of this assertion, the author exhibited large crystals of nitrate of strontia formed in a concentrated tincture of campeachy wood rendered purple by a few drops of ammonia. In these crystals white light developed by transmission under certain incidences, a red color, and under others a blue or violet. Observed with a doubly refracting prism the crystals gave two images, the one red and the other dark violet, according to the thickness, and these images exchanged their colors, passing through identity, as the crystallized plate was made to turn in its own plane. Two similar and perfectly transparent laminæ superposed with a parallel orientation allowed a portion of the incident white light to pass with a purple color; superposed with a right angled orientation, they arrest the light like tourmalines, or at least reduce it to a violet shade so obscure that we may consider the light as extinct. Finally we may detach from these crystals perfectly pure and homogeneous plates slightly inclined to the optic axes. By placing such a plate very near the eye and using white natural light, we see alternately in the direction of each of the axes, a brilliant orange spot traversed by a hyperbolic branch. These open to the right and left of the principal section under the form of curved brushes composed of two equal parts of violet and sombre blue and dividing the field of the plate into two regions in which the purple tints regularly degenerate on both sides of their common limit. The dark tufts interrupted by the luminous spot are moreover fringed towards the point with a little yellow and blue, colors which are altogether local, and which arise manifestly from the dispersion of the optic axes corresponding to the different colors. These phenomena are characteristic of polychroism in crystals with two optic axes, and perfectly similar to those which Brewster observed in Cordierite, and Haidinger in Brazilian Andalusite. The author obtained similar results with other coloring matters and other crystals, and promises more ample details hereafter.—*Comptes Rendus*, xxxviii, 101, *Janvier*, 1854.

[*Note.*—The results obtained by Senarmont clearly demonstrate that the existence of polychroism in crystals by no means necessarily implies their *chemical* homogeneity, since this polychroism may in any

case be produced by the mechanical admixture of foreign coloring matter. Is it not possible that the difficulty of expressing the composition of certain minerals by chemical formulas may arise from such an admixture of coloring matter, the optical characters of these minerals having hitherto entirely misled us?—W. G.]

2. *Rate of transmission of impressions made upon the nerves.*—HELMHOLTZ has communicated to the Physico-Agricultural Society of Königsberg a paper on the methods of measuring very small portions of time, and on their application to physiological purposes. The author alludes in the first place to the remarkable difference observed by astronomers between the observations of different individuals and termed by them the personal equation. The measurements of each observer agree well among themselves, but differ more or less by a constant quantity from those obtained by other persons. The apparatus of Siemens for the measure of the velocity of a musket or cannon ball is next described in its general features. This apparatus only differs from the common revolving cylinder and point now so well known in this country, by the arrangement of this point, which is here fixed, and presses against the surface of the cylinder: when an electric spark passes from the point to the cylinder it leaves a fine dark spot. The passage of the ball establishes metallic connection so that the discharge takes place at the instant of this passage. In this manner the time during which the ball traverses a space of half a line may be measured. The author next alludes to the principle of the revolving mirror due to Wheatstone, and used with so much success by Fizeau and Foucault. Finally he gives an account of the method of Pouillet as modified and used by himself. This consists in causing the galvanic current to act upon an oscillating magnet, observing this magnet by Gauss and Weber's method, and determining the constant factor necessary to convert differences of oscillation into differences of time. In this manner accurate determinations could be made up to the $\frac{1}{10000}$th of a second of time. The physiological questions which the author sought to solve were these. In the transmission of intelligence, is a measurable time necessary for the ends of the nerves to communicate to the brain the impression made upon them; and on the other hand, is time required for the conveyance of the commands of the will from the brain to a distant muscle? By operating with the muscle of a frog severed from the body of the animal but connected with the nerves proceeding from it, the author found that the activity of the muscle is by no means instantaneous, but appears sometime after the excitation of the muscle, increases gradually to a maximum, and then sinks, first quickly, afterwards slowly, so that while the greater part disappears in about one-third of a second, the remaining portion requires several seconds afterwards. The object was to show that the different stages of activity of the muscle take place later when the excitation has to pass through a greater length of nerve, and this is actually the case. The most probable value of the velocity of propagation in the motor nerves of the frog was found to be 26·4 metres or about 80 feet per second. The results of the author's experiments upon the human subject were as follows: The intelligence of an impression made upon the ends of the nerves in communication with the skin is transmitted to the brain with

a velocity which does not vary in different individuals, nor at different times, of about 60 metres, or 195 feet per second. Arrived at the brain an interval of about $\frac{1}{10}$th of a second passes before the will, even when the attention is strung to the uttermost, is able to give the command to the nerves that certain muscles shall execute a certain motion. This interval varies in different persons, and depends chiefly upon the degree of attention. It varies also at different times in the case of the same person. When the attention is lax, it is very irregular, but when fixed very regular. The command travels probably with the above velocity toward the muscle. Finally about the $\frac{1}{100}$th of a second passes after the receipt of the command before the muscle is in full activity. In all therefore from the excitation of the sensitive nerves till the moving of the muscle $1\frac{1}{4}$ to two-tenths of a second are consumed. [For more ample details and for much matter of a highly suggestive character, we must refer to the original lecture.]—*L. & E. Phil. Mag.*, *Nov.*, 1853.

3. *Preparation of large crystals of sulphate of iodo-quinine for optical purposes.*—HERAPATH has succeeded in obtaining crystals of this very interesting and valuable substance of sufficient size to be substituted for tourmaline in polarizing light. For the details of the process however, we must refer to the original paper.—*L. & E. Phil. Mag.*, *Nov.*, 1853.

4. *On the law of induction in magnetic and paramagnetic substances.*—PLUCKER has communicated an elaborate memoir on this subject from which we shall content ourselves with abstracting the summary of results. These are as follows:

(1.) In all magnetic and diamagnetic substances the same general law gives the intensity of the induced magnetism as a function of the exciting force. For each substance this law is particularized by the values of two constants. Of these two constants the one gives, as the inducing force vanishes, the ratio of this force to the induced magnetism (constant of induction), and the second determines the resistance which prevents the induced magnetism from increasing proportional to the inductive force (constant of resistance).

(2.) For every substance there is a point of saturation to which it constantly approximates as the inducing force increases.

(3.) Diamagnetic substances (Bismuth, Phosphorus,) so far as the law of the intensity of their excitation is concerned, behave precisely like magnetic substances, though they exert a directly opposite action upon the inducing magnetic pole. This similar behavior compels us, in my view, to assume that the condition of a diamagnetically excited body is in itself in no wise different from the condition of a magnetically excited body, that furthermore polarity is also present in the excitation of diamagnetic substances, but that this is called forth by an induction which is the opposite to that which occurs in magnetic bodies.

(4.) The curves which represent the law of induction for diamagnetic substances are surrounded on both sides by the curves for magnetic substances. They show that the resistance which is opposed to the excitation of diamagnetic bodies is less than in most magnetic substances, but by no means vanishes; on the contrary it is greater than in oxygen and hydrate of oxyd of cobalt.

(5.) There can be no assumption of a specific magnetism in a substance in a general signification as we speak of specific gravity. Cobalt is precisely as magnetic as iron, when we use a definite magnetic force, which in intensity lies between that produced by one and that produced by two Grove's cups. Iron is more strongly magnetic with a greater, cobalt more strongly magnetic with a less magnetic force. If we compare the specific magnetism of hydrate of oxyd of cobalt with that of hydrate of oxyd of nickel, we obtain numbers which differ from each other by the multiple 2¼, according as we use a single element or a battery of 16 elements. The specific magnetism (iron as the point of comparison) of the hydrate is with the last exciting force twice as great as with the first. For oxygen and bismuth this ratio descends to about 1·9 and 1·8. This partly explains the fact that while Faraday's estimate of the magnetism of oxygen, with a nearly equal force and similar mode of observation, corresponds well with my own, other physicists find much smaller numbers for oxygen and bismuth.

(6.) The alternation between magnetic attraction and diamagnetic repulsion in mixtures of magnetic and diamagnetic substances, is completely explained by the change of the specific magnetism with the magnitude of the inducing force. When we mix 10 million parts of bismuth with 310 parts of iron, and fill a small flask with the mixture, we observe neither attraction nor repulsion if we apply a current which is twice as powerful as that which corrresponds to a single Grove's element; with a less powerful current the mixture is attracted, with a more powerful one it is repelled. We can calculate the repulsion like the attraction for a given force. When we use a single Grove's cell, the attraction is nearly one-third as great as the repulsion which, with an equal force, would occur if the whole vessel were filled with bismuth. By employing a battery of 6 elements, we obtain in place of the attraction just determined a nearly equal repulsion. With a battery of 16 elements we obtain finally a repulsion about ⅘ths of that which pure bismuth experiences with a single element. These results would appear yet more striking if bismuth were mixed with nickel instead of iron. They would make their appearance in the opposite order if the metal were mixed with hydrate oxyd of cobalt.

(7.) Those substances which oppose a lesser resistance to magnetization appear also more easily to retain the magnetism once received. At least we find it asserted of pure nickel that it retains no magnetism while one hydrate of oxyd of cobalt does this; the same is the case with oxygen, which as the author first remarked is attracted or repelled at pleasure by changing the poles of the electro-magnet when contained in an indifferent glass sphere.

In conclusion the author promises us a memoir on the nature of the coercive force.—*Pogg. Ann.*, xci, 1, *Jan.*, 1854.

5. *On the laws of the attraction of electro-magnets.*—Dub has published a continuation of his interesting and valuable researches on magnetic forces. Before stating however the results of this investigation, we must premise that the author distinguishes between magnetism, attraction, and sustaining power in electro-magnets. By magnetism he understands the magnetic excitation of a piece of soft iron by the galvanic current; Lenz and Jacobi measured this by means of the induced

current excited by the vanishing of the magnetism, to which it is proportional. When a second bar of soft iron is caused to approach the first, this also becomes magnetic and by n-fold *magnetism*, n^2 times the *attraction* is produced. As however the author's experiments have shown that in immediate contact the attraction is not proportional to the squares of the currents it is clearly necessary to distinguish between these two cases of attraction in contact and attraction at a distance. The author gives the following summary of his results:

(1.) The attraction of U-shaped electro-magnets with an equal number of windings of the electro-magnetic spirals is proportional to the squares of the magnetizing current force.

(2.) The attraction of U magnets is, with equal currents, proportional to the square of the number of windings of the magnetizing spirals.

(3*a*.) The attraction of U magnets is proportional to the square of the current-force multiplied by the square of the number of windings.

(3*b*.) The *attraction* and the sustaining force both of straight magnets and U magnets is proportional to the square of the current-force multiplied by the square of the number of windings.

(4.) The magnetism of massive cylinders of iron of equal length, which are magnetized by galvanic currents of equal force, and by spirals of an equal number of windings closely surrounding the core, is accurately proportional to the square roots of the diameters of these cylinders.

(5.) For the particular case in which the surface of contact does not disturb the result, the attraction and the sustaining force are, with equal magnetizing forces, proportional to the diameters of the bar or U magnets.

(6.) The attraction of bar and U electro-magnets, with equal magnetizing forces, increases the nearer the whole of the windings are to the poles.

(7.) The attraction, like the sustaining force of U electro-magnets, *ceteris paribus*, remains the same whatever be the distance of the branches of the magnet.

(8.) The length of the branches of a U electro-magnet has no influence on its attractive or sustaining force if the windings of the spiral surround its whole length.

In addition to these laws the author has found that the attraction which a helix or spiral exerts upon a soft iron bar placed in its axis follows the same law as an electro-magnet, so that we have

(9.) The attraction of a spiral is proportional to the square of the magnetizing current multiplied by the square of the number of windings.—*Pogg. Ann.*, xc, 248, 436, *Oct. and Nov.*, 1853.

6. *Identity of Niobium and Pelopium.*—H. Rosé has communicated to the Royal Academy of Sciences at Berlin a memoir in which he admits the identity of these metals. The author alludes in the first place to the extraordinary difficulties which have accompanied his investigations from their outset several years since. In earlier memoirs the analogy between pelopic and tantalic acids had been pointed out, as well as the marked difference between pelopic and niobic acids. Continued researches have, however, completely established the fact that pelopic and tantalic acids are essentially different, while between

niobic and pelopic acids there exists a remarkable and unexpected connection. The remarkable peculiarities of the chlorids of niobium and pelopium long since attracted the author's attention. It will be remembered that the only method yet given for the separation of these metals consisted in taking advantage of the difference in the volatility of their chlorids. Chlorid of pelopium being orange red and more volatile than chlorid of niobium. Every time, however, that the white chlorid of niobium was converted into niobic acid, mixed with carbon and heated in a current of chlorine, a quantity of the orange red chlorid of pelopium was formed, and it finally appeared after innumerable most careful and laborious experiments, that the one chlorid became converted into the other by absorbing or losing chlorine; chlorid of pelopium always yielding chlorid of niobium in greater or less quantity. A small quantity of niobic acid considered perfectly pure was mixed with a very large quantity of sugar and gradually carbonized. The carbonaceous mass was again mixed with sugar and charred, and this operation was repeated three or four times to obtain the most intimate mixture of the acid with the carbon. The mixture was then introduced into a glass tube, a current of dry carbonic acid gas passed over it to expel all the air and then heated in a rapid current of chlorine gas. Under these circumstances the niobic acid yielded no chlorid of niobium but only the purest yellow chlorid of pelopium. After several experiments the author found that in order to obtain pure chlorid of pelopium from niobic acid the following precautions were necessary. 1. A large proportion of carbon in comparison with the acid applied. 2. A very careful expulsion of all moisture by strongly igniting the mixture in dry carbonic acid. 3. A complete expulsion of the carbonic acid after the mixture had been cooled in this gas by a very rapid current of chlorine which was only introduced after all the atmospheric air had been driven out of the chlorine apparatus. 4. Finally a very gentle heating after all the parts of the apparatus were so filled with chlorine that they appeared intensely green. The decomposition of the chlorid of pelopium by water yields pure pelopic acid. When any one of the above mentioned precautions is omitted, we obtain at once the white and the orange chlorids. From these experiments it is clear that niobic and pelopic acids are derived from one and the same metal. It is however very remarkable and wholly without analogies among other oxyds that the two acids cannot be transformed into each other by oxydation or reduction. As the quantity of chlorine in the so-called chlorid of pelopium is greater than in the chlorid of niobium, it follows that the two oxyds resulting from these chlorids are different stages of oxydation, and not merely isomeric or allotropic. The author does not give the proportion of the oxygen in the two acids, but simply states that it is very anomalous and only to be compared with the oxygen ratio in two of the acids of sulphur. As the two acids have the same metallic radical it becomes necessary to select a single name, and the author chooses niobium. The former pelopic acid now becomes niobic acid as the highest state of oxydation, but the author does not propose any name at present for the former niobic acid.—*Pogg. Ann.* xc, 456, *Nov.*, 1853.

7. *Preparation and Properties of metallic Aluminum.*—St. Clair Deville has communicated a memoir on aluminum which contains some new facts but which does not add enough to our positive knowledge to justify the extraordinary flourish of trumpets with which the communication was made and received. The metal was prepared by Wöhler's method, namely, by heating the chlorid with sodium, and afterwards fusing the globules into one mass under the mixture of common salt and chlorid of aluminum. As thus prepared it was silver-white, malleable and ductile, and had the fusing point of silver. Its hardness was increased by hammering but it again became soft on heating. Its density was 2·56; it was a good conductor of heat and could be fused and poured out in the air without becoming sensibly oxydized. Aluminum is completely malleable in air dry or moist. Sulphuretted hydrogen, hot and cold water, nitric acid weak or concentrated, and dilute sulphuric acid have no action upon it. Its true solvent is muriatic acid with which it evolves hydrogen, sesquichlorid of aluminum being formed. Heated to redness in muriatic acid gas it yields dry and volatile sesquichlorid. The author stated that the chlorid of aluminum was acted upon by common metals at high temperatures and hoped that further experiments would point out a simple and cheap method of procuring in large quantities and at a low rate a metal so likely to be useful in the arts. The Academy unanimously voted that a sum of money should be placed at the disposal of M. Deville to aid him in the prosecution of his experiments.—*Comptes Rendus, Feb. 6th*, 1854.

[*Note.*—The properties of aluminum as described by Deville, differ in some particulars from those of the metal obtained by Wöhler. Thus Wöhler's metal slowly evolved hydrogen when placed in boiling water and was very readily dissolved by a dilute solution of caustic potash. Further investigations will doubtless explain this difference. The preparation of metallic aluminum in quantity is particularly interesting in a purely chemical point of view, since it will enable us to determine by direct oxydation the equivalent of aluminum which is by no means to be regarded as satisfactorily determined at present.—W. G.]

8. *Preparation of Aluminum by electric deposition.*—Gore has addressed to the editors of the L. and E. Phil. Mag., a letter in which he asserts that aluminum and even silicon may be thrown down from their solutions in a coherent state by feeble electric currents. Hydrate of alumina was dissolved in muriatic acid and a porous cup containing dilute sulphuric acid placed in the mixture. A plate of zinc was placed in the acid and a plate of copper in the solution of alumina, the two being connected by a wire. After a few hours a lead colored deposit of aluminum (?) was obtained, which when burnished resembled platinum. The result was hastened by warming the solutions, and the experiment succeeded with other solutions of alumina. With a small Smee's battery the effect was produced much more rapidly.

When a solution of soluble glass was employed, a single Smee's battery threw down a nearly silver-white metallic film which the author considered to be silicon, but which certainly possesses no analogy with the silicon obtained by purely chemical processes.—*L. and E. Phil. Mag., March*, 1854.

9. *On the arsen-ethyls.*—LANDOLT has studied the combinations of ethyl with arsenic, which are obtained by the action of iodid of ethyl upon an alloy of arsenic and sodium. Of these compounds two, namely, arsenbiethyl and arsentriethyl, are to be regared as analogues of kakodyl, while the third, arsenethylium, is an ammonium in which nitrogen is replaced by arsenic and hydrogen by ethyl. We omit the details of the mode of preparing and purifying these interesting compounds, and pass to the description of their constitution and properties. Arsenbiethyl is a faintly yellow colored liquid, which strongly refracts light, and which possesses an extremely disagreeable penetrating odor of garlic. It boils between 185° and 190° C. Exposed to air it bursts into pale flame, giving out vapors of arsenous acid. When the combustion is imperfect, a red substance corresponding to Bunsen's Erytharsin is always produced. The solution of arsenbiethyl reduces the solutions of the noble metals immediately. The radical unites quietly with the haloids as well as with oxygen and sulphur; its constitution is represented by the formula $As(C_4H_5)_2$, and it consequently corresponds to kakodyl, ethyl here replacing methyl. All the compounds of this radical are fluid, and all possess the same offensive and persistent odor. Arsentriethyl is also a colorless liquid, soluble in ether and alcohol, but insoluble in water. It fumes in the air, but does not generally inflame unless gently heated. The solution does not reduce the oxyds of the noble metals. The formula of this radical is $As(C_4H_5)_3$; it unites with 2 equivalents of oxygen, sulphur, chlorine, &c., and its oxyd saturates two equivalents of acid. The oxyd is a colorless oily liquid. Arsenethylium is formed when iodid of ethyl is brought into contact with arsentriethyl. The radical unites with 1 equivalent of chlorine, iodine, bromine, &c., and gives with 1 equivalent of oxygen a powerful base which forms with acids both acid and neutral salts. These compounds are remarkably susceptible of crystallization, and remain unchanged in the air. They are all without smell, have a bitter taste, and do not appear to possess poisonous properties. The oxyd of arsenethylium is a white mass which has a strong alkaline reaction, separates ammonia from its combinations in the cold, and precipitates the earths and heavy oxyds from their salts. The formula of the crystallized chlorid is $AsAe_4Cl+8aq$. The bisulphate has the formula $AsAe_4O, SO_3+HO, SO_3$.—*Journal für praktische Chemie*, lx, 385.

10. *On the alcohol of benzoic acid.*—CANNIZARO has found that the oil which results from the action of an alcoholic solution of caustic potash has the constitution $C_{14}H_8O_2$. It is colorless, heavier than water, refracts light strongly, and boils at 204° C. In its relations to reagents it behaves like an alcohol the aldehyd of which is represented by oil of bitter almonds. By the action of nitric acid at a gentle heat the new alcohol is converted into oil of bitter almonds; the action of chromic acid converts it into benzoic acid. The vapor of the alcohol passed over red hot platinum sponge yields an oil which is specifically lighter than water and is probably $C_{14}H_6$. By passing muriatic acid gas into the alcohol the liquid separates into two layers, of which the upper is the chlorid $C_{14}H_7Cl$. This is a highly refracting, strong-smelling liquid, heavier than water, and boiling between 180° and 185°. With caustic potash it gives chlorid of potassium and the alcohol is regene-

rated. Warmed with an alcoholic solution of ammonia, the chlorid gives sal-ammoniac and a crystallizable base which fuses at a higher temperature than Toluidin. By mixing a solution of the alcohol in acetic acid with a mixture of sulphuric and acetic acids, an oil is obtained which is the acetic ether of the new radical, $C_{14}H_7O + C_4H_3O_3$. This is a colorless liquid, having an aromatic odor, and boiling at 210°. With caustic potash it yields acetic acid, and the alcohol.—*Ann. der Chemie und Pharmacie*, lxxxviii, 129, *October*, 1853.

11. *Formation of nitruret of benzoyl from hippuric acid.*—Limpricht and von Uslar have found that when hippuric acid is heated in a tubulated retort, the acid fuses at 130°, gives off a little benzoic acid at 210°, and boils at 240°. The only volatile products of this distillation are benzoic acid, colored faintly red, traces of prussic acid, and a liquid which proves to be nitruret of benzoyl, $C_{14}H_5N$. The constitution and properties of this body correspond perfectly with those of the nitruret obtained by Fehling by the distillation of benzoate of ammonia.—*Ann. der Chemie und Pharmacie*, lxxxviii, 133.

W. G.

12. *On the exhibition of the fixed lines of the solar spectrum with ordinary flint glass prisms;* by Ogden N. Rood.—Herschel in his treatise on light, when speaking of the method of displaying the fixed lines in the solar spectrum remarks: "The slightest defect of homogeneity in the prism, however, as may readily be imagined, is fatal. With glass prisms of our manufacture it would be quite useless to attempt the experiment." This would appear to account for the generally diffused opinion, that the fixed lines in the solar spectrum cannot be seen except by means of "a prism of incomparable flint glass." Not being possessed of a prism of even *comparable* flint glass, I was led to try what could be done with articles of poorer quality. Two flint glass prisms having angles of 60°—such as are ordinarily sold by opticians, and filled with striæ from one end to the other as such prisms almost invariably are, with the extra addition of numerous scratches on the surface and a polish somewhat dimmed—were employed. The prism was placed twelve or fifteen feet distant from a slit $\frac{1}{30}$th of an inch in diameter, through which sunlight was admitted; the room not being darkened. In this manner about twelve or fifteen lines could plainly be seen by the naked eye: the principal of these were D in the orange, F and another line in the green, the strong line in the blue, and two very strong lines in the violet: by more careful manipulation with the prism about thirty lines could be seen and counted. When this spectrum was magnified by a small telescope placed behind the prism, with the exception of the red space the spectrum was filled with lines from one end to the other. It must be remarked however, that very much more careful manipulation is required when employing the telescope: it sometimes being necessary to work for thirty minutes before defining the lines, whereas with the naked eye half a minute will suffice. The angular inclination that succeeds best with the naked eye will not answer for the telescope: that is, for the naked eye the prism should not be placed at its angle of minimum deviation, but the spectrum should rather be expanded.

Light from a white cloud was next employed with the same prism, the slit having a diameter of $\frac{1}{20}$th of an inch, and being distant from the prism about 12 feet. D, F, with three or four of the lines to the left of F, and the strong line in the blue, could thus easily be made out with the naked eye: when magnified by the telescope about forty lines could be counted. In each case, the lines were shown to a number of persons, and in all the cases enumerated except one, colored drawings were made on the spot. With these two prisms fixed lines could be seen in the spectrum from a strip of white paper $\frac{1}{20}$th of an inch in diameter, placed on a black ground and illuminated by sunlight; or better in the spectrum from a white cord $\frac{1}{20}$th of an inch in diameter stretched across a black ground and illuminated by sunlight. In this manner, D, F, with several other lines in the green, and the strong line in the blue, can easily be made out, the prism being held in the hand before the eye, and parallel to the cord.

In order to ascertain the degree of imperfection necessary to prevent prisms showing the fixed lines, thirteen prisms taken indiscriminately from candelabra, and for optical purposes execrable, as all such ornamental prisms proverbially are, were examined. These prisms were ground at angles of 45° and 90°, but were so filled with striæ that *none* of them would give a defined image by total reflexion, and some would scarcely give an image that could be recognized. Sunlight was employed, the slit, &c. being as above: to the naked eye one of the prisms exhibited five fixed lines, two of the prisms showed four lines, four of the prisms showed three lines, four showed two lines, and the remaining two showed one line.

When the light from a white cloud was used, eleven of these prisms showed two lines, viz. F and the strong line in the blue; two showed three lines, viz. D, F, and the line in the blue. In examining the light from the clouds these prisms were not placed at more than four feet from the slit.

It would seem from these experiments then, that the real difficulty is to find a flint glass prism which will *not* exhibit some of the fixed lines, provided its dispersive power is sufficiently high.

New Haven, Feb. 28, 1854.

II. Mineralogy and Geology.

1. *Appendix to Observations on the Homœomorphism of some Mineral Species*;* by James D. Dana.—In order to exhibit the true crystallographic relations of hexagonal and dimetric mineral species, they are presented together in the following tables. The hexagonal species are in five groups, and the relations of these groups are shown by the symbols of planes at the head of the column.

As dimetric and hexagonal forms are alike in having the lateral axes equal, the relation between the crystals of these two systems are best shown by the inclination of the base of the prism on a pyramidal plane. The calculated axes deduced from these angles may diverge, when these angles are the same, since in one system the lateral axes are regarded as crossing at angles of 60° and in the other at angles of 90°.

* Page 210 of this volume.

If we conceive of an ellipsoid of revolution tangent at its sides to the several faces of the prism, as the molecule of each form, it is plain that the angles alluded to afford a correct exhibition of the relations of such molecules as respects their dimensions, and therefore a true representation of the relations of the forms.

There are blanks in the Tables, which are easily filled by calculations according to the usual methods in crystallography.

Very many of the cases of homœomorphism here shown, both in the dimetric system and the hexagonal, and also in the groups on pages 211 to 213, have been brought out from time to time by different authors. The writer has simply endeavored to extend somewhat the groups, and exhibit their relations. The Corundum group was first brought to its present extent by the investigations of G. Rose, who added to it the rhombohedral metals, with tetradymite and red zinc ore. The earliest recognition of the relations of corundum, specular iron and ilmenite, was made by the distinguished chemist and mineralogist of Munich, Prof. von Kobell, in an important paper in the Jahrbuch der Chemie und Physik of Dr. F. W. Schweigger-Seidel for 1832,* in which he was also the first to point out the homœomorphism of apophyllite and anatase; of copper pyrites, braunite, scheelite, scheeletine, wulfenite, cerasine, idocrase, uranite and mellite, besides other facts of interest.

The following important conclusions flow from the Tables.

A. Hexagonal System.—(1.) The vertical axis in Section II is to that of Section I as $1\frac{1}{2}:1$. The species of the two Sections may be regarded as closely related in form if not homœomorphous.

(2.) The vertical axis of Section IV is to that of Section I nearly as $3:1$, showing again a simple mathematical relation, if not pointing to actual equality.

(3.) The fundamental pyramid of Section V, is nearly identical in angle with the intermediate pyramid 2-2 of Section I, $\frac{4}{3}$-2 of Section II, in $\frac{2}{3}$-2 of Section IV.

(4.) The Rhombohedral angle of Section IV, is near 90°—or mostly between 84° and 88°; and this approximation to the angle of a cube is its *characteristic*. The pyramid 1(1P) should consequently approach 109° 28′, (the angle of a monometric octahedron,) in its basal angle; it actually varies for the species from Corundum to Iridosmine from 113° to 117°.

(5.) In Section V, the basal angle of pyramid 1 is near 90°, and this appears to be the *characteristic* of the species. Hence the Rhombohedral angle of the Calcite series is near 105°. Hence this rhombohedral angle is a common one among rhombohedral species.

(6.) Section III in its rhombohedral angle approaches 90° as nearly as Section IV, but differs in having the angle greater than 90° instead of less. Its vertical axis is most nearly related to that of Section I, it being approximately *twice* as long as in that Section.

* Neues Jahrbuch der Chemie und Physik, of Dr. F. W. Schweigger-Seidel, Band iv, Heft. 7, 1832; paper entitled, *Beitrag zur isometrischer und homöometrischer Krystallreihen.* The writer's attention has been directed to this paper since the results on page 210 of this volume were published; but he has not yet been able to consult it, except at second hand.

HEXAGONAL SYSTEM.

I.	O : 1	O : 2	O : 3	O : 1-2	O : 2-2	O : 4-2
Coquimbite,	151° 00′					
Beryl,	150° 3′	130° 57′	120° 3′	153° 29′	135° 4′	116° 37′
Pyrosmalite,	148° 30′	129° 13′	118° 33′	152° 3′	133° 18′	115° 13′
Eudialyte (R),	148° 38′	129° 21′	118° 40′	152° 10′	133° 27′	115° 20′
Dioptase (R),	148° 38′	129° 21′	118° 40′	152° 10′	133° 27′	115° 20′
II.	**O : $\frac{2}{3}$**	**O : $\frac{4}{3}$**	**O : 2**		**O : $\frac{4}{3}$-2**	
Apatite,	150° 35$\frac{1}{2}$′	131° 35′	12 ° 36′		135° 41′	
Levyne (R),	150° 43′	131° 43′	120° 44′		135° 50′	
Mimetene,	150°	130° 53′	120°		135° 0′	
Pyromorphite,	150° 27′	131° 26′	120° 28′		135° 32′	
III.	**O : $\frac{1}{2}$**	**O : 1**	**O : $\frac{3}{2}$**	**O : $\frac{1}{2}$-2**	**O : 1-2**	**O : 2-2**
Quartz (R),	147° 35′	128° 13′	117° 42′	151° 12′	132° 17′	114° 27′
Chabazite (R),	148° 32′	129° 15′	118° 35′	152° 5′	133° 20′	115° 15′
Dreelite (R),	147°					
Susannite (R),	147° 26′	128° 03′		151° 3′	132° 7′	114° 19′
Cinnabar (R),	146° 32′	127° 06′		150° 13′	131° 8′	113° 35$\frac{1}{2}$′
Zinc,		126° 57′				
IV.	**O : $\frac{1}{3}$**	**O : $\frac{2}{3}$**	**O : 1**	**O : $\frac{1}{3}$-2**	**O : $\frac{2}{3}$-2**	**O : $\frac{4}{3}$-2**
Corundum (R),	152° 19′	133° 37′	122° 26′	155° 34′	137° 44′	118° 49′
Specular Iron (R)			122° 30′	155° 37$\frac{1}{2}$′	137° 49′	118° 53′
Ilmenite (R),			122° 30′	155° 37$\frac{1}{2}$′	137° 49′	118° 53′
Willemite (R),			122° 18′	155° 27′	137° 35′	118° 41$\frac{1}{2}$′
Phenacite (R),			123° 16′	156° 15′	138° 39′	119° 36′
Bismuth (R),			123° 36′	156° 31′	139° 1′	119° 55′
Arsenic (R),			122° 9′	155° 20′	137° 26′	118° 34′
Antimony (R),			123° 32′	156° 28′	138° 56′	119° 51′
Tellurium (R),			122° 24′	155° 33′	137° 42′	118° 48′
Iridosmine (R),			121° 33′			
Tetradymite (R),			118° 38′			
Red Zinc Ore,			123° 5′	156° 5′	138° 27′	119° 26′
Copper Mica (R),			124° 9′			
Brucite (R),			119°			
V.	**O : $\frac{2}{3}$-2**	**O : $\frac{4}{3}$-2**	**O : 2-2**	**O : $\frac{1}{2}$**	**O : 1**	**O : 2**
Pyrrhotine,	149° 53′		119° 53′	153° 20′	134° 52′	116° 28′
Greenockite,				154° 32′	136° 23′	117° 42′
Breithauptite,				153° 38′	135° 15′	116° 46′
Copper Nickel,				154° 41′	136° 35′	117° 51′
Iodid of Silver,*				154° 49′	136° 46′	118° 0′
Nepheline,				154° 13$\frac{1}{2}$′	136°	117° 22$\frac{1}{2}$′
Cancrinite,				154° 7′	135° 52′	117° 16′
Parisite,					($\frac{1}{3}$) 136° 33′	($\frac{1}{2}$) 117° 50′
Tourmaline,	149° 10′			152° 40′	134° 3′	115° 49′
Calcite (R),	150° 20′			153° 45′	135° 23′	116° 53′
Dolomite (R),				154° 20′	136° 8′	117° 28′
Magnesite (R),				154° 57′	136° 56′	118° 9′
Spathic Iron (R)				154° 43′	136° 37′	117° 53′
Smithsonite (R),				155° 2′	137° 3′	118° 24′
Dialogite (R),				154° 37′	136° 31$\frac{1}{2}$′	117° 48′
Pyrargyrite (R),				155° 32′	137° 42′	118° 47′
Proustite (R,)				155° 7′	137° 9′	118° 19′
Millerite (R),					142° 44′	

* Relation to Greenockite is shown by Descloizeaux, Ann. d. Ch. et de Phys., [3], xl.

DIMETRIC SYSTEM.

I.	O : 1	O : 2				O : 3
Tin,	151° 24′	182° 31′				121° 26′
II.	O : $\frac{2}{3}$				O : 1	O : 2
Zircon,	148° 52′				137° 50′	118° 54′
Rutile,					137° 40′	118° 46′
Cassiterite,					136° 26′	117° 43′
Œrstedite,						
III.					O : 2*i*	O : 4*i*
Scapolite,					138° 38′	119° 36′
Sarcolite,					138° 25′	119° 24′
Meionite,					138° 43′	119° 40′
Mellilite,					137° 43′	118° 48′
IV.	O : $\frac{1}{3}$				O : $\frac{1}{2}$	O : 1
Apophyllite,	149° 30′				138° 32′	119° 30′
Nagyagite,					137° 30′	118° 37′
Uranite,					137° 27′	118° 35′
Anatase,					138° 23′	119° 22′
Matlockite,					138° 37′	119° 34′
Calomel,					138° 56′	119° 51′
Hausmannite,					140° 17′	121° 3′
V.		O : 1*i*	O : 1			
Romeine,		134° 17′	124° 35′			
Cerasine,			123° 06′			
Chiolite,			123° 17′			
Braunite,			125° 40′			
Idocrase,			(2) 123° 27′			
Copper Pyrites,		135° 25′	125° 40′			
? Tin Pyrites,						
Mellite,		(1) 133° 27′	123° 16′			
VI.		O : $\frac{1}{2}$	O : 1*i*			
Scheelite,		133° 38′	123° 59′			
Scheeletine,		132° 5′	122° 33′			
Wulfenite,			122° 26′			
Fergusonite.			124° 20′			

(7.) Sections I, II, IV, may form *one Group;* and Section V a *second* Group. Section III belongs most nearly with the first of these Groups. The species of either Group may not have the vertical axis approximately equal; but when widely unequal, the difference is in some simple ratio. As the fundamental pyramid is so *assumed* often for mathematical convenience, there may be in fact approximate equality in some cases, where there appears now to be a multiple ratio; or there may be a multiple ratio, where there is supposed to be equality.

B. Dimetric System.—(1.) The vertical axis of Section I is to that of Section II nearly as 1 : 1$\frac{1}{2}$; of Section II to that of Section IV, as 1 : 2; and as it is somewhat hypothetical whether, in the last two, $\frac{1}{2}$ or 1 be correctly the fundamental plane, the relation may in fact be that of equality. For a like reason, since 2*i* may be correctly plane 1, Sec-

tion III may also fall into the same category; so that Sections I, II, III, IV, may form a single homœomorphous Group.

(2.) Sections V and VI have the same relations to one another as III and IV, and may constitute a Second homœomorphous Group.

(3.) In this Second Group, the basal angle of the fundamental pyramid, 1, is near 109° 28′, and the pyramidal angles also; or in other words, the form approximates to the monometric octahedron, as has been often observed.

(4.) In the First Group, the basal angle of the fundamental pyramid, taking Section II as the type, is near 90°, or varies between 82° and 86°.

(5.) The *first* of the two Groups of Dimetric species, corresponds in molecular dimensions with Section V (or Group II) of the Hexagonal System, and the *second* most nearly with Section IV (or Group I) of the Hexagonal System. It remains to be shown how far this relation is an important one.

2. *On Descloizite, a new Mineral Species;* by M. A. Damour, (from the Ann. de Ch. et de Phys.)—The specimen here described was received at Paris from La Plata among different ores of lead. I received it from Mr. Sæmann, Naturalist at Paris, who has allowed me the pleasure of examining it. The following are its characters:

Crystals 1 to 2 millimeters thick, clustered in a group. Form a right rhombic octahedron, with the basal edges truncated; cleavage none. Mostly enveloped in a reddish clayey material, and implanted on a silicious and ferruginous gangue, and associated with green phosphate of lead, in acicular hexagonal prisms. Lustre quite bright. Color in general deep black; but some of the smallest crystals have an olive tint with chatoyant bronze-like lustre. Translucent on the edges, and the color by transmitted light, brown, inclining to red. On a surface of fracture, the color is zoned with straw yellow, reddish-brown and black; and nearly clear at middle; the brown and black colors are deepest at the extremities. Powder pale brown. Surfaces of the crystal, although shining, are generally striated, rugose, and present numerous little cavities. Scratches calcite, and is scratched by fluor: specific gravity at 15 C., 5·839.

Vertical prism M, 116° 25′; brachydome (top angle) 122° 6′; octahedron, 127° 10′, 88° 18′, 115° 10′, the last angle over M.

Heated in a tube, a little moisture is disengaged at an incipient red heat. B.B. on charcoal it melts, and is partially reduced to a globule of metallic lead enveloped in a black scoria. After cooling on charcoal, the mass is surrounded by a yellow areola. Fused with borax in the reducing flame, it gives a glass of a green color; and on adding a little nitre and heating in the oxydation flame, affords a violet tint, characteristic of oxyd of manganese. With salt of phosphorus affords in the reduction flame a glass of a fine chrome-green color, which becomes orange-yellow in the oxydation flame.

Dissolves in the cold in nitric acid diluted with 6 times its volume of water, and leaves a residue of brown oxyd of manganese, mixed with a variable quantity of siliceous sand from the gangue; the solution is colorless. On adding sulphuric acid to it, affords an immediate precipitate of sulphate of lead.

The following is the mean of two analyses:

		Oxygen.	
Vanadic acid, - - -	22·46	5·82	3
Oxyd of lead, - - -	54·70	3·92	2
Oxyd of zinc, - - -	2·04		
Oxyd of copper. - - -	0·90		
Peroxyd of iron, - - -	1·50		
Protoxyd of manganese, - -	5·32		
Water, - - - -	2·20		
Chlorine, - - - -	0·32		
Sesquioxyd of manganese, insol. in nit. acid,	6·00		
Siliceous sand, - - -	3·44		
	98·88		

Excluding the oxyds of manganese, iron, copper and zinc, which are coloring ingredients, the mineral is a vanadate of lead, of the formula $\dot{P}b^2\dddot{V}$. As this compound is a new one among minerals, the author proposes for it the name of Descloizite, in honor of M. Descloizeaux.

3. *Ancient Lake in the Colorado Desert;* (Commercial Advertiser, California.)—The desert lying on this side of the Colorado river was traversed in the latter part of November, by Lieut. J. G. Parke and party belonging to Lieut. Williamson's Surveying Expedition.

The route pursued was from San Bernardino through the pass of San Gorgonio to the eastern side of the mountains, thence along the northern extension of the Colorado Desert to the emigrant trail leading from the Gila river to San Diego. This is a portion of country never before explored.

Mr. Wm. P. Blake, the geologist of the expedition, made a geological reconnoissance of the country passed over, and has communicated to the *Commercial Advertiser* the following interesting statement of facts which came under his observation.

Our party started from San Bernardino on the 13th of November, and on the 16th encamped at the base of the eastern slope of the mountains, under the peak of Weaver Mt., or San Gorgonio. We had already encountered drift sand in ridges and low hills, and at our camp a high drift partly encircled a rude well dug by Indians in the clay. The well was about twenty-five feet deep, wide at the top, growing small to the bottom, where there was about a barrel full of muddy water; this was reached by means of rude stairs cut in the side of the well. The whole was partly overshadowed by several mezquite trees, growing about the opening. The clay surface, in which this well was dug, was hard and smooth, and the bare and rugged mountain rose abruptly from it, with the line of junction as angular and as sharply defined as it could be with a sheet of placid water.

In the morning we travelled over this floor-like surface, in which our wagon wheels scarcely left an impression, encountering an occasional sand drift which had polished the intervening bare surfaces of clay—when, on turning a sharp projecting ridge of the mountain, our attention was suddenly arrested by a distinct horizontal discoloration of the rocks, extending for a long distance on the flanks of the ridge. On approaching it I found that the difference of color was due to a cal-

careous incrustation over the whole surface of the rocks below the horizontal line. This crust was evidently coralline, and had been deposited under water; in its interstices were small spiral shells, similar to those inhabiting fresh water lakes. On examining the clay, this also was found to contain the shells in great number, and I soon collected six species. The water line at this point was not more than fifteen feet above the general surface. Beyond this point the shells became exceedingly numerous, and portions of the surface were thickly covered with shells like the ordinary "fresh water clam."

We were traveling in a long, gradually expanding valley, extending eastward towards the Colorado, and evidently the northern and western extension of what has been called the "Colorado Desert." The southern side of this valley, along under the bordering range on the south, is thickly inhabited in its western part by Indians calling themselves Cohuillas." We passed large villages of them during the day. They build near springs, and their huts are completely hid in groves of mezquite. The squaws were horridly tattooed as well as painted, and manifested much interest in the advent of our wagons and such a number of "pale faces" to their hitherto almost unvisited retreat. Many of the men had often been into the settlements, and had learned some Spanish and accumulated quite a stock of second-hand clothing. Our coming having been duly heralded by *smokes* and excited express riders from one village to the other, the whole population had collected to gaze upon us in wonder. Old hats and odd buttons seemed to have been in especial demand, and were worn with much dignity on the occasion of our arrival. We camped at their last village and spring for a few hours, and learned the existence of traditions among them concerning an ancient lake. They describe it as a wide sheet of water filling the whole valley; it abounded with geese and ducks; and their fathers lived in the mountains. The waters subsided "*poco poco*," (little by little,) and they moved their villages down to the valley it had left. Suddenly the great waters returned and overwhelmed them, drowning many of their people and driving the rest to the mountains. It is their belief that the waters will again come in.

At this part of the valley its width between the bounding ranges is probably fifteen miles, and it here widens towards the south. Our Indian guide refused to accompany us further, and "*mucho malo!*" was the only information that could be extracted from him regarding the section we were to go over before we could reach the emigrant trail. We kept under the southern mountains as much as possible, and the long horizontal line of an ancient beach was distinctly visible for twenty miles on the opposite side of the valley. On our side it was now high above our heads, following all the irregularities and sinuosities of the rugged granite ridges. Observing a few miles off a place that appeared accessible, I started with an assistant and barometer to ascertain the elevation of the line. Arriving under it, the rocks that had appeared diminutive in the distance were found to be huge blocks of granite, covered with the coralline growth nearly *two feet thick.* Large masses of this had fallen off by its own weight, and rolled to the foot of the cliff. By the barometer, the altitude of this water line is *nearly that of the sea level.* The lowest point of the valley is probably not less than five hundred feet below the water line.

The clay surface that has been mentioned, was deposited by the ancient lake. It is of great thickness, and now forms the substratum of the desert. We found numerous deep ravines extending across our course, deeply cut in the clay by floods from the mountains; they were generally narrow, but had *vertical* banks often twenty to thirty feet high. They could not be seen until we came within a short distance, and this often obliged the train to make long and tedious detours.

The "New River" of which you speak has been well known to the travellers of the desert since its sudden appearance a few years ago. The grass that springs up along its banks has hardly an opportunity to attain its full growth, as it is rapidly eaten off by the large droves of cattle and sheep constantly pouring into California from New Mexico and Sonora.

I have reason to believe that this is not the first instance of the overflow from the Colorado, and that it once flowed in a much larger and stronger stream than now, extending along by the well, known as "*Alamo mucho*," where cottonwoods were growing a few years ago. An Indian now living at Fort Yuma well remembers this stream. At the present time "New River" is not a running stream, except at certain high stages of water in the Colorado river; it then flows backward from the river *towards* the lower parts of the basin, and fills a series of local depressions which retain the water for a long time after the river has subsided. I crossed the dry bed of this stream between the pools known as the great and the little "lagoon," in December last; there was no water in it, and that in the lakes was green and charged with organic matter, but made excellent coffee.

It is probable that a local change of level produced by earthquakes has modified the direction of New River, and the quantity of water admitted to its channel.

Earthquakes are not uncommon in that vicinity. In Nov. 1852, two volcanoes burst out from the surface of the desert, and threw up a large column of steam and showers of mud; the country was well shaken for many miles around, and a portion of Chimney Peak, north of Fort Yuma, thrown down; great fissures in the clay of the old lake were also opened.

I did not find the eastern borders of the ancient lake; it is beyond the Colorado; its extent and boundaries cannot be precisely determined until the maps of the region are completed, but it is probable that its area will not be less than 7,500 square miles.

There were a series of outcrops of marine sedimentary formations rising above the general surface of the lake bed; these were filled with the most extraordinary concretions of all sizes and shapes. The ancient sea-drift abounds with silicified wood and marine fossils, all highly polished by the wearing action of sand. Numerous remarkable examples of its cutting action were seen on solid rocks and the hardest minerals. The whole region is one of peculiar interest, not only to the geologist, but it is highly attractive to the botanist and artist.

The facts that I have here given will be presented more in detail, with others, together with observations upon the agricultural capabilities of the desert, in my official report. I may observe in connection with this subject, that a similiar series of dynamical phenomena are present-

ed on a smaller scale in the valley of the Tulare Lakes, into which the waters of the San Joaquin are said to flow when the river is high.

The Gulf of California has undoubtedly extended far to the North of its present limits. Indeed the great ocean once washed the base of the Sierra Nevada and hid the interior of the country far from the light of day. In the slow, but persistent elevation of the continent since the tertiary era, the waters must have necessarily occupied more and more narrow limits, retiring from the elevated plain to the deep valleys until we find them imprisoned between the mountains of the Peninsula and the shores of Sonora. We cannot say that this elevatory movement has yet ceased; it is more probable that it still continues, although unappreciable during the comparatively momentary existence of man. The lake may have been originally cut off from the upper part of the gulf by submarine elevation; but, as the waters covered the whole surface, the sands could not have been acted on or drifted by the northwest winds to form the supposed barrier, as is suggested by your informant.

4. *Quicksilver Mine of Almaden, California;* by W. P. Blake, (from a letter to J. D. Dana, dated San Francisco, Feb. 14, 1854.)—I visited the Quicksilver mine at New Almaden a few days ago, and took a general view of it, and the establishment for the reduction of the ore. I shall soon visit it again and spend several weeks for the purpose of making a full examination of the geology of the vein.

The "Valley of San José" is properly the southern end of the great valley occupied by the Bay of San Francisco. The ground slopes so gently towards the bay that it is hardly perceptible, and appears like a perfectly level plain; the portion bordering the water is low and marshy, and many thousands of acres are left bare at low tides, the streams wander in crooked channels through the deep clay soil—reminding me of the great salt meadows along the Passaic and Quinnipiac rivers.

At the time of my visit the grass was springing up and the wild oat was covering the smooth round hills with a rich green carpet. The majestic oaks and sycamores that give a park-like character to the landscape were leafless, but the monotony of bare branches was relieved by the thick dark foliage of the evergreen oak—and although the middle of winter, the air was as soft and balmy as in our month of May on the Atlantic coast.

The works (or "Hacienda") where the Cinnabar is reduced are two or three miles from the broad valley, up one of the small side valleys of the mountains. The small village has the name of New Almaden, and it has few inhabitants that are not employed in or about the works or the mine.

The mine is about one mile from the works, and high up on the side of the mountain. The cinnabar is brought down on mules that carry about 200 lbs. each, and make two trips a day.

I expected to find the ore in close relation with the deposit of tertiary age, and among bituminous shales, but in this I was disappointed. All the tertiary beds appear to have been washed away, and hard serpentine and trappean rocks constitute a large part of the ridge in which the ore is found. There are however large outcrops of sedimentary rocks, composed of alternating beds of argillaceous shales and layers

of flint; they are highly tilted and much flexed. They are unlike any of the tertiary series that I have seen in the state, and I am inclined to refer them to the lower Silurian age.*

The ore appears to be mingled with the strata of this formation in a series of beds and interlaminations.

The mine and works are now under the management and general superintendence of Capt. H. W. Halleck, formerly of the U. S. Engineer Corps. An adit level has been run in for 900 feet, cutting the old workings about 200 feet below the former entrance to the mine; this adit is 10 feet by 10, and is well timbered. A railroad of 4 feet guage has been laid down throughout its length, and all the ore and refuse rock of the mine is brought out in cars that are weighed as they pass the office of the mine, and pass to the dressing floors, or to the attle heap. The irregularities and contorted windings of the excavations of the mine can scarcely be imagined; they extend in all directions. The transit between the upper and lower workings is effected by means of stairs or rude steps cut in the slopes, often replaced by notched poles instead of ladders. The ore has been followed and excavated wherever it could be found, and occasional pillars are left to support the roof.

The prevailing opinion among the miners and others appears to be that there is no regular vein of the ore—that it is a "pocket," a "deposit," &c.

The great width and number of the beds has confounded all ideas of extension or prolongation in any particular direction, and led to the general belief that the bed is "as broad as it is long," and the mode of working conforms to this view. The character of the deposit may not be that of a *true vein*, but that it has a determinate extension and direction I have not the slightest doubt, and I had abundant evidence of the fact. There is no reason why systematic mining should not be practised, both on the score of economy and safety.

The ore is divided into lenticular masses by intercalations of rock of variable thickness, and these are often filled with seams and veins of the sulphuret.

Numerous veins of carbonate of lime traverse the rock in various directions, cutting through the ore and faulting the small veins; it also lines cavities in the masses of cinnabar, and is then finely crystallized, and sometimes contains small quantities of bitumen in cavities, and implanted in globules among the crystals.

Sulphurets of iron and copper and arsenical pyrites are associates of the ore, but occur in very small quantity. Gold is said to have been found frequently in small quantities. I searched for crystals of cinnabar in vain, it is all massive, but presents various shades of color, and fresh fractures possess great brilliancy and beauty.

The mine is so free from water that the vein is scarcely decomposed at any point. If this had been the case, there would probably be some interesting mercurial salts formed along the walls.

The mining is principally performed by Mexicans and by Yaqui Indians. These are from the Gulf of California, and are of the same tribe that are employed in the pearl fishery. They make excellent

* Similar rocks (talcose and argillaceous shales and a Jasper rock) occur north of the harbor of San Francisco; they are more or less metamorphic, and probably *not older* than stated above by Mr. Blake.—J. D. D.

miners, and endure much hard labor. One or two Cornish men had recently commenced work.

The drilling is all done with long heavy drills, and the hammer is not used. I saw one bed of cinnabar about 8 feet thick, into which a powerful Yaqui Indian was drilling a hole; the crimson vermillion mixed with water was flowing from it in a stream and bespattering the surrounding surfaces. It is really delightful to see such quantities of beautiful ore as are displayed in this mine.

I was greatly pleased with the construction of the furnaces and arrangement of the works for reduction. They are an instructive example of the value of science united with practical skill. The superintendent has discarded all the old and complicated methods, and adopted a furnace in which the reduction is rapidly effected without the use of lime, or even crushing the ore. All the vaporized sulphur is converted into sulphurous acid by the admission of atmospheric air, thus separating it from the mercurial vapor, and preventing a recombination in the condensing chambers. Some improvements can yet be made, especially in the apparatus for condensation, as a small quantity of mercury escapes from the chimneys in vapor and deposits the peculiar gray coating on their tops.

I have before me a specimen of cinnabar from a new vein recently discovered in Sonora. It has been found in small quantities only, and is chiefly interesting from the occurrence of metallic mercury in the parts of the vein near the surface. They occupy small cavities in the rock, and are apparently derived from the decomposition of the sulphuret.

5. *Conistonite.*—In a recent letter from R. P. Greg, Jr., (dated Manchester, Eng., March 17,) we have received a drawing of another crystal of Conistonite, which has on the angles, the planes of a brachydome, in addition to those of the figure on p. 133. He gives also the following revised angles, M : M$=$97° 5′, M : $e=$122° 55′, M : c (brachydome) $=$117° 30′, $c:e=$121° 0, $c:c$ (top) $=$96° 50′, $c:c$ (base) $=$83° 10′, $e:e$ (base) $=$93° 30′, $e:e$ (top) $=$86° 30′. Faces not very bright, c the brightest. No cleavage observed. When heated it gives off a peculiar pungent argillaceous odor, and something like the smell of burning paper.

III. Botany and Zoology.

1. *On the Age of the large Tree recently felled in California;* by A. Gray. (Read before the American Academy of Arts and Sciences.)—The age attained by the largest known trees is a matter of considerable interest; but it is seldom that an opportunity occurs of testing it by an actual counting of the annual layers of the trunk. This is said to have been done in the case of the gigantic tree recently felled near the head of the Stanislaus River, on the Sierra Nevada, California, a section of the trunk of which, at 25 feet from the ground, and hollowed out to a shell, is now on exhibition at Philadelphia. The trunk of this tree "was sound from the sapwood to the centre;" and its annual layers are very distinct to the naked eye in pieces of the wood in my possession. The size of this tree is such as to give it a presumptive claim to rank among the oldest of the present inhabitants of the earth; its length being 322 feet; the diameter of the trunk at 5 feet from the ground,

29 feet 2 inches; at 18 feet, 14 feet 6 inches; at 200 feet, 5 feet 5 inches, including the bark. These measurements are copied from Mr. Lobb's account of the tree published in England, except the height (by Mr. Lobb said to be about 300 feet), which I have given on the authority of the proprietor of the section now at Philadelphia. This section was taken at the height of 25 feet from the ground, and, according to the measurement of my friend, Thomas P. James, Esq., of Philadelphia, it is about 12½ feet in diameter, including the bark. Mr. James, at my request, has taken careful measurements of the wood itself, excluding the bark. The three diameters taken by him, respectively measure 9 feet 6 inches, 10 feet 4 inches, and 10 feet 10½ inches; the average diameter of the trunk at the height of 25 feet from the ground, is a little over 10 feet 3 inches. From the statements which have appeared, it would seem as if the layers had actually been counted, and ascertained to be 3000 in number. This surely ought to have been done; but an examination of the statements does not prove that it was. Mr. Lobb's statement, as definite and reliable as any, is, that "the trunk of the tree in question was perfectly solid, from the sapwood to the centre: and *judging* from the number of concentric rings, its age has been *estimated* at 3000 years."

The number of layers, therefore, has only been *estimated;* and we are not in possession of the exact data on which the estimate was founded. The data wanting are the average thickness of the layers towards the centre, giving the rate of the tree's growth as a young and middle-aged tree, when it must undoubtedly, like other trees, have increased more rapidly than in later years.

Dr. Lindley, I find, (in the Gardener's Chronicle), has accredited the estimate which assigns to this tree an age of above 3000 years; stating that "it may very well be true if it does not grow above 2 inches in diameter in 20 years, which I believe to be the fact." That rate would indeed give 3500 layers at the height of 5 feet from the ground, where it is 29 feet 2 inches in diameter. But this measurement appears to include the bark, to allow for which Dr. Lindley would perhaps give up the odd 500 years. There is a further consideration. At 25 feet from the ground the diameter of the wood is less than 10 feet 4 inches. Here the rate of two inches in diameter in 20 years would give the trunk an age of only 1230 years, so that on these data, the tree in its youth would have been 1770 years in adding 20 feet to its stature! It is evident that the trunk is much enlarged at the base, as in many old trees, especially of the same tribe, such as the *Taxodium*, &c.

The section of the trunk at Philadelphia has been hollowed out, by fire and other means, to a shell of 3 or 4½ inches in thickness. Of this I have, through the kindness of the proprietor and of Mr. James, a piece of the wood, including nearly 3 inches of this section. What is now wanted, and what unfortunately I do not possess, is a foot or two of the wood from the central parts of the tree—a desideratum which may doubtless be supplied hereafter. The data at hand, however, will suffice for determining an age which the tree cannot exceed, unless it be supposed to have grown more slowly during the earlier $\frac{8}{10}$ of its existence than during its later years, which is directly contrary to the ascertained fact in respect to trees in general. Now the piece of wood in my hands exhibits an average of 48 layers in an inch. The semi-diameter

of the trunk at the place whence it was taken is about 5 feet 2 inches. If the tree increased in diameter at the same rate throughout, there would have been 2976 annual layers; which, allowing 24 years for the tree to have attained the height of 25 feet, would give it an age of 3000 years from the seed. This corresponds so closely with Dr. Lindley's estimate that we may suppose him to have employed equivalent data in a similar manner. How great a deduction must we make from this estimate, in consideration of the greater thickness of the layers as a younger tree? The only direct data I possess bearing on this point are derived from a piece of a transverse section 3½ inches deep, of a "rail," which the exhibitor says was taken from the trunk at the height of 275 feet from the ground. As its layers, on a breadth of nearly ⅞ths of an inch, show only a slight curvature, it must have come from a part of the trunk still of several feet in diameter. On this section the exterior inch, nearly all alburnum, contains 90 layers; the next 60; the next 45, the remaining half inch 16, making 32 to the inch. That the exterior layers should be thinner at this height than those near the base of the tree, is just what would be expected. If we apply this ratio of decrease of the number of layers to the inch as we proceed inwards, to the section at 25 feet from the ground, we should, at four inches within that part of the circumference which I have examined, have only 17 layers to the inch, which taken as the average thickness, would make the tree only $1034+24=1058$ years old. But it is not probable that the thickness of the layers increases so rapidly. The data we possess on other trees goes to show that a tree after it is 400 or 500 years old, increases in diameter at a pretty uniform rate for each 20 additional years, on the whole, although the difference in the thickness of any two or more contiguous layers, or of the same layer in different parts of the circumference, is often very great. Still, when we consider how very much thicker are the annual layers of a vigorous young tree than of an old one, perhaps we should not be warranted in assuming more than the average of 17 layers to the inch for the whole section.

Some useful data may be obtained from a tree more nearly related than any other to those of California, though of a different genus, namely the so-called Cypress of our southern States (*Taxodium distichum*). I possess three sections of different trees of Taxodium, reaching from the centre to the circumference. One of these, on an average radius of 27 inches, exhibits 670 layers; a second, on a radius of 30 inches, has 525; a third, on a radius of 22 inches, has 534 layers. The average is 576 layers to a semi-diameter of 26 inches, or about 22 layers to an inch. Half of this growth (13 inches radius) was attained at the close of the first century; while the exterior layers of the oldest specimen were only the fiftieth or sixtieth of an inch in thickness. We have reason to believe, therefore, that the *Wellingtonia* (as it is called) of California is at least as rapid in its growth as the *Taxodium*. We may safely infer, I think, in the absence of other data, that when the tree in question had attained the size of 26 inches in semi-diameter, it was only 576 years old. If therefore we suppose it to have increased at the intermediate ratio of 35 layers per inch for the next 26 inches, and at the actual rate of the last century (as ascertained by inspection), namely at 48 layers per inch, for the remaining 10 inches, we should assign to it the age of 2066 years as its highest

probable age. I think it more likely to be shown, when the wanting data are supplied, that the tree does not antedate the Christian era. There are said to be 80 or 90 such trees, of from 10 to 20 feet in diameter, growing within the circuit of a mile from the one felled. When the next of these venerable trees are wantonly destroyed, it is to be hoped that its layers will be accurately counted on the whole section, and the thickness of each century's growth carefully measured on the radius.

The tree in question is a near relative of the *Redwood* of California, namely the *Taxodium sempervirens* of Don, of late very properly distinguished as a separate genus under the unmeaning and not euphonious name of *Sequoia*, a tree now grown in England, and sparingly also in our own vicinity, where it is barely hardy. My friend, Dr. Torrey, has for nearly a year possessed specimens of foliage of this tree, which he took to be a new species of *Sequoia*. The fruit and branches of the juniper-like foliage (probably only one form of a dimorphous foliage, which is common in *Cypressineæ*) having been received in England from Mr. Lobb, by Dr. Lindley and Sir. Wm. Hooker, they have recognized in this tree the type of a new genus, distinct from *Sequoia*, to which the former has given the name of *Wellingtonia*. The wood is, I believe, much the same as that of *Sequoia sempervirens*, which tree also attains a gigantic size. The principal characters yet ascertained are that the cones of *Wellingtonia* are oblong and have a thick woody axis. Additional materials are needed to confirm the genus, if such it be.

2. *Synopsis Plantarum Glumacearum, autore* E. G. Steudel, fasc. 1. Gramineæ, p. 1–80. Stuttgart, 1854. Imp. 8vo.—The characters, in this new revision of Grasses, are about as full as those of Kunth's *Enumeratio Plantarum:* the references less full. The large page is compact and in double columns, so that eleven such fasciculi may, as proposed, comprise the known Glumaceous plants. This fasciculus takes in the *Oryzeæ*, the *Phalarideæ* (in which Zea is still included!) and the greater part of the *Paniceæ*,—among them 568 species of *Panicum!* without reaching the end of that genus. A proper study would reduce the number of species fully one-half. A. G.

3. *Lindley's Folia Orchidacea:* part 5, was published in February last. It comprises 8 genera, of which the principal as to number of species are *Brassia*, *Sobralia*, and *Cœlogyne*. A. G.

4. *Epistolæ Caroli a Linné ad Bernardum de Jussieu Ineditæ, et mutuæ Bernardi ad Linnæum;* curante Adriano de Jussieu, (Ex Act. Acad. Art. et Scient. Amer. ser. nov. tom. v.) 1854 pp. 54, 4to.—It is somewhat singular that the correspondence of these two founders of modern botany should have remained so long unpublished (excepting translations into English of several of the letters published by Sir J. E. Smith); and it is not a little remarkable that it should at length be given to the world in this country, as a contribution by the lamented editor to the publications of an American scientific society of which he was a distinguished foreign member. The notes furnished by Adrien de Jussieu possess the sad interest of having been the last scientific occupation of the last of the Jussieu's. The publication makes a part of the forthcoming half-volume of the Memoirs of the American Academy of Arts and Sciences. The greater part of the extra copies printed

have been forwarded to the family of the editor for distribution among his scientific friends. A. G.

5. *Localities and Habits of certain species of Insects, &c.*; by J. P. Kirtland.—In vol. xiii, page 336 of the Amer. Jour. of Science, were described the Libythea Bachmanii and the Macroglossa balteata, from solitary specimens, captured many years since at Poland, O., little was known respecting them. Additional facts in regard to their localities and habits have been recently obtained.

In June last, while visiting the late Wm. Jenison,* of Dayton, I found several specimens of the first named insect in his collection. He informed me that they visited his gardens every year, in rather limited numbers, but he had never discovered them in their larva state.

Subsequently Dr. Hoy, of Racine, Wis., furnished me with many fine specimens. From him I learn that during a brief period of about two weeks, they resort in great numbers to the flowers of the garden raspberry, (Rubus Idæus), and are seen at no other time. During their sojourning they doubtless deposit their eggs on some species of vegetation, which affords appropriate food for their larva. Whether their resort to the flowers of the raspberry is for that purpose, or to obtain food for themselves, has not been ascertained. It is evident that one brood only can be produced in a summer.

The Macroglossa I found in Dr. Hoy's cabinet, as also in Gov. Farwell's collections at Madison, Wis., and learned that it is occasionally seen in different parts of that state. In its habits it is said to resemble the *Sesia pelasgus.*

From what I have ascertained, I conclude that the Libythea is sometimes found as far east as Boston, Mass., but that both it and the Macroglossa belong more appropriately to sections of the country west of Ohio.

The crustaceous animal, closely allied to the *Palemon vulgaris* of Say, taken by Prof. Baird at Port Sarnia, in Canada, I have recently captured in Rocky River, seven miles west of Cleveland. This animal Prof. Dana has, I believe, decided to be specifically new, and related in genus to Anchistia of the Palæmonidæ.

IV. Astronomy.

1. *Comet* V, 1853, (Astron. Jour., No. 67.)—This comet, discovered by Mr. Van Arsdale, on the 25th of November last, was observed at Göttingen by Klinkerfues, on December 2. Professor S. Alexander, of Princeton, N. J., has obtained the following elements of its orbit from Van Arsdale's observations of Nov. 30, Dec. 27, and Jan. 21.

T. 1854, Jan. $5^{d}\cdot77967$, M. T. Greenwich.

Long. of perihelion,	55° 15′ 7″	Mn. Eqnx Jan. 1.
" asc. node,	227 5 27	
Inclination,	66 11 53	
Log. of perihelion dist.	0·3097879	
Motion retrograde.		

* Mr. Jenison died in July last. His was a life of vicissitudes. He was at one period a student of Comte Dejean. In the retired situation in which he spent a number of the last years of his life, near Dayton, O., amusing himself with horticulture, and almost every branch of natural science, his amiable disposition, talents and attainments, were neither known nor appreciated. He was an accurate and intelligent entomologist.

2. *Obituary.*—Professor A. C. Petersen, (Astron. Jour., No. 68.)—This eminent astronomer has recently closed his earthly labors. For many years he had been connected with the Observatory at Altona. His numerous contributions to the *Astronomische Nachrichten* continued through twenty-five years, attest the extent and value of his labors. On the death of Schumacher, in 1850, Dr. Petersen succeeded him in the direction of the Observatory, and in the editorship of the *Astr. Nach.* "The mantle of the illustrious Schumacher seemed to have fallen upon Petersen. To him, in conjunction with Professor Hansen, was bequeathed the direction of the priceless *Astronomische Nachrichten* to which a large portion of many years of his life had already been devoted. To him was confided also the direction of the Observatory of Altona. And he possessed in a signal degree the noble qualities of heart and soul which have endeared the memory of Schumacher to all who knew him. Warm hearted, unselfish, true, devoted to science, to duty, and to his friends, he has left a void which it will not be easy to fill."

V. Miscellaneous Intelligence.

1. *Abstract of Meteorological Observations made at Burlington, Vt., in* 1853; by Z. Thompson.—Location, Lat. 44° 29′, Long. 73° 11, one mile east of Lake Champlain and 256 feet above it, or 246 feet above tide.

1853.	Thermometer.				Barometer.			
Months.	Mean.	Highest.	Lowest.	Range.	Mean.	Highest.	Lowest.	Range.
	°	°	°	°				
January, . .	19·63	45	−11	56	29·76	30·45	28·54	1·91
February, .	21·47	47	−10	57	29·68	30·13	28·86	1·27
March, . .	32·77	54	1	53	29·57	30·28	28·84	1·44
April, . . .	42·58	77.	22	55	29·63	30·00	28·70	1·30
May, . . .	55·19	87	27	60	29·65	30·08	29·30	·78
June, . . .	67·27	94	43	51	29·72	30·02	29·40	·62
July, . . .	68·70	90	50	40	29·71	29·94	29·39	·55
August, . .	68·15	95	44	51	29·65	29·90	29·28	·62
September, .	59·48	93	30	63	29·70	29·97	29·10	·87
October, . .	47·02	70	23	47	29·67	30·14	29·09	1·05
November, .	37·27	62	9	53	29·93	30·50	29·09	1·41
December, .	22·94	42	− 4	46	29·67	30·01	28·74	1·27
Annual mean,	45·21	95	−11	106	29·69	30·50	28·54	1·96

1853.	Winds.								Weather.		Snow.	Water.
Months.	N.	N. E.	E.	S. E.	S.	S. W.	W.	N. W.	Fair.	Cloudy.	Inches.	Inches.
January, .	12	3	0	1	10	1	1	3	15	16	14	1·22
February, .	10	1	1	4	9	0	0	3	13	15	29	3·94
March, . .	4	1	1	2	8	1	5	9	21	10	14	1·70
April, . .	10	2	1	0	11	1	2	3	21	9	14	2·25
May, . .	9	1	1	1	15	1	1	2	18	13	0	3·95
June, . .	5	0	1	0	16	1	4	3	25	5	0	1·74
July, . .	5	1	1	1	13	5	2	3	24	7	0	3·12
August, .	8	1	0	1	15	3	1	2	26	5	0	3·46
September,	11	1	1	1	10	2	1	3	21	9	0	5·67
October, .	2	1	1	2	12	1	6	6	20	11	4	3·04
November,	8	1	0	2	11	1	2	5	15	15	7	2·17
December,	10	1	0	2	14	1	1	2	17	14	8	0·79
	94	14	8	17	144	18	26	44	236	129	90	33·05

The results given in the above tables are derived from three daily observations, made at sunrise, 1 P. M., and 9 P. M.

The mean temperature of 1853 was one-half of a degree warmer than the average of the preceding 15 years. The warmest day was the 12th of August, the mean heat of which was 83¼°; and, on the same day, the thermometer was highest in the shade, standing at 95° at 3 P. M. The coldest day was the 26th of January, which averaged −3°, or 3° below zero; and the greatest cold was −11°, on the morning of January 27th.

The barometer was highest on the 27th of November, when it stood at 30·50 inches, and lowest on the 24th of January, standing at 28·54 inches, showing a range for the year of 1·96 inch.

The range of the thermometer was 8° less, and that of the barometer 0·27 inches greater than in 1853.

The fall of water in rain and snow, was just 1 inch more than the mean annual fall in the preceding fifteen years, (which was 32·05 inches) and 4·23 inches more than in 1852. The fall of water in December, 1853, was less than in any December in the preceding 15 years, and less than half the average for December, that being about 2·5 inches.

The fall of snow was 13 inches less than in 1852, and 19 inches more than in 1851. During the year there were 54 days of tolerable sleighing,—the same amount as in 1852. The broad part of Lake Champlain, opposite Burlington, was closed with ice about the 29th of January, for two days, but opened and did not close again till the 20th of February. From that time it remained closed, for the most part, till the 10th of April; but the ice was, all the while, considered unsafe, and teams ventured to cross only for a short time about the middle of March. Steamboats appeared on the lake the 12th of April. The lake was highest about the 29th of May, and was then 18 inches below the extreme high-water mark of May, 1847. It was lowest on the 8th of September, being then 6 ft. 8 in. below high-water mark, showing a change of level in 1853, amounting to 5 ft. 2 in.

The following indications of the advance of spring were noted:

March 22d, Robins and Long Sparrows seen; 25th, Blue Birds, *S. Wilsonii;* 26th, Cherry Birds, *B. Carolinensis.*

April 18th, White-bellied Swallow, *H. bicolor;* 24th, Liverwort in blossom, *H. triloba;* 26th, Toads and Frogs heard; 29th, Red Maple, *A. rubrum;* White Elm, *U. Americana;* Barn Swallows, *H. rufa.*

May 9th, Tree Toads, *H. versicolor;* 13th, Bobolink, *I. agripennis;* 17th, Plums in blossom; 21st, May Bugs, *M. quercina;* 22d, Pears in blossom; 26th, Crab Apple, Siberian; 27th, Common Apple.

The appearances of the Aurora Borealis at Burlington in 1853, noted in my journal, were as follows:

January 4th, A bright glow in the N., but no distinct arch; 6th, ibid; 8th, a distinct arch in N. E. at 7 P. M.

February 8th, Glow of light in N. W.; 14th, a well formed arch, 12° high at 9 P. M.

March 7th, Faint Aurora at 8 P. M.; 8th, very splendid from 8 to 10 P. M.; 10th, ibid, with a well defined flat arch, 15° high at 10 P. M.; 30th, faint.

April 1st, Aurora Borealis in N. E., arch 10° high at 9 P. M; 5th, Aurora very splendid, commenced as the clouds dispersed a little before 9 P. M., and increased in brightness till 10. At 9½ it consisted of an exceedingly bright glow in the north, rising nearly to the pole-star. At 10 it rose considerably above the pole, with numerous bright pillars of light lower down; 7th, a very fine Aurora, with the principal arch nearly stationary from 8½ to 10 P. M., and its vertex about 10° high; 10th, faint.

May 1st, Faint Aurora low in the N.; 2d, meteor very splendid, at 8 faint, at 8½ a regular arch from E. to N. W., 6° above the pole; at $8^h\,55^m$ a corona formed 8° S. W. of the zenith, with the radiations very distinct and beautiful. The meteor was very changeable, and the streamers at times exhibited a dazzling brightness. 4th, faint Aurora, with no distinct arch; 6th, ibid; 7th, ibid; 31st, ibid.

June 2d, Aurora Borealis faint; 8th, ibid; 9th, ibid.

July 4th, Faint Aurora; 10th, ibid; 12th, fine Aurora, with streamers flitting rapidly at 9½; 23d, faint Aurora in N. E.

August 31st, Slight Auroral light.

September 1st, Fine Aurora, commenced at 9 P. M. and continued through the night; 2d, very fine Aurora which continued through the night,—at 9 P. M. there was a flat arch under the pole, 15° high; 3d, another fine Aurora, with a distinct arch under the pole, 25° high at 9 P. M.; 10th, faint Aurora; 24th, ibid.

October 31st, Slight Aurora low in N.

November 1st, Ibid.

December 4th, Beautiful Aurora, bright arch low in the N.; 8th, faint Auroral light in N.

2. *Extract of a letter from Colonel J. C. Fremont, respecting his Explorations for the route of a Railroad to the Pacific.*

[The letter from which extracts are here given was prepared by Col. Fremont for publication shortly before he set out on his journey, but for particular reasons it was not printed at the time. Those reasons no longer exist, and it is now proper that the letter and extracts from the correspondence of Col. F. should be given to the public. The name of the person to whom the principal letter is addressed need not be given.]

"My own journeys through our interior mountains had already in 1847 satisfied me that a direct railroad route ought to be searched for along the parallel of 38° 39′. Information acquired from all sources led me to believe that this range of country was certainly practicable, and that the most important points on either ocean might be connected, by a line which should be direct, and at the same time penetrate the mountains through a region admirably adapted for settlement and cultivation. With this view I had embraced in my original plan of explorations, (and so stated in one of my published journals,) an examination of the central section of the Rocky Mountains; comprehending the Three Parks, with the numerous valleys which enclose the head waters of the South Platte, the Arkansas, and the Del Norte on the one side, and the sources of the East Fork of the Great Colorado on the other.

Since 1847 my attention has been continuously occupied with this line, and with this section of the Rocky Mountains through which I have proposed that the great road should pass, entering the valley of the Colorado through the valley of the San Luis or Upper Del Norte. To this point my own examinations have been extended, with the satisfactory result of finding a way easy and good, through a broad and fertile country, allowing straight roads and choice of lines with continuous and expanded settlements. Such a line seems to comprehend all the advantages which ought to be combined in a trunk road to the Pacific. It is direct, central, and feasible of construction; it commands the most practicable pass, and best known route for a branch road into Oregon, and would be the means of forming a new and great state of this Union—the Switzerland of America.

With these views I attempted, but was prevented from exploring it in 1847. In 1848 I resumed the attempt in an expedition which was defeated in its complete result; but was successful so far as it went, and completely successful so far as it realized my own idea.

About this time finding myself owner of a property which promised to be of extraordinary value, I conceived the idea of wholly devoting it, as far as it would go, to the prosecution of the work; and on my return to Washington in 1850, I communicated it to Mr. Francis P. Blair and other friends, who highly approved of my plan, and through whom, without design on my part, it reached the public press.

In the fulfillment of this plan I visited England with the expectation of engaging foreign capital in the development of the property, so as to make it available to the enterprise. Protracted delays and difficulties growing out of the contest for my property with the government, blunted or paralyzed my efforts, and I had succeeded only to a limited extent, when information of the general movement at home in favor of the railroad reached me. I immediately arranged my affairs for an absence of six or eight months, provided the necessary instruments, and started for Washington. On my arrival I found the [U. States] exploring parties were fully organized, and the Government commands already disposed of. Finding myself prepared with time and instruments, and fairly engaged in the enterprise, which properly formed a continuation of my own plans, I have decided to carry it out by my private means. The property above referred to had diminished in value from the litigations of the United States; much of its absolute and immediately available wealth had been carried away, but it had become better defined, and I had recently received in England an offer of two millions [of dollars?] for half, dependent upon recognition of title. This realization of the property has been again subjected to indefinite delay by the recent appeal of the Government from their own tribunal, but I have decided to invest it, whenever it becomes available, in that part of the road which shall go through the state of California from San Francisco to its western frontier, at whatever point the road from the East should strike it.

Under this deprivation of resources, I can only go on with one branch of my intended enterprise, that of completing my examinations of the country between the Upper Del Norte and the valley of the San Joaquin. Upon this line I propose to make a double examination—

going out before the snows fall, and returning in mid-winter. A winter exploration, in making me acquainted with the depth and prevalence of the snows, and the extent of their impediments to winter traveling, would enable me to judge practically of some points very material to the right decision of the question.

The field of operations reaches over an expanse of mountain wilderness extending the whole length of our domain from north to south. In view of the enduring and unchangeable character of the work, no line can be considered approximately determined until all possible information should have been obtained, so that the best route, under every aspect for the country, may be adopted. To meet the actions of the next Congress, will give ample employment to all the labor that can be brought to it, and my examinations therefore must necessarily contribute to the mass of materials for the solution of the question. Whatever may be the results at which I arrive, they shall be fairly communicated to the public, as an element in aid of their decisions.

Finally, and above every other consideration, I have a natural desire to do something in the finishing up of a great work in which I had been so long engaged. I do it with the object and the hope of adding to the favorable considerations which (I may be permitted to say) have recognized the disposition I had already shown to serve the country. A deference to this favorable opinion, which I should regret by any act to impair, makes the occasion for the present letter. I felt that some explanation was due to the public for taking part as a private individual in this public concern, and was unwilling to leave the motives of the present journey exposed to misconstructions. I judged therefore that a clear statement of them would not be considered improper or uncalled for."

Soon after Col. Fremont started on his journey, illness forced him to return to St. Louis for medical aid. The exposures of the winter of 1848 had fastened upon him a painful affection of the left leg which had become "neuralgic sciatica." He had been free from it for a year or more, but the great change of living seemed to develop it again. After a delay of just a month, he was able to make a second start, leaving Westport the 21st of October. During his illness, his party, which consists of some twenty-two men (half being Delaware Indians,) waited for him about a hundred and fifty miles from the frontier. On rejoining them, Col. Fremont wrote that the animals were all in bad traveling order, as the grass was gone, and they were not near to any fort or trading post where corn could be procured in its place. From this cause he anticipated much delay, as his last letters prove was the case. His own health however was so entirely restored, that, with the success of his daguerreotypist, and some other causes of satisfaction, he writes in confident good spirits of his ultimate success. The next steamer from California may confirm all his hopes; for it is only reasonable to suppose his calculations may prove just, and that he will have reached San Francisco in February—about a month since.

Extracts from letters from Bent's New Fort, Nov. 24, 1853.

"The expedition so far, has been successful enough to gratify all our expectations, both in its general results, and in its particular object.

Our way forward now will in a few days become a struggle with the winter, forcing our way on and preserving the lives of men and animals, while at the same time we carry on the work uninterruptedly. It will amount to a wintering in the mountains, but will be a progressive one, and will fulfill all the requisitions of a winter there, as the two months of December and January exhaust the strength of the winter. * * * * Without some bad mischance, you may expect when you next hear from me, to learn of good results."

"Our condition as regards the efficiency of the camp in animals, is very bad, as regards the work we have done and are doing, very good. Provided we can reach the San Joaquin valley in the same position, our main object will be accomplished. Before reaching this place, we had lost thirteen animals. I arrived here with difficulty, traveling 10 or 12 miles a day, and having nearly all my party on foot. In the early part of the month we had encountered severe weather, the daylight thermometer being at 15° and 17°, and the buffalo had so eaten off the grass, indifferent as it was, that our animals were nearly starved. I am determined to carry this enterprise through. We will fight with the winter, and every other obstacle, to the end, prudently and cautiously, but never giving way. In the mean time, we shall do a valuable work. The astronomical, barometrical, and topographical work all goes on well. After surmounting some difficulties which required much skill to remove, the daguerreotypist has been eminently successful, and we are producing a series of pictures of exquisite beauty which will admirably illustrate the country. Every successive picture improves upon its predecessor, and those of yesterday were jewels. They were of the Cheyenne village here among the timber. As we go on, and the mountains rise before us, the views will become more interesting.

The coal formation re-appears in this neighborhood, and I have become acquainted with a locality where the coal-beds are said to be developed largely. The coal has been tried and found to burn perfectly well. It is near the line of the Road.

It will probably be two months yet before I can reach San Francisco, as I must move slowly through the mountains. Notwithstanding the early cold, we may yet have a mild and open winter. For the last 10 days the weather has been beautiful; yesterday and to-day even quite warm at mid-day—not a particle of snow to be seen, and this, as Dr. Ebers remarked, when all Germany is covered with three feet of snow."

3. *On the Action of Alkalies on Rocks;* by M. Delesse, (communicated for this Journal by the author.)*—In order to study the action of alkalies on rocks, I have taken the powder of the rock under trial, and have heated it with a solution containing a quintuple weight of potash; I have then determined what substances were contained in the potash solution.

As a large number of rocks contain water, after I have dried the residue from the action of the potassa and determined its weight, I bring it to a red heat and take anew its weight; in this way I have obtained for different rocks the following comparative results:

* This paper should have had a place among the articles of this number.

	I. Trachyte	II. Trachyte	III. Pertinite.	IV. Retinite.	V. Retinite.	VI. Perlite	VII. Obsidian	VIII. Eurite.	IX. Palagonite	X. Melaphyre	XI. Basalt	XII Black lava.	XIII. Porphyry
Silica dissolved by potash,	36·00	17·06	19·40	[illegible]·23	9·50	19·5.	18·39	11·45	7·05	8·50	7·6[illegible]	4·[illegible]0	5·35
Alumina " "	*tr.*	2·39	3·75	1·16	1·25	1·8.	3·78	1·55	2·10	2·20	2·8[illegible]	2·60	*tr.*
Total loss of rock after calcination of the residue.	37·85	27·27	30·15	17·89	16·55	26·85	24·44	17·20	18·60	18·41	15·3[illegible]	8·50	5·80

I. Brownish red Trachyte with gray globules and blackish mica, from Hungary.
II. Trachyte "molaire," with a little orthoclase and mica, from Hungary.
III. Blackish brown Pertinite, from Planitz.
IV. Globulous Retinite of a maroon-red color, from Meissen.
V. Retinite without lustre, and very resinous, from Sardinia.
VI. Grayish white and black Perlite, from Cape de Gata.
VII. Deep black Obsidian, from Lipari.
VIII. Argillaceous Eurite, of a brick-red color, from Saxony.
IX. Yellowish brown Palagonite, with a resinoid paste, from Iceland.
X. Melaphyre with a dark green paste containing greenish-white Labradorite, from Belfahy.
XI. Basalt with a blackish paste and crystals of Augite, from Bohemia.
XII. Black Vesuvian Lava, from the stream of 1819.
XIII. Quartziferous porphyry with a cellular paste of a reddish-gray color and much glassy quartz, from Saxony.

The following general conclusions may be deduced:

When a rock is attacked by an alkali, some alumina as well as silica is removed, with also some water, potash and soda; besides also a little lime, magnesia, and traces of oxyd of iron. The amount of silica removed exceeds that of all the other substances.

The alumina and water follow next after the silica in amount removed.

Granite is not thus attacked when boiled with an alkaline solution; Quartziferous Porphyry is feebly attacked, losing only some hundredths. Lava, Basalt, and Melaphyre, lose less than 20 per cent.

Trachyte, Retinite, Perlite and Obsidian, suffer the largest loss, but not exceeding 40 per cent.

A rock containing water is attacked much less readily by alkalies after it has been calcined; for the Perlite of Cape de Gata, for example, the loss before and after calcination, is nearly as $2\frac{1}{2}$ to 1.

A rock is much more easily attacked when partially decomposed. Argillaceous Eurites or Kaolins, which are only decomposed granitic rocks, experience a much greater loss than the granitic rocks.

Other things equal, the action of the alkalies is greater the larger the amount of silica, or the less crystalline their structure, and the less of hyaline quartz they contain.

The vitreous rocks, which contain little or no quartz, like Retinite, Perlite, Obsidian, Trachyte, are strongly attacked by alkalies.

On substituting alkaline carbonates for the alkalies, certain rocks, and especially the vitreous, are still attacked, but to a less extent.

The facility with which the alkalies and even the alkaline carbonates attack rocks, shows that it is difficult to use them for separating the free or immediately soluble silica which may exist in a rock, especially in clays and kaolins.

I observe, also, that in obsidian, for example, the silica which is dissolved is not *free* silica, but it is in the condition of a silicate attackable by the alkali; so also with Retinite, the silica is not in the state of opal, as generally regarded, but in that of an attackable hydrosilicate. In a

word, in all vitreous or porphyritic rocks, hydrated or not, the silica is in combination, forming a compound, not definite, which the alkali attacks, and which is the paste of the rocks.

The waters of infiltration which penetrate rocks contain always small proportions of alkaline salts, even near the earth's surface; and hence it is obvious that these salts should contribute towards the decomposition of the rocks and the formation of pseudomorphs. But at a small depth below, the waters are more largely charged with alkaline salts, and both temperature and pressure increase rapidly; they may attack, therefore, quite strongly, the rocks with which they come in contact; as happens notably with mineral waters, geysers, and other results of volcanic action. Consequently the action of alkalies or of alkaline salts on rocks plays an important part, not only in the formation of pseudomorphs, but also in the chemical reactions which take place in the interior of our planet.

4. *On the Prosopite of Scheerer;* by JAMES D. DANA.—Prosopite is described as a new mineral by Scheerer in Poggendorff's Annalen, No. 10, 1853, p. 315. The mineral occurs at the Tin mines of Altenberg in crystals altered mostly to a kaolin; and from some qualitative trials of unaltered portions, it is supposed to consist of aluminium, calcium, fluorine and water. It had been regarded as a pseudomorph after Heavy Spar: Scheerer recognizes its relations to that species in the angles, and also marks differences; Heavy Spar has the three angles 77° 43′, 128° 36′, 105° 24′, and Prosopite correspondingly 77° 30′, 132°, 116½°. Unlike Heavy Spar, the crystals are hemihedral. The faces of the crystals are dull, and admit of measurement only with the common goniometer. Scheerer suggests that the formula may be $CaF + \dot{A}lF^3$, analagous to that of Heavy Spar which is $CaO + SO^3$.

This comparison with Heavy Spar does not exhibit its true affinities. In fact the angles are *almost identical with those of Datholite*, with which it also agrees in its hemihedral character. The following angles show this resemblance. The planes of Datholite here referred to will be learned from the figures on page 215 of this volume.

Datholite.	*Prosopite.*
$i\breve{2} : i\breve{2} = 76° 44'$	$i\breve{2} : i\breve{2} = 77°–78°$ (= $d : d'$ of Scheerer.)
$O : 2\bar{i} = 135°$	$O : 2\bar{i} = 135°$ (= C : B)
2 : 2 (adj.) = 131° 52′	2 : 2 (adj.) = 132° (= $z : z'$)
$2\breve{i} : 2\breve{i}$ (top) = 115° 26′	$2\breve{i} : 2\breve{i}$ (top) = 116½° (= $o : o'$)
$2\breve{3} : 2\breve{3}$ (adj.) = 118° 9′	$2\breve{3} : 2\breve{3}$ (adj.) = 119° = ($t : t'$)

Owing to the disguised character of the mineral, it was named *Prosopite* from *προσωπειον, a mask*—a name certainly well deserved.

The symbols of the planes used above become Naumann's on inserting a P, and putting ∞ for *i*. The plane $i\breve{2}$ ($\infty P\breve{2}$) is that usually considered ∞P.

5. *Geological Survey of Tennessee.*—The State of Tennessee has ordered a Geological Survey of its territory, and appointed to the work Prof. J. M. Safford of Cumberland University, Tennessee. The appointment is a most excellent one. Professor Safford is well prepared for the duties, and his final Reports will, beyond doubt, prove both valuable and honorable to the State and to science.

6. *Telegraphic Longitude at Brussels.*—Under this title, the Athenæum of January 14th, states that the American method of determining difference of longitude by means of the Electric Telegraph, has at last been introduced into Europe. "The Royal Observatory at Greenwich is now permanently connected by one line of wires with the South Eastern and Electric Telegraph System, and by another line with the System of the Submarine and Electric Telegraph." At the observatories of Brussels and Greenwich, about 3000 signals have been observed simultaneously for the comparison of the two transit clocks. The lines are available also for experimenting on the time occupied by the passage of the galvanic pulse from Greenwich to Brussels and the reverse. And from the observations thus far made, it appears that the time is "pretty accurately *one-tenth of a second.*" "Rapid as is the velocity which this implies (about 2700 miles per second, supposing the velocity uniform along the whole line) it is much less rapid than that found in the experiments with Edinburgh (about 7600 miles per second), and still less than that determined on some of the American lines (about 18000 miles per second). The difference undoubtedly depends on the circumstance that the greater part of the line to Brussels is subterranean and submarine, which position of the wires, without in any degree impairing the insulation (which, perhaps, is the most perfect in the world), does by an ill-understood effect of induction, greatly retard the speed of transit."

It is expected that before long Greenwich will be connected in a similar way with the French and Dutch observatories, and these with others over Europe.

"Nearly the whole of Europe is now covered with a net of geodetic triangulation, connecting the western coasts of Ireland and France with the interior of Russia and borders of Turkey. The combination of the geodetic measure with the ascertained difference of longitude will afford one of the best materials for the measure of the earth."

7. *The World of Science, Art and Industry, illustrated from Examples in the New York Exhibition*, 1853 '54. Edited by Prof. B. Silliman, Jr., and C. R. Goodrich, Esq., aided by several scientific and literary men. With 500 illustrations, under the superintendence of C. E. Döpler, Esq. 200 pp. 4to., New York, 1854, G. P. Putnam.—This work, independently of the great number and elegance of its illustrations, has a high value as a record of the Exhibition at New York in 1853, and also on account of the many essays it contains on subjects connected with the recent progress of practical science and the various arts.

8. *The Electro-Magnetic Telegraph*; by Laurence Turnbull, M.D., 2nd edition, revised and improved, illustrated by numerous engravings. 264 pp. 8vo. Philadelphia, 1853. A. Hart.—This work on the Telegraph is practical, scientific and historical, and the best exposition we have of the American system. Moreover, in an appendix it contains several important telegraphic decisions and laws. The work would be improved by an account of the application of the Telegraph to the determination of longitude.

9. *Outlines of a Mechanical Theory of Storms*, containing the true [?] law of lunar influence, with practical instructions to the navigator, to enable him approximately to calculate the coming changes of the wind and weather, for any given day and for any part of the ocean; by T. Barnett. 246 pp. 12mo. New York, 1854. D. Appleton & Co.

10. *Fownes' Chemistry for Students.*—A new edition of this well established and valued work has just been published by Messrs. Blanchard & Lea, of Philadelphia, from the last Lond. edition by Drs. Bence, Jones and Hoffman. The American edition is edited with care by Dr. Bridges.

11. *A Manual of British Mineralogy;* by R. P. Greg, F.G.S., and W. G. Lettsom. 8vo. Price £1 1s.—The British Mineralogy of Messrs. Greg & Lettsom is now in press and its appearance is promised in the course of the season. The work, as we learn, and should infer from our knowledge of the authors, will be a thorough treatise on the minerals of Britain, and will contain a large amount of original matter, descriptions and figures of many new forms of crystals, new analyses, besides statistical and other useful information respecting the mines of Britain and their products. The work will be illustrated by nearly 300 wood-cuts and a few colored lithographic plates, illustrating some unique and remarkable specimens.

12. *My Schools and My Schoolmasters; or, the Story of My Education*, by Hugh Miller. Edinb., Johnstone & Hunter. (Extracts from a notice in the Athenæum of March 11, 1854).—Mr. Miller's grandfather was a buccaneer—his father was a sailor—to whom he was born, the first child of marriage, in the remote shire of Cromarty, A. D., 1802. The boy showed some glimpses of the fierce and piratical thews and sinews of his ancestor in his early days, when he laid the hands of force on his schoolmaster, who tried to flog him because he would not spell "*awful*" in the right way; but earlier even than this, he had become a devourer of books—(and to that remote place the 'Arabian Nights' and 'Gulliver,' and 'Captain Cook's Voyages' and 'Udolpho,' and 'Ambrose on Angels,' penetrated as well as 'the Shorter Catechism,' the Proverbs and the New Testament)—and an observer of the minute aspects and hidden things of nature. He had begun to watch the habits of birds, and to note the colors of flowers, and to try to read that which (to avail ourselves of one of Mr. Ruskin's fantastic figures of speech) creative wisdom has written in the caverns of the earth. His kith and kin, some of them pure Highlanders, were men of marked character,—so that, with these surroundings, and these propensities of mind and endowments of body, there was small chance of the Cromarty boy lacking such adventures as serve a bold spirit for schooling, and an apt intelligence instead of schoolmasters. * * *

Active life, however, must needs be commenced, and as the boy was not particularly amenable (as may have been guessed) to academical discipline, and as his kinsfolk had little choice within their reach, there was no alternative for him save to betake himself to manual labor. Regarding this again, he shall tell his own story.

"The husband of one of my maternal aunts was a mason, who, contracting for jobs on a small scale, usually kept an apprentice or two, and employed a few journeyman. With him I agreed to serve for the term of three years; and, getting a suit of strong moleskin clothes,

and a pair of heavy hob-nailed shoes, I waited only for the breaking up of the winter frosts to begin work in the Cromarty quarries—jobbing masters in the north of Scotland usually combining the profession of the quarrier with that of the mason. * * The quarry in which I commenced my life of labor was, as I have said, a sandstone one, and exhibited in the section of the furze-covered bank which it presented, a bar of deep-red stone beneath, and a bar of pale-red clay above. Both deposits belonged to formations equally unknown at the time to the geologist. The deep-red stone formed part of an upper member of the Lower Old Red Sandstone; the pale-red clay, which was much roughened by rounded pebbles, and much cracked and fissured by the recent frosts, was a bed of the boulder clay. Save for the wholesome restraint that confined me for day after day to this spot, I would perhaps have paid little attention to either. * * *

Though now seventeen, I was still seven inches short of my ultimate stature, and my frame, cast more at that time in the mould of my mother, than in that of the robust sailor, whose 'back,' according to the description of one of his comrades, 'no one had ever put to the ground,' was slim and loosely knit, and I used to suffer much from wandering pains in the joints, and an oppressive feeling about the chest, as if crushed by some great weight. I became subject, too, to frequent fits of extreme depression of spirits, which took almost the form of a walking sleep—results, I believe, of excessive fatigue—and during which my absence of mind was so extreme that I lacked the ability of protecting myself against accident, in cases the most simple and ordinary."

The reader is referred to the work itself for the continuation of the eventful and instructive history, the final result of which, in the able geologist, Hugh Miller, is familiar to all.

13. *Annual of Scientific Discovery, or Year Book in Science and Art for* 1854—exhibiting the most important discoveries and improvements in Mechanics, Useful Arts, Natural Philosophy, Chemistry, &c., edited by David A. Wells, A.M. Boston, 1854. Gould & Lincoln.

14. *Smithsonian Contributions to Knowledge*, vol. v, 1853.—Art. I. Introduction.

II. A Flora and Fauna within living animals; by Joseph Leidy, M.D.
III. Memoir upon the Extinct Species of Fossil Ox; by J. Leidy.
IV. Anatomy of the Nervous System of the Rana pipiens; J. Wyman.
V. Nerealis Boreali-Americana, Part II; by Wm. Henry Harvey.
VI. Plantæ Wrightianæ Neo-Mexicanæ, Part II; by Asa Gray.

15. *Seventh Annual Report of the Board* of Regents of the Smithsonian Institution for the year 1852. Washington, 1853.

Wm. Stimpson: Synopsis of the Marine Invertebrata of Grand Manan, or the region about the mouth of the Bay of Fundy, New Brunswick—From the Smithsonian Contributions to Knowledge. 56 pp. 4to, with two plates. Contains descriptions of many new species and some new genera in the different orders of Invertebrata.

Dr. J. Victor Carus: System der thierischen Morphologie, 506 pp. 8vo, with 97 wood-cuts. Leipzig, 1853. W. Engelmann.

Hanns Bruno Geinitz: Die Versteinerungen der Grauwackenformation in Sachsen und den angrenzenden Länder-abtheilungen. Heft II. 4to, with 20 lithographic plates. Leipzig, 1853. W. Engelmann.

—This work and the preceding were received too late for a further notice in this number.

PALÆONTOGRAPHICAL SOCIETY: Volume for 1853—rich in its contents, containing Part IV of Fossil Corals of Great Britain; Part IV of Fossil Brachiopoda of Great Britain; Part I of Fossil Shells of the Chalk; Cephalopoda, by Daniel Sharpe, F.R.G. & L.S; Part II of a Monograph of the Mollusca from the great Oolite, chiefly from Minchinhampton, and the coast of Yorkshire, by J. Morris and John Lycett; Part III of the Crag Mollusca, by Searles V. Wood; Part IV of the Fossil Reptilia of Great Britain, by Richard Owen. Illustrated by numerous plates. The annual subscription to the Palæontographical Society is one guinea.

E. SABINE: Observations made at the Magnetical and Meteorological Observatory at Hobarton, in Van Diemen Land, printed by the order of Her Majesty's Government, under the superintendence of Col. Edward Sabine, of the Royal Artillery. Vol. III, commencing with 1846. 622 pp. 4to. London, 1853.

American Association for the Advancement of Science.—The next meeting of the Association will be held in Washington, D. C., commencing with Wednesday the *twenty-sixth* of April.

F. OVERMANN: Practical Mineralogy, Assaying and Mining, etc., 2nd Edition. 230 pp. 18mo. *Philadelphia.* Lindsay & Blakiston, 1853.

PERUVIAN ANTIQUITIES, by Mariano Edward Rivero, and John James von Tschudi; translated into English by Francis L. Hawks, D.D., LL.D. *New York*, 1853. G. P. Putnam. A work of great popular and scientific interest.

JAMES D. FORBES: Norway and its Glaciers visited in 1851, followed by Journals of Excursions in the High Alps of Dauphiné, Berne, and Savoy. *Edinburgh*, 1853.

ADOLPH SCHLAGINTWEIT: Über die orographische und geologische Structur der Gruppe des Monte-Rosa. 20 pp. 4to, with 2 tables. *Leipzig*, 1853. Also, Neue Untersuchungen über die physicalische Geographie und die Geologie der Alpen, by Adolph Schlagintweit und Herm. Schlagintweit.

PROCEEDINGS OF THE ACAD. NAT. SCI. PHILADELPHIA, vol. vii, No. 1.—p. 3. On a new Entomostracan of the family Limnadidæ (n. g. Limnadella); *C. Girard.*—p. 4. On the Ancient Alluvium of the Ohio River and its Tributaries; *A. T. King.*—p. 8. Notice of American Animals formerly known but now forgotten or lost; *John Le Conte.*—p. 16. Descriptions of some new Coleoptera from Oregon; *J. L. Le Conte.*—p. 20. Synopsis of the Œdemeridæ of the United States; *J. L. Le Conte.*

ANNALS OF SCIENCE, Cleveland, Ohio. JANUARY.—p. 2. New species of fossil plants; *J. S. Newberry.*—p. 4. Pœcilosoma erythrogastrum, a fish from Rocky river near Cleveland; *J. P. Kirtland.*—p. 5. List of diurnal Lepidoptera of Northern and Middle Ohio; *J. P. Kirtland.*—p. 6. New locality of the Limnea megasoma of Say; *J. P. Kirtland.* FEBRUARY.—p. 44. On the viviparous or ovoviviparous character of a species of the Salmonidæ: *T. Garlick.*—p. 44. Alburnus nitidus; *J. P. Kirtland.*—p. 45. Observations on diurnal Lepidoptera of Northern and Middle Ohio; *J. P. Kirtland.* MARCH.—p. 71. On the artificial reproduction of Fishes; *T. Garlick.*—On the diurnal Lepidoptera of Northern and Middle Ohio; *J. P. Kirtland.*—p. 77. Analyses of Ohio coals; *J. S. Newberry.*—p. 78. Revision of the species of the genus Esox, inhabiting Lake Erie and the River Ohio; *J. P. Kirtland.* APRIL.—p. 100. On Alburnus acutus; *L. A. Lapham.*

Received from the Publishers:

GUSTAV LENZ: Ueber die geschichtliche Enstehung des Rechts. Eine Kritik der historisch Schule. 352 pp. 8vo. *Greisswald* and *Leipzig*, 1854. C. A. Koch's Verlags Buchhandlung, Theodor Kunike.

A. H. BAIER: Symbolik der christlichen Confessionen und Religions-partheien. Erster Band. Symbolik der römisch-katholischen Kirche. Erste Abtheilung. Die Idee und die Principien des römischen Katholicismus. *Greisswald* and *Leipzig*, 1854. C. A. Koch's Verlags Buchhandlung, T. Kunike.

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SCIENTIFIC INTELLIGENCE.

ERRATA.

P. 19, transfer last line to bottom of p. 18.

An error on p. 65 is corrected on p. 300.

P. 415, line 23 from top, dele "all but two of."

Vol. XVII, p. 287, line 10 from top, for "6 P.M.," *read* "10 P.M.:" same page, line 21 from bottom, for "59° 27," *read* "127° 9."

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

Art. I.—*On the first Hurricane of September* 1853, *in the Atlantic; with a Chart; and Notices of other Storms:* by W. C. Redfield.

Since I first ascertained the rotary and progressive movement of storms, in the year 1821, I have on various occasions endeavored to show some of the results which then became obvious or have been established in the progress of more extended investigations.* Of the results thus noticed, the systematic rotation of storms and gales in different regions, their opposite rotation and polar progression on opposite sides of the equator, and the mechanical influence of their rotary action on the movements of the barometer, when viewed in their practical relations, are the most important.

On the present occasion, I propose to give some account of the progression and extent of the earliest autumnal hurricane of 1853 in the north Atlantic. This case is not selected as differing in its essential features from other gales in the same region, but chiefly on account of the unusual extent of route that can be traced by direct observations. For it seems to have been inferred by some, that those gales which have previously been traced and

* See this Journal, vols. xx, xxv, xxxi, xxxv, and xlii, First Series; and vols. i and ii, New Series: with other communications. For further elucidations in this department of meteorology, since the year 1837, see the valuable publications of Col. Reid, Mr. Piddington, Surgeon Thom, and other writers.

their routes shown on our storm-charts, must have originated at or near the places where our first observations were obtained. It is obvious, however, that such inferences are quite erroneous.

It is not deemed necessary in this stage of the inquiry, to give such extensive elucidations of the rotation and geographical relations of the storm-wind as I have shown in the case of the Cuba hurricane of Oct. 1844.* I shall therefore only adduce in concise form such marine reports and observed phenomena as will serve to show the continued cyclonic violence, progress, and extension of the gale.

In submitting the reports by which its general route is established we shall follow the path of the gale westwardly, from off the Cape Verde Islands, crossing the Atlantic to the vicinity of Cape Hatteras, on the American coast, and from thence, on its recurvated course through the higher latitudes of the Atlantic, in the direction of the Spitzbergen sea, and touching, in its vast extension, the western shores of the British Islands.

1. The barque *William Money*, from Bombay, experienced heavy weather, with shifting winds, Aug. 30th and 31st, in lat. 13° N., lon. 29° W., with apparently worse weather in the vicinity.

2. *Independence*, from Talcahuano, Sept. 1st, in lat. 15° 40′, lon. 48° 20′, a violent hurricane, beginning at N. E. and ending at E. S. E.; lost all three topmasts.

3. *Sea*, dismasted in the gale, Sept. 2d, lat. 16° N., lon. 50° 30′ W.

4. *Warwick*, Sept. 3d, severe hurricane, lat. 16°, lon. 51° W.; vessel damaged.

5. *Hermann*, Sept. 3d, lat. 20°, lon. 56° W., severe gale 12 hours: barometer falling as low as 27·30 in. Four other vessels dismasted in this vicinity were reported from one of the Windward Islands; where some heavy weather was experienced at this time. [See note, p. 10.]

6. *Sylphide*, Sept. 3d, lat. 22° 29′, lon. 63°, hurricane; lost topmast, deck load, &c. Two other vessels dismasted Sept. 3d arrived at Havana.

7. *Arve*, night of Sept. 3d, in lat. 22° 30′, lon. 63° 50′, hurricane; disabled and abandoned.

8. *Ocean Bird*, Sept. 4th, [civil time] at 4 P. M. gale increasing from east, with heavy swell, steering south, under double reefed topsails, in lat. 27°, lon. 69°. At 10 P. M. full hurricane from E. N. E.; scudded under bare poles; wind gradually veering round by the north, during the night; hardest gusts from westward; at 4 A. M. Sept. 5th, had got to S. S. W., and hurricane began to abate. During its continuance ran before it, through a curve of eighteen points of the compass.†

9. Brig *Commerce*, bound south, had the hurricane Sept. 5th, from N. E. to S. W., lat. 29° 30′, lon. 69° 50′; was hove on beam ends and dismasted.

* This Journal, vol. ii; 1846: New Series.

† From Capt. Atkinson: The barometer had fallen to 29·10 in the early part of the gale, but was not observed during the period of its greatest violence.

10. *Regatta*, Sept. 6th, lat 29° 20′, lon. 71°, hurricane; veering from N. E. to S. W.; split storm sails; main rail under water three hours; could not be heard four feet, with utmost effort of voice.

11. *Elena*, totally dismasted, Sept. 6th, lat. 32°, lon. 70°.

12. *J. Grierson*, for Gulf of Mexico, Sept. 6th, lat. 31°, lon. 74° 30′, heavy gale from N. N. E., hauling to S. W.; dismasted.——*Caroline*, crippled in heavy blow, Sept. 6th, and put back to Charleston.

13. *Flash*, Sept. 6th, lat. 33° 40′, lon. 76°, hurricane from N. E. to W.; damaged.

14. *Dione*, Sept. 6th, lat. 33° 15′, lon. 77° 20′, hurricane from E. N. E.; lost topsails, top-gallant-masts, &c.

15. *G. W. Lawrence*, Sept. 7th, lat. 33°, lon. 75°, dismasted in hurricane from E. N. E.

16. *Norfolk Packet*, hurricane, Sept. 7th, lat. 33° 50′, lon. 76° 20′; dismasted.

17. *Levant*, dismasted Sept. 7th, lat. 34° 10′, lon. 74° 10′, in a terrific gale from the eastward.——*John Adams*, dismasted in hurricane, 6th–7th of Sept. on south side of gulf stream.

18. *Viola*, dismasted in hurricane, Sept. 7th, near lat. 34° 16′, lon. 73°.

19. *Segesta*, Sept. 7th, lat. 34° 32′, lon. 72° 30′, hurricane, S. S. E. to N. N. W.; dismasted.

20. Steamship *Georgia*;* Sept. 6th, barometer at noon 29·85; lat. 38° 9′, lon. 73° 55′; wind southward, with a large heavy swell from southward and eastward, indicating a blow in that direction; during the night cloudy, with rain. Sept. 7th, [civil time] commences cloudy with fresh breezes from southward and eastward, and heavy cross swell: bar. at 1 A. M. 29·78, 4 A. M. 29·76, wind freshening to a gale: at 9 A. M. bar. 29·45, blowing heavy and sea rising. At noon bar. had descended to 29·10, wind still increasing and sea high; steamer's position about 80 miles E. of Cape. Hatteras; [lat. 35° 14′, lon. 74° 10′]; 1 P. M. bar. 28·40; blowing very heavy in squalls. At 2·30, moderating; 3 P. M. bar. 28·20, wind came out from northeastward, with exceeding heavy squalls; the sea-drift flew across the decks with great fury; no one could withstand its force; at 4 P. M. bar. 28·10, blowing harder than ever, and so continued till 6·30 P. M.; skylights and part of hurricane deck blown away; at 10 P. M. still blowing heavy and sea high: Midnight bar. 29 in., wind still subsiding; and at daylight on the 8th, had abated sufficiently to make sail; 8 A. M. weather moderating. Noon, lat. 35° 40′, lon. 72° 48′.

21. *Rescue*, Sept. 7th, lat. 36°, lon. 74° 30′, hurricane; dismasted.

22. *Albemarle*, capsized in the hurricane at midnight on the 7th, lat. 35° 30′, lon. 73°; all lost except one seaman.

23. *Lyra*, bound south, Sept. 7th, lat. 35° 50′, lon. 73° 30′, hurricane, from E. S. E. to W. N. W.; dismasted.

24. *Fanny*, Sept. 7th, lat. 35° 09′, lon. 71° 39′, hurricane, dismasted. ——*Algorna*, for the Chesapeake, dismasted in lat. —, lon. 70°. [Either the date or the latitude erroneously reported].

25. Brig *Swan*, Sept. 7th, [civil time] at noon, lat. 36° 26′, lon. 71° 44′, had reduced to short sail, gale S. E. by S., and high sea for last

* From reports and memoranda of Capt. Budd.

24 hours; standing south; 2 P. M. heavy gale; furled foresail and hove to; 4 P. M: gale increasing, furled topsail and scud before it under storm stay-sail, then bare poles; 6 P. M. hurricane, E. S. E.; broached to on port-tack; 7 P. M. wind truly terrific; thrown on beam-ends; dismasted about 9 P. M; barometer 28·85: [28·94 as compared with mine], at 11 P. M. force of hurricane began to abate, and before midnight gale had veered to E. N. E. At 4 A. M. Sept. 8th, gale N. N. E. moderating; 8 A. M. stiff breezes; noon, moderate; lat. 36° 13′, lon. 72° 40′.

26. *Star*, Sept. 7th, lat. 36° 10′, lon. 72°, hurricane, from N. E. to W.: dismasted.

27. *Addy Swift*, Sept. 6th, [civil time] fine weather, lat. 37° 20′, lon. 71° 30′, bar. 30·10 in., *a very heavy swell from S. E.*, gradually increasing; wind from S. W. to N. E. Sept. 7th, fine; light winds from N. E.; 8 A. M. bar. 30 in.; at noon, lat. 36° 30′, lon. 71°; wind S. S. E., a double reefed topsail breeze; western horizon very hazy; 2 P. M. cloudy, wind increasing; bar. 29·90; 5 P. M. sails furled, hove to under storm try-sail, gale S. S. E., slight rain, and tremendous sea running; bar. 29·50; [six hours from gale's center]. 7 P. M. severe hurricane from S. E.; at 8 P. M. vessel on her beam-ends, heading within five points of the wind; remained in this position until 10 P. M., when I cut away the mast, and she righted. At 11 P. M. it was almost calm; at midnight the wind came from the west, with increased violence. The wind veered with the sun, that is, from S. E. to West. Sept. 8th at 4 A. M. wind decreasing: at 6 A. M. clear weather and moderate winds. During the heaviest of the gale, the sea was smooth.*

28. *Clarissa*, Sept. 7th, lat. 36°, lon. 70°, severe hurricane from 8 P. M. till next morning, from S. S. E. to S. W.; thrown on beam-ends, with loss of topmast, sails, rudder, &c.

29. *B. L. Swan*, night of Sept. 8th, [nautical] lat. 37°, lon. 71°, hurricane, from E. S. E. going round [probably by N.] to N. W.; dismasted.

30. *Olivier*, Sept. 8th, lat. 36° 37′, lon. 69°, hurricane, from S. W. to N. E.; dismasted.

31. *Octavia*, Sept. 8th, lat. 37° 05′, lon. 68° 04′, severe gale from S. and heavy seas; damaged, and one man lost.

32. *J. W. Buddecke*, dismasted in the gale, and foundered: crew picked up on the 11th, lat. 39°, lon. 65°.

33. *Adrian and William*, Sept. 9th, lat. 39° 50′, lon. 66° 50′, hurricane; dismasted.

34. *Nauticon*, Sept. 8th, lat. 33° 15′, lon. 65°, at daylight gale commenced from S., and increased rapidly: boat's bulwarks and spars swept from the ship.

35. *Bessie Grant*, Sept. 8th, lat. 37° 30′, lon. 63°, in hurricane from N. N. E. was thrown on beam-ends and dismasted.

* Letter from Capt. Berry. Several other captains report a like effect of the most violent portion of the hurricane, in smoothing down the sea. Capt. Berry states, that from 7 to 11 P. M. the cards of his compasses were flying round from east to west like a top; perhaps at the rate of thirty times a minute. When the gale was from the western quarter the compasses were steady.

36. *Revenue*, Sept. 8th, lat. 38° 30′, lon. 64° 48′, at 10 A. M. the hurricane blew all sails from the yards; at 12 30 still increasing; hove on beam-ends at 1·30 P. M., and dismasted; at 5 P. M. hurricane abated.

37. *Liverpool*, near lat. 41° 02′, lon. 68° 23′, Sept. 9th; gale commenced from eastward, blowing hard at N. E. and N. N. E. about four hours, late in the afternoon, and veering westward; barometer at noon 29·20 in. [Estimated as 210 miles to the left of the center path].

38. *Abner Taylor*, Sept. 8th, lat. 39° 30′, lon. 66° 20, hurricane, from S. E. to N. W.; dismasted.——*Georgiana*, dismasted on the 8th, in lat 40°, and abandoned.——*Cairo*, thrown on beam-ends, on southern edge of gulf stream, and abandoned.

39. *Glamorgan*, Sept. 9th, lat. 40°, lon. 65°, hurricane, from E. N. E.; dismasted.

40. *Saragossa*, dismasted in violent hurricane, Sept. 8th, lat. 39°, lon. 63°.

41. *Queen of Sheba*, Sept. 7th, a hurricane, in lat. 39° 50°, lon. 64° 35′, in which lost spars, sails and bulwarks; with other damage.

42. *Juanito*, Sept. 8th, severe gale from N. E., lat. 40°, lon. 64°; thrown on beam-ends and dismasted.——*Tarquin*, Sept. 8th, thrown down in the hurricane, in lat 40°; lost sails, topmast, &c.

43. *Conqueror*, Sept. 8th, lat. 38°, lon. 59°, hurricane; dismasted and filled.——*Haabet*, dismasted Sept. 8th, westward of the Grand Bank; abandoned.

44. *Matchless*, Sept. 8th, lat. 39° 29′, lon. 59° 45′, severe hurricane from south four hours, when it died away and suddenly shifted to the west, blowing very violent; dismasted.

45. *Ionian*, night of Sept. 8th, lat. 40°, lon. 60°, took the hurricane from S.; which shifted to N.; was hove on beam-ends and dismasted.

46. *Henry Harbeck*, Sept. 8th, hurricane commenced at noon from the southward, lat. 40°, lon. 56°. While lying to was struck by a sea, on larboard side, with loss of bulwarks and deck house; five men lost or disabled. At 3 A. M. blowing harder, lost topmasts and sails. Ship foundered.

47. *Tuscarora*, on beam-ends, with loss of sails, &c., in a violent hurricane, Sept. 9th, lat. 41°, lon. 57°, W.

48. *Cadet*, damaged in heavy gale from N. W. Sept. 10th, lat. 43° 30′, lon. 61° 20′ W.

49. *Independent*, Sept. 9th, lat. 40° 20′, lon. 50° 30′, hurricane from S. W. to N. W.; lost topmasts, sails, &c.: at 11 A. M. the hurricane blew with its utmost fury; and the barometer had then fallen to 27·75 inches.*

50. *Wildfire*, Sept. 9th, lat. 42° 04′, lon. 51° 21′, at 11 A. M. under close reefed topsails, wind E., was struck by the hurricane and hove on beam-ends: lost mainmast, topmasts, &c., and one man.

51. *Albert Gallatin*, Sept. 8th, lat. 39°, lon. 48°, severe hurricane six hours, from S. S. E. to N. W.

52. *Charles Humbertson*, Sept. 9th, lat. 43° 16, lon. 45°, in the gale under double reefed topsails, wind suddenly changed from S. to N. without any warning and blew a hurricane; lost sails, &c.

* From the official report and protest of Capt. Smith.

53. *London*, Sept. 9th, squally, with rain; 9 A. M. barometer 29·80: noon, lat. 43° 13′, lon. 44° 12′; at 2 P. M. barometer 29·60; blowing harder; 3 P. M. bar. 29·40, with appearances of a heavy blow; wind S. veering to S. W.; 4 P. M. bar. 29·20; nearly calm, but looked threatening: At 4·30 P. M. bar. 29 inches, when the blast struck us from N. W., like a discharge of cannon; went before it furiously; burst the spencer and sprung main-yard; ship settling away every few seconds as if going down. At 5 P. M. bar. had risen to 29·20; at 6 P. M. the fury of the hurricane was broken, but the gale blew from N. W. through the night; moderating at noon of 10th in lat. 42° 27′, lon. 46°; so that at 2 P. M. we had set fore and main topsails.* [Estimated 165 miles to the right of center path.]

54. *Connecticut*, Sept. 9th, lat. 44° 30′, lon. 47°, terrific hurricane; at 10 A. M. broached to; lost spars and sails, with four seamen; at noon, gale gradually abated; ship lying to on port tack, with head N. E.

55. *Washington*, Sept. 10th and 11th, lat. 46°, lon. 48°, heavy gale at S. S. W., veering round to the northward.——*Wilton*, from Jamaica, was dismasted in the gale Sept. 10th.

56. *Nathaniel Thomson*, Sept. 10th, lat. 42° 26′, lon. 38°, severe hurricane from S. S. W. to N. N. W. for 12 hours; ship on beam ends for three hours; lost all sails.——*Juno*, for Bremen, had the hurricane Sept. 9th, lost three men: was spoken 13th, lat. 41°, lon. 42°.——*Kezia*, from Mirimichi, encountered it on the 10th, from S. E., with much damage.

57. *Mercury*, Sept. 12th [?], lat. 44°, lon. 41°, took the hurricane from S.

58. *Hibernia*, Sept. 10th, lat. 45°, lon. 42°, hurricane, from N. E. to S. W.

59. *Ossippee*, Sept. 10th, lat. 46° 30′, lon. 42° 30′, very heavy gale from S. E. to N.; split sails, stove bulwarks, &c.

60. *Western Empire*, Sept. 9th, lat. 46°, lon. 36°, hurricane, from S. E. and S. to N. N. E.; lost spars and sails, with other damage.

61. *Sardus*, Sept. 9th [10th ?], lat. 43° 16′, lon. 32° 24′, in a hurricane, lost sails and bulwarks; with other damage.

62. *Burlington*, Sept. 10th, lat. 40° 45′, lon. 29°, severe gale, 16 hours.

63. *John Winthrop*, Sept. 9th, lat. 35° 48′, lon. 29° 30′, severe hurricane, from S. W. to N.; lost spars, sails, &c.

64. *Olympus*, Sept. 10th, lat. 36°, lon. 27°, hurricane, with loss of topmasts, sails, and rigging.

65. *John Dunlap*, Sept. 11th, lat. 46° 15′, lon. 32° 15′, hurricane; lost sails, &c.

66. *Eli Whitney*, Sept. 11th, heavy gale, with damage. Sept. 12th, lat. 47° 19′, lon. 30° 38′, saw *Barbara Ann*, disabled.

67. *Clara Wheeler*, Sept. 10th, lat. 49°, lon. 35°, was thrown on beam-ends, with loss. 11th, gale still continuing, saw a large ship in dismasted condition.

* From Prof. C. U. Shepard, then passenger on the London.

68. *Robert Kelly*, Sept. 10th, lat. 46° 30′, lon. 31°, hurricane; lost sails, &c.: barometer fell to 28·15 inches.

69. *Rialto*, in the gale, lat. 50° 28′, lon. 35° 43′, shipped a heavy sea, filled cabin, shifted cargo, &c.

70. Brig *Elizabeth*, Sept. 9th, lat. 45°, lon. 29° 30′, heavy gale from S. E. to N. E.; hove on beam-ends while under bare poles; gale abated at 4 P. M. next day.

71. *Stephen Glover*, Sept. 10th, lat. 47° 13′, lon. 30° 16′, hurricane from N. W. and W. S. W.; thrown on beam-ends and dismasted.

72. *Emperor*, Sept. 10th, lat. 47° 30′, lon. 30° 30′, severe gale from S. S. E. to N. W. ending in a perfect hurricane; lost sails, spars, &c.

73. *Royalist*, dismasted in the gale, Sept. 10th, lat. 48°, lon. 30° 30′; abandoned.

74. *Southerner*, Sept. 9th, ended with increasing gale from E. S. E.: 10th, at 4 A. M. gale heavy from N. E.; 5 A. M. a hurricane; 7 A. M. broached to under bare poles; 2 P. M. wind hauled to N. N. W. blowing tremendously; barometer 28·27 in.; 7 P. M. heavy cross sea; 10 P. M. 7 feet water in hold: at 11·30 P. M. crew took to the boat. Sept. 11th, at 6·30 A. M. ship went down head foremost, in lat. 47° 15′, lon. 30° 24′.

75. *Caroline*, Sept. 10th, lat. 48° 12′, lon. 30°, gale commenced in heavy squalls from E. S. E., soon hauling to different points of the compass, and blowing a hurricane; laid six hours under bare poles; the furled canvass blown from yards, with other damage.

76. *Harvest Queen*, severe hurricane Sept. 10th, lat. 47° 10′, lon. 29° 30′; from S. S. W to N. W.

77. *George Hulburt*, for Havre, violent gale Sept. 10th, between lat. 48° and 49°; lon. 30°; was hove down and lay many hours on port-side.

78. *Palermo*, Sept. 10th, lat. 49°, lon. 31°, in hurricane from S. E., decks swept, with loss of the mate: gale continued next day from N.W.; in lat. 48°, lon. 30°. On three following days strong winds from S. W. to N. W., to lon. 18°.

79. *William Hitchcock*, lost sails, &c. in the hurricane from W. S. W. Sept. 10th, lat. 46° 30′, lon. 27°.

80. *Devon*, in violent gale, under bare poles, Sept. 12th [?], lost spars, bulwarks, binnacle, &c. &c.; lat. 46° 33′, lon. 26° 40.

81. *Victoria*, dismasted and water-logged in terrific gale from westward, Sept. 10th, lat. 47° 17′, lon. 27° 09′.

82. *Chesapeak*, Sept. 10th, lat. 47° 10′, lon. 27° 30′, severe hurricane from S. E. to N.; received much damage.

83. *Metropolis*, Sept. 10th, lat. 47°, lon. 26°, gale from S. W. to N. W.; dismasted.

84. *Josephine*, hurricane, Sept. 10th, lat. 47° 01′, lon. 24°.——Next day, passed *Lady Seymour*, dismasted.

85. *Larpqol*, Sept. 11th, lat. 48°, lon. 25°, heavy gale; on beam-ends four hours, with much damage; barometer, 28·52 inches.

86. *Solway*, abandoned Sept. 11th, lat. 48° 30′, lon. 24° 30′, at 9 A. M: wind then heavy from N. Was wrecked the day previous in the gale.

87. *Alexina*, hurricane, Sept. 11th, lat. 46°, lon. 22° 50′; hove on beam-ends, with much damage.

88. *Brown*, Sept. 10th, lat. 49° 45′, lon. 25°, lying to with strong gale from S. E.; about 4 P. M. it fell dead calm for about half an hour, while rain fell in torrents; at 4·30 a sudden gust came up from the west, and continued to blow a perfect hurricane; ship hove to under bare poles, and leaking badly: 8 P. M. hurricane as violent as ever. At 1 A. M. vessel fell over and was dismasted; crew taken off on 16th.

89. Barque *Elizabeth*, at Quebec, reports; Sept. 11, lat. 47° 56′, lon. 22° 07′, experienced a hurricane from S. W. which proved to be a revolving storm. At midnight, wind veered from W. to W. N. W. At 2 A. M. being most violent, it blew away the close reefed topsails. The ship being laid to with head to the southward, escaped the vortex.*

90. *Avalanche*, Sept. 10th, lat. 48°, lon. 20° 15′; at 4 P. M. [civil time] gale very severe at S. S. E.; brought the ship under a single topsail; [bound west] at 5 P. M. barometer 28·50, was struck with a heavy gust from N. W., and thence twice round the compass: 6 P. M. lying to under bare poles, barometer 28·70; gale, after the crisis, mostly N. N. W. by compass. [N. W. nearly.] Sept. 11th, at 8 A. M. wind N. W., and so far moderated as to allow a close reefed main-topsail.†

91. *Rufus K. Page*, in the gale, Sept. 11th, lat. 39°, lon. 17°, was struck by a heavy squall from the northward and dismasted.

92. *Barque Swan*, from Lisbon, Sept. 12th [?], lat. 36° 50′, lon. 15° 25′, severe gale from E. N. E. round by S. to N. W.

93. *William Ray*, Sept. 11th, lat. 49° 10′, lon. 20° 30′, hard gales; at 4 P. M. furled all sails and hove to, in a mountainous sea; midnight, dreadful sea, ship lay on her broadside; 6 A. M. got before the wind under double reefed fore-topsail; water-logged; abandoned on 14th. The gale veered from S. by the W., to W. N. W.

94. *Euterpe*, Sept. 9th, lat. 48° 42′, lon. 19° 30′, severe hurricane, which came on at S. E. and abated at N. W.

95. *Esther G. Barney*, severe gale Sept. 10th, lat. 48° 04′, lon. 18° 36′; threw over part of cargo.

96. *Nicholas Biddle*, lat. 52°, lon. 19°, dismasted Sept. 14th [?], while lying to in a gale from N. N. W.

96 *a*. R. M. steamer *Andes*, severe gale, S. S. W. veering to N. N. W.; lat. 51° 30′, lon. 18° 30′; barometer 28·48.

97. *Constantine*, lost sails, top-gallant-masts, and sprung fore-topmast in the gale from W. S. W., Sept. 14th [?], lat. 52° 34′, lon. 17° 30′

98. *Devonport*, took the gale Sept. 10th, lat. 54°, lon. 22°; continued till 6 A. M. of 12th, with heavy sea.

99. *Commerce*, lost spars and sails in the gale, Sept. 10th, lat. 48° 53′, lon. 13° 40′.

100. *Mary Glover*, in gale from S. W., Sept. 11th, lat. 50°, lon. 14°, lost mainsail, with other damage.

101. *Susan & Sarah*, Sept. 11th, lat. 55° 20′, lon. 15° 30′, in severe gale from S. W. was hove on beam-ends; lost mizenmast and one man. Returned to port.

* It too often happens that ships, when running westward in a southerly gale in these latitudes, and being thus in the right side of the storm-path, are hove to unwittingly on the *port-tack;* perhaps as more convenient, or with a view to avoiding some loss in distance. Of the increased danger of this tack, *when in the right side of the storm-path*, in the northern hemisphere, every navigator should be informed.

† Statement of Capt. Leach.

102. *Anne*, from Orkney, for Limerick, lost bulwarks in the gale, with sundry other damages; was as far as lat. 55° 20′, lon. 10° 15′ W.; hove to for sixty hours, and drifted off Tory Island; never experienced such a sea.——*Neptune*, with loss of foremast, was passed Sept. 13th, lat. 50°, lon. 25° 40′.

103. *Zanoni*, from Greenock; Sept. 11th, lat. 57°, lon. 15°, heavy gale; sprung a leak, and was abandoned. This position is on the Rockall Bank.

104. *Virginia*, from Gothenburg, Sept. 12th, [naut.?] in lat. 60° 40′, lon. 11° 34′, encountered a heavy gale and was struck by a heavy sea which caused the ship to leak: in continuance of the bad weather, lost topmasts, and bore up for the nearest port. Had good weather previous to this gale.*

The master of an English brig who was off the Lands End of Cornwall in this gale, reported to Capt. Leach of the Avalanche that its strength was only sufficient to bring him to two-reefed topsails.

At Scilly, Sept. 10th, wind S. S. E. to S. W. fresh, with heavy rain; bar. 29·50: Sept. 11th, wind S. S. W., fresh, with rain; bar. 29·70, to 29·75.

At Holyhead, 70 miles west of Liverpool, Sept. 10th, wind E. to S. E., fresh: 11th, S. W., hard gale and squally.

On the western coast of England and coast of Ireland, the exterior portion of this cyclone set in from the eastern or southern quarter, and veered to the westward as the body of the storm passed on to the northward; with "a very heavy ground sea on the coast."

At Tobermory, I. of Mull, lat. 56° 37′, lon. 6° 04′ W., Sept. 11th, A. M., a heavy gale from N. E.: evening more moderate. The variation being 28¾° W., shows the wind at N. 6¼° W., true. In view of the trending of the strait and the high land along its northern opening, this may consist with an outside gale from N. N. W.

The foregoing accounts show the right border of the storm to have extended to the shores of England; but in no extraordinary force, and with a depression of less than half an inch in the barometer: while the axial area, with the more active portion of the cyclone, appears to have passed over the Rockall Bank or on a center-path still more distant from the British Islands, in its course toward the polar basin. See Track XXIV on the Chart.

In reviewing the daily progress and phenomena of the storm, it should be recollected that most of the sea accounts are given in nautical time, and thus are often one day in advance of the calendar. The direction of the winds being given by compass,

* The whole number of vessels noticed in the foregoing reports, is 125; of which 104 are seen to have reported their several positions at the time of the gale, or the same is otherwise indicated. I have a further list of 17 vessels, lost or disabled in the gale, making an aggregate of 142 vessels reported. Of these, no less than 75 were lost or dismasted; and 46 were crippled, or damaged: while of the remaining 21, no report of injuries was made.

Of this vast series of disasters, not one was occasioned by rocks, shoals, or a lee shore. The fate of a far greater number of vessels (probably of less commercial value,) which were doubtless exposed in this gale, cannot now be ascertained.

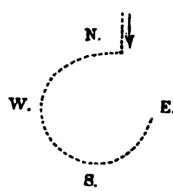
a correction of ten to thirty two degrees is required for the westerly variation, from off Georges Shoals to the Rockall Bank, and the shores of Europe.

The first report in the above series is important, viewed as the earliest notice, and as from a region long supposed to be free from hurricanes and gales; of which more hereafter.

The commencement of the gale at N. E. with the *Independence* (2) marks nearly its center-path. On carrying back the trace-line derived from this and subsequent reports, it appears that the *William Money* (1) was north of this line, in the right side of the gale, as relates to its course of progression. The Independence was also on the right of the center-path, during the middle and latter part of the gale, as is shown by the veering of the wind: both vessels being bound northward. The positions reported, are probably those of the noon immediately preceding the onset of the gale.

The *Hermann* (5) was bound southward. Hence her final position in the gale was probably more southward than that reported. The remarkable fall in her barometer showed a position near to the axis of the storm.*

The *Ocean Bird* (8) first encountered the gale at E. N. E., which places her in the right-front of the storm. Had she then hove to on the starboard tack, the wind would have veered by the east, as with (2); but steering south, and then scudding before the wind during the night, she crossed the center-path and ran partly round the axis of rotation; and had the gale been slower in its advance, she might thus have completed an entire circuit. This case of running with the wind, with another which I shall adduce, may be of interest to some who think we have not shown the wind's rotation.



The rate of advance appears to have lessened as the storm approached the line or axis of equal diurnal motion; the position of which, on this occasion, appears to have been somewhat above the 30th parallel of latitude.†

* In the New York Daily Times, I find a letter of which the following is an extract; which, with an obvious correction, probably gives the true place of the Hermann in the gale:——"St. Thomas, Sept. 19, 1853. On the 4th, the barometer fell, and the wind was fresh from the North, giving rise to some apprehension that a hurricane was at hand. But it was only the wind from the wings of one passing just to the north of our island. The Danish bark *Hermann* encountered it on Sept. 2nd, lat. 53°, and lon. 18°, [obviously *lat.* 18°, *lon.* 53°]. On the 4th, it had arrived to the northeast of us, where the French brig *Diamond*, and the American brig *Carlton*, from Boston, and the schooner *Ann Maria*, from Baltimore, struggled with its fury. Col. Reid's Theory, or rather Mr. Redfield's Theory of the gyratory movements of these storms, can no longer be doubted. A vessel was seen by the *Diamond* on the 4th, dismasted."

† From the physical relations between the diurnal rotation of the earth's crust and that of the immediately incumbent atmosphere, which result from the inertia

The progression of the gale, as in former cases, appears to have been greatly accelerated after it passed the axis of equal diurnal motion, on its recurvated course through the temperate and higher latitudes. The following estimates are roughly made, by setting off the progression on the Chart, as shown in trace XXIV, reckoning in English miles, at seventy for a degree of the meridian.

From a point opposite the position of the William Money (1) to that opposite the Hermann (3)

		Hours.	Miles.	Av. per hour.
Say,	- - - -	84	1942	22
To position of	Ocean Bird (8)	50	980	19·6
"	Georgia (20)	62	814	13
"	Addy Swift (27)	7	175	25
"	Independent (49)	36	1102	30·6
"	Avalanche (90)	30	1505	50
"	Virginia (104)	15 (?)	758	50·6 (?)
		284	7276	

and unstable mobility of the latter, the great storms of the inter-tropical regions must necessarily have a westerly progression; the rate of which denotes the existing difference of the diurnal motion. It is shown also by numerous investigations of these storms, that their westwardly movement as integral portions of the lower atmosphere, is united, in most cases, with a constant movement *from the equator.* Hence the relative movement becomes northwesterly; and is continued until the storm reaches a parallel of latitude where the diurnal motion of the earth's crust and that of the lower incumbent atmosphere are equal. This line, or belt, I call the axis of *equal diurnal motion.* It often marks the exterior limits of the trade winds; and its position is found to vary at different times, and in different seas or regions: good examples of which are found in the several points of recurvation seen in the storm-tracks on the Chart. On crossing this axis, the diurnal motion of the lower atmosphere is found to exceed that of the earth's surface, and produces a relative movement from the west, which, combined with the continued movement from the equator, determines the route of the storm through the temperate latitudes.

This prevailing tendency or movement from the equator, in the inferior strata of the atmosphere, is equally developed in the temperate latitudes, at all seasons of the year. It is probably due to the general gravitation of the atmosphere, acting counter to the centrifugal effect of the earth's rotation: for the latter force, owing to the greater axial radius, is necessarily greatest in the higher atmosphere, in regions beyond the cognizance of our observations.

In those cases where the easterly and westerly currents of rotation are less active, or in other words, when the diurnal motion of the lower atmosphere is least unequal to that of the earth's surface, the alternate westerly and easterly progression of the storm becomes greatly modified, in degree, though subject to the same general law of planetary dynamics. This may be seen exemplified on the Chart, in tracks VII, XIX, XX, and Col. Reid's Bermuda hurricane, of Sept. 1839. This dynamical law governs the progress of all cyclones, however gentle in their rotary action; and necessarily applies to the general movements of the lower atmosphere.

The *lower atmosphere,* in my apprehension, includes all that portion of the atmosphere in which the direction of its currents can be discerned by means of natural phenomena; and, in the largest sense, with an upper limit not higher than is indicated by a pressure of fourteen inches of the barometer. In a more restricted sense, I would apply it to the lower winds and currents so far only as to include an elevation of four thousand, to six thousand feet above the surface. The latter will prob-

Thus we have an estimated distance of 7276 miles, traversed by the storm in about twelve days: at an average rate of progression of nearly 26 miles an hour.

The slower rate of progression at and near the axis or belt of equal diurnal motion, accords with results ascertained in my previous inquiries, and with those severally shown by REID, THOM, and PIDDINGTON, in the gales of the Southern Ocean and the Asiatic Seas.

In my approximated delineation of the axis line or center-path on the Chart, I have had reference to the path of greatest violence, where observations were had from both sides, and especially to the opposite veering of the wind, which is found in the opposite sides of an advancing cyclone. Northeastward of Cape Hatteras, we find the storm-center to have passed between the *Georgia* (20) and the *Swan* (25) on one hand, and the *Addy Swift* (27) on the other.

The incurvation of the storm-path toward the Azores is quite remarkable. This feature I first noticed in the case of the storm of Sept. 1846, as shown by observations extending to lat. 62° 30′, far beyond the limits of my former map, on which its track (XIX) was first delineated. This tracing, with that of the hurricane of August 1851, (XXII) has been copied on this Chart, and extended without further alteration. The number and extent of the reports obtained in the present case, have induced me to delineate this feature more fully than I first intended. Its probable relation to the expansion and perhaps the falling off of the southern portion of the cyclone toward the equator, may be considered hereafter.

Eastward of the Grand Bank, nearly all the reports are from the right side of the storm-path, and so far as appears, mostly at great distance from the true axis path of the gale. This may be owing to the diminished violence of the two left quadrants of the cyclone, caused by its accelerated progression, as well as to the paucity of reports from the more northern portions of the Atlantic, which are less frequented by navigators. That the cyclonic nucleus of the storm had become greatly enlarged, and that it pursued the general course I have indicated, appears from its wide-spread violence, and the intensity and wide extent of its influence on the barometer, and, especially, by the most northern reports. From these few northern reports, we see this violence

ably include the entire volume of all our great storms; except on high table lands or in mountainous localities.

But as regards the movements in the upper atmosphere, in regions higher than the limits first mentioned, almost nothing appears to have been yet learned; although inferences, urged with great confidence, have been sufficiently common. These inferences may be as variant as the hypotheses on which they are founded; and seldom appear reconcilable with visible phenomena, if these be widely and carefully considered.

extended beyond lat. 60°, in the general direction of the Feroe Islands, and the main entrance to the Arctic Sea.

In other cases we find, that after passing the latitude of Bermuda, the expansion of the storms is often so great that their southern portions advance nearly from west to east; but reach the successive meridians no sooner, and sometimes even later than the axial portion of the gale, which pursues a more northeasterly course. Thus in the present case, east of lon. 60°, and between lat. 35° 30′ and lat. 42° 30′, a belt of seven degrees, we have a series of thirteen observations, carrying us east to lon. 15° 25′, in lat. 36° 50′; almost to the southwestern extremity of Europe. See reports 43, 44, 46, 47, 49, 51, 56, (including *Juno*,) 62, 63, 64, 91, 92. If, instead of the broad range of our present inquiry, we were limited, as in the United States, to fewer parallels of the temperate latitudes, how readily might an east progression of the storm be shown, by these partial observations, and its true course remain unnoticed.

But I apprehend this southward expansion of the storm to have been due to something more than the centrifugal force of the cyclone, acting against the statical pressure of the circumjacent atmosphere. In such a wide-spread cyclone, whose diameter on the 9th of September extended from Newfoundland,* to beyond the Azores, or more than 1500 miles, how could its vast entireness be much longer maintained, against both the centrifugal and gravitating forces of the earth, acting in opposite directions, and with opposite degrees of effect, or predominance, on the sides respectively nearest the equator and the pole? May we not suppose that the southern portion of the gale was in process of separation or falling off toward the equator, and thus supplying the influx which sustains the inferior trade winds in northwestern Africa and the eastern Atlantic? And is not such a view supported, in some degree, by the gradually altered course of the nucleus of the storm, which in becoming more free from the equatorial influence, is left to pursue its course toward the polar basin under the predominating influence of direct gravitation?

In reference to these questions, it may be stated that the Steamer *City of London*, in her passage from the Mediterranean, where she had fine weather, left Gibralter Sept. 11th, and encountered strong gales from N. E. with heavy sea; arriving at Southampton on the 18th;—thus showing a brisk movement of the winds, at this period, toward Madeira and the lower latitudes.†

* Through the kindness of R. Dinwiddie, Esq., I am in possession of observations made at Harbor Grace, in Trinity Bay, N. F., lat. 47° 40′, lon. 53° 16′, which show that the left margin of the storm touched that place Sept. 9th, with a stiff wind from N. E., and cloudy.

† Taking into view the greatly diminished force of this gale at the entrance of the English Channel; and that we have no notice of its action in the Bay of Biscay, while we trace its violence *continuously* from off Cape Hatteras to the Rockall Bank and beyond lat. 60° on one hand, and to lat. 36° 50′ and lon. 15° 25′ on the other, with all the characteristics of a severe gale, I confess that some solution like the

EFFECT OF THE STORM-WIND ON THE BAROMETER.—The unfailing mechanical effect of the cyclonic wind in producing a fall of the barometer within the area of its circuit, and greatest in the axial region of the cyclone, is clearly seen in this storm. The following are the cases reported in which the barometer fell below 29 inches: to which I have annexed a rough estimate of the probable distance of the vessel from the axis of the storm at the time of nearest approach.

	Minimum of Barometer. Inches.	Supposed distance from gale's axis.
Hermann (5)	27·30	*near.*
Georgia (20)	28·20	45 miles.
Swan (25)	27·94	30 "
Independent (49)	27·75	220 "
Robert Kelly (68)	28·15	265 "
Southerner (74)	28·27	240 "
Larpool (85)	28·52	315 "
Avalanche (90)	28·50	430 "
Andes (96 *a*)	28·48	280 "

From observations of the barometer and winds taken at various points in the United States near the Atlantic coast, and from those made at the signal station in Bermuda, it appears that the storm was but little felt at the latter place, except as exhibiting the true cyclonic wind, from 6th to 8th, from S. E., veering gradually from S. E. to S. W., as the bearing and progression of the storm became changed; with a force of wind marked from 2 to 4; the barometer at 30·10, at noon of 5th, 6th and 7th and 30·07 at noon of 8th. The left side of the storm encroached to some extent upon the eastern borders of North Carolina and Virginia. At Scuppernong, N. C., lat. 35° 50′, lon. 76° 20′, the cyclonic wind blew from N. E., with a force marked 3 and 2, with rain; and the lower stratum of clouds [the true storm-scud or cyclonic stratum] flew rapidly from N. E.; *the upper clouds quite still.* At 9 P. M., the wind had veered to S. W. No observations of barometer. At Fort Monroe, Va., the reported direction of the storm-wind and cloud, are the same as at Scuppernong, with rain from S. W. at 9 P. M., with thunder and lightning.

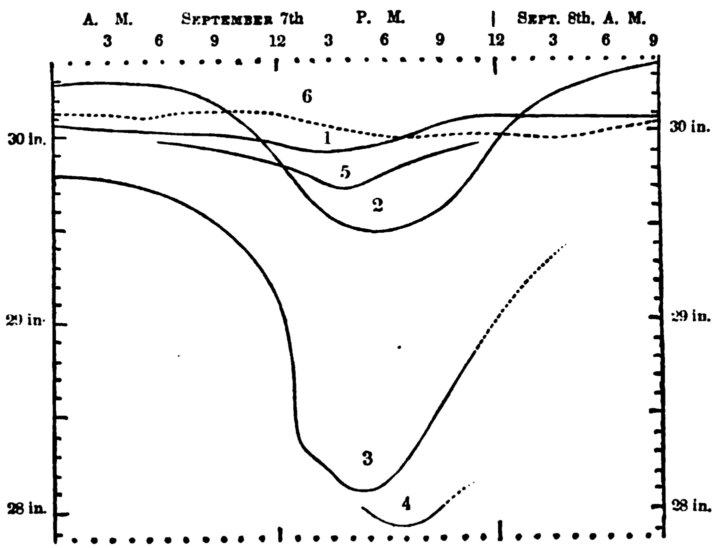
At Savannah, on the 5th and 6th, in front of the storm, the maximum of the barometer was 30·20, and 30·19; on the 7th, the minimum of the report is 30·06, and on the 8th, under the rear portion of its annular wave, 30·21. At Jacksonville, East

above is apparently required. The normal course of the circulation, or of the "current of rotation" in the basin of the North Atlantic, between latitudes 10° and 50°, as well as the routes taken by some storms which have recurvated in low latitudes, clearly indicate that a part of the out-moving atmosphere, from the inter-tropical latitudes, in the western Atlantic and the Mexican Gulf, moves in an elliptical circuit, and returns to the trade wind in the eastern Atlantic. This apparent tendency in storm-routes is seen in Col. Reid's track of the great hurricane of Barbadoes in 1780, and in my track XV, on the present Chart; also in track XIII of my former Chart, which is better seen on Chart III in vol. i of this Journal, New Series.

Florida, nearly the same: as also at Charleston, S. C., nearer to the path of the storm; and scarcely falling below 30 inches at New York and Nantucket, as the storm passed. But at Camden, S. C., 140 miles N. N. W. of Charleston and 280 miles W. S. W. of Hatteras, the successive maxima on the 6th and 8th were only 30·04, and 30 inches; showing this place to have been beyond the crest of the external barometric wave. When storm-tracks recurvate on the interior meridians of the United States, the minimum depression of the barometer frequently moves nearly parallel to the direction from Camden either to Hatteras or to Chesapeak Bay.*

The annexed diagram shows the barometric curve at Washington, Fort Monroe, steamer Georgia, and Bermuda, while the storm was passing between the latter and Washington. These two places are distant from each other about 840 miles; which perhaps may be considered as an approximate measure of the *barometric* diameter of the storm on the 7th of September. The barometric curve of the *Georgia*, if increased so as to reach the minimum of the *Swan*, may represent a section through the center of the cyclone, in the direction of the storm's progression.

Storm-Curves of barometric progression.



1. Curve at Washington: 2. at Fort Monroe: 3. Steamer Georgia: 4. Brig Swan: 5. Ship Eagle, crossing in front of storm: 6. Bermuda.

* I am indebted, on this occasion, to the officers of the Smithsonian Institution, and to Gen. Lawson, chief of the medical bureau at Washington, for observations from various parts of the United States; also to Lieut. Maury, Supt. of the Naval Observatory, for abstracts of the logbooks of Ship Eagle, and steamer Northern Light. Surgeons Williamson and Harrison of the Navy, and many ship-masters, and others, have kindly aided my inquiries.

It will be seen that the above diagram includes a period of thirty-three hours; and if we rate the progression at twenty-five miles an hour, it will comprise a distance of 825 miles. It contains the curve derived from observations on board the ship Eagle. I add the following condensed statement which is derived from the abstract of the ship's log sent me by Lieut. Maury.

The clipper ship *Eagle*, Warren, from Rio, crossed the center-path on the morning of Sept. 7th, perhaps 350 miles in front of the axis of the gale, while running in the direction of Cape May. It is interesting to find that this vessel, which crossed the equator Aug. 17th, was overtaken by the external barometric wave of the storm as early as 4th–5th of Sept., and by *a long swell coming up from S. E.;* being then from 100 miles to 60 miles southward of Bermuda. Through the 6th, winds from southeast quarter, veering to South, with *a heavy swell from S. S. W.;* latitude at noon 34° 39′, lon. 69° 32′; bar. 30·11. Sept. 7th, *a very heavy swell from S. S. W.;* wind fresh, from S. S. E. to S. S. W.; and from S. S. W. back to E. N. E.—bar. at 8 A. M. 29·90, at noon, 29·84 in. lat. 37° 17′, lon. 72° 28′: P. M. very threatening appearances from S. E. by S. to S. W., with *a very heavy swell from S. W.;* at 4 P. M. bar. 29·70,—at 5 P. M., 29·77; wind fresh from E. N. E. to N.; 8 P. M., lightning at N. W.; at 11 P. M., in a heavy squall, wind shifted to N. N. W.; no rain; heavy sea still.——Sept. 8th, cloudy; no sea; lat. at noon, 38° 38′, lon. 74° 13′; bar. 30 inches.

The steamer *Northern Light*, bound for the Isthmus, was several hours ahead of the *Georgia*, and on a more eastern track. She crossed the center-path in front of the gale, and ran through its eastern side.——Sept. 7th, lat. 34° 30′, lon. 73° 25′; through the day, strong gales from the South; with a heavy sea from S.W.——Sept. 8th, lat. 32° 01′, lon. 73°, strong gales from S.W., with heavy squalls, and *a large sea from W. N. W.:* Clear in the S. E., with stormy appearances in the N. and W.:——found the weather improving as we made south.——This account is probably in nautical time.

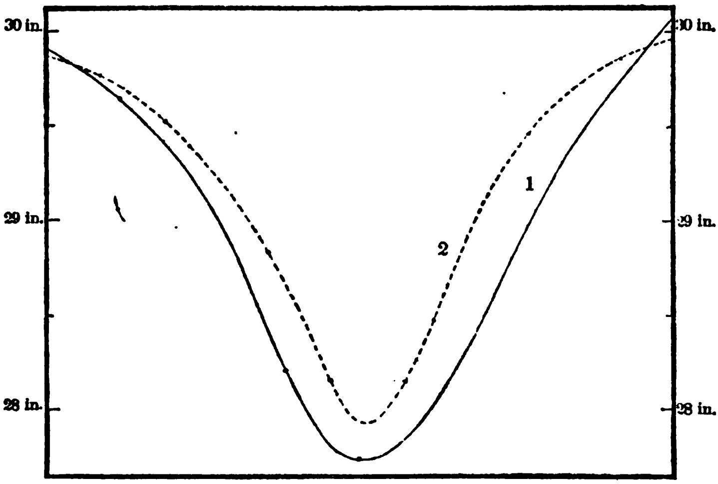
The succeeding diagram represents, in its horizontal scale, the distance of 840 miles between Washington and Bermuda. The full line (1) represents, approximately, the barometric curve through the center of the storm, *transversely to its path.* The comparison of this transverse curve with the central curve of *progression*, indicated on p. 18, is of some interest; although we have no observations intermediate to the Swan and Bermuda. The resemblance of the two central cross-curves may show that the storm was of nearly equal extent and force on all of its sides, at that time.

I have been apprehensive of a clerical error in the barometric report from Fort Monroe for 3 P. M. and 9 P. M. of Sept. 7th; and that 29·063 and 29·087, should have read 29·63 and 29·87, respectively. In preparing the second diagram I became convinced

that the correction is required; and have accordingly applied it, in tracing the transverse curve: but have drawn a short trace line to show the observations as found in the report.

I have also inserted in this diagram, in broken lines, the transverse barometric curve through the center of the Cuba hurricane of October, 1844; when in nearly the same geographical position. This curve is approximated from twenty-eight observations in the path of that storm.

Barometric Storm-Curves, transverse to the Progression.



1. Transverse centre-curve of Cape Verde and Hatteras hurricane, Sept. 7, 1853.
2. Transverse center-curve of Cuba hurricane of 1844, Oct. 6th.

Vortical Rotation of the Gale.—The true character of this gale as a cyclone, is made evident by the foregoing series of observations. This is most extensively shown by the various observations made on all sides of the storm during its passage between Bermuda and the nearer portions of the United States. The absolute whirlwind movement of the storm stratum, and the increasing rapidity of its leftwise rotation ↺ which is found in approaching the axial area, from whatever side of the storm, as well as the direct effects of this increased rotation on the fall of the barometer, in the interior portions of the gale, are made manifest by direct observation. This I might point out in full detail, were it at all necessary in the present stage of the inquiry. Nor can these results be evaded by denominating any one portion of the cyclonic wind, on either side of the cyclone, as another or distinct gale. The local variations and inequalities of the cyclonic action and the errors, imperfections or defects which may exist in the reports, are alike overborne by the amount of evidence

which serves to show the extent and general entireness of the vortical rotation in the gale.*

It would be an error to suppose that the gales and hurricanes which have been traced on our storm charts, were but exceptional cases of cyclonic action and progression in the winds of our globe. For there is a constant succession of rotary movements, greatly variant in their activity and their visible effects, and to which I shall further allude. It is the more violent cyclones, however, that afford us complete evidence of their geographic routes and their continued movement of rotation.

Of this active class, designated as hurricanes, gales, and storms, it is believed that the tracks or routes of several hundred might be added to our storm maps, by carefully collating the records which already exist. It is certain that a large number might be traced from the records and notices now in my possession or otherwise at hand; of which, the case I have now presented is but a single example. But the storms noticed in the succeeding portions of this article are selected in reference to their peculiar localities, as showing the uniform extension of the cyclonic system in equal latitudes around the globe, rather than for the amount of information possessed, regarding their extent and progression.

(To be continued.)

ART. II.—*Account of a Rainbow caused by Light reflected from Water;* by Prof. E. S. SNELL, of Amherst College.

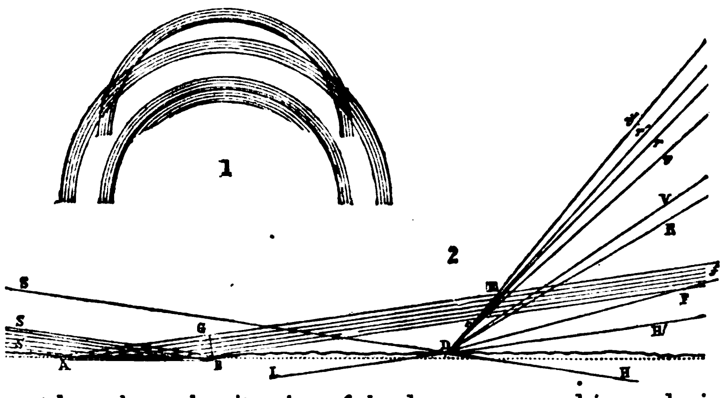
I HAVE received from my friend and former pupil, Mr. H. M. Adams, of the Theological Seminary in East Windsor, Conn., a very interesting description of a brilliant rainbow scene, witnessed by himself and others, on the 24th of Sept. last. After a slight shower, the sun shone out, about 5 P. M., and produced the usual primary and secondary bows, except that they were of uncommon brightness. Four or five supernumeraries, exceedingly vivid and beautiful, underlined the upper part of the primary, the usual attendant of a very brilliant rainbow. In addition to these, there was seen an excentric bow, quite as luminous as the secondary, but in angular size and order of colors, just like the primary, and

* While the printing of these pages was in progress I received from the government of Denmark, through Consul Bech, observations made at Oefjord, on Skage Strands Bay in the north of Iceland, in lat. 65° 40′ N., lon. 20° 40′ W.; which show the maximum pressure in advance of the cyclone to have been 29·75 in., at 1 P. M. Sept. 10th, under an east wind, the force of which is marked 2. The fall of the barometer under the cyclone continued till the night of 12th; the lowest observations being 29·12 in. at 10 P. M. of 12th, and 29·11 in. at 6 A. M. of 13th. The wind was S. E. on the 11th and 12th, and, on the rising of the barometer, was followed by the N. W. wind of the cyclone on the 13th, which afterwards changed to S. W., its force being marked 1. The normal effect of the cyclone at Oefjord, a position which is remarkably sheltered from the force of the cyclonic winds, by the peculiar outline and the extensive elevations of Iceland, is deserving of notice; not taking into account the modified direction and the abatement of force which appears to occur in the left quadrants of the gales in this highly northern portion of the Atlantic.

vertex 4° above it. Its extremities came within 15° of the horizon, and if prolonged a little, would have intersected the primary itself. This gorgeous display lasted some ten minutes, when the third bow began to fade at the top, and soon wholly disappeared. (See Fig. 1.)

Mr. Adams rightly judged that this additional bow was the effect of the sun's rays, reflected from the Connecticut river, which runs on the west side of the Seminary hill, and whose waters were then very placid. It was in fact a primary bow produced by an image of the sun in the river. As the vertex of the reflected bow was 4° above that of the secondary, or 16° above that of the primary, the image of the sun must have been 16° below the sun itself, and the sun therefore 8° above the horizon. In first considering the case, I found it difficult to conceive, that the stratum of light, reflected so obliquely from a narrow belt of water, could have sufficient thickness to form a bow occupying between 160° and 170° of a circle.

The accompanying vertical section through the axes of the bows, exhibits in their true proportions, the breadth of the river, the distance and elevation of the observer, and the thickness of the reflected sheet of light; and shows pretty accurately what



must have been the situation of the drops concerned in producing the upper and lower parts of the bow. (See fig. 2.) The breadth of the river, AB, is a little more than half a mile. The observer at D, is elevated 100 feet above the level of A B, and his distance from B, the nearest bank of the river, is three-fourths of a mile. S D H is the axis of the direct bows, inclined 8° to the horizon. The axis of the reflected bow, I D H′, intersects S D H at D, making with it an angle of 16°. A B E F is the reflected sheet of light which penetrates the shower and forms the image-bow. Its thickness in the section G B, measures about 370 feet. R D, V D, making with D H the angles 40° 17′ and 42° 2′, are the red and violet rays from the summit of the primary bow; *v* D, so much elevated, as to cross the secondary, and extend at the

r D, in like manner, mark the top of the secondary, making with D H respectively the angles 50° 59′, and 54° 9′; while r' D, v' D, are drawn so as to make the angles 40° 17′ and 42° 2′ with the axis of reflected light, I D H′. These last rays must come from drops occupying the space E e. D F is next drawn, at an inclination of 15° with the horizon, that being the estimated height of the extremities of the image-bow. This line, piercing the luminous stratum in F f, indicates the position of the drops which produced the lower portions of the bow. D E, representing the distance of the remotest drops, which could reflect the summit-rays to the eye, is about one-half of A B, or one-fourth of a mile; and these drops, if falling perpendicularly, would reach the ground within 900 feet of the observer. But the lowest rays, vertically projected in F D, must come from drops, whose least distance from the eye, is $\frac{\text{D F} \cos 7°}{\cos 42° 2'}$; since F D is 7° above the axis D H′. Now D F in the section is about equal to D B, or three-fourths of a mile. Hence the ray, whose vertical projection is D F, is $\frac{3}{4}$ m. $\times \cos 7° \div \cos 42° 2' =$ one mile in length, very nearly. The lines, B G, D E, and D F, may be readily calculated, and will be found to accord nearly with the above values. It appears, then, that the drops forming the top of the bow, cannot fall at a *greater* distance than 900 feet, while those forming the lower ends, cannot fall *nearer* than 5200 feet.

How can we account for what at first view seems to be true, that the light already somewhat enfeebled by reflection from the river, should be able to penetrate more than 4000 feet into the shower, aud then return through the same 4000 feet of rain, and yet reach the eye in sufficient quantities to exhibit brilliant colors? I apprehend that this part of the phenomenon can be explained only on the supposition that several favorable circumstances conspired to produce a remarkable result.

1. The air was undoubtedly so clear that the sun shone with intense brightness. The extraordinary brilliancy of the bows, and the number and vividness of the supernumeraries, are a sufficient proof of this.

2. The shower was probably not very dense; so that the rays could penetrate into it much farther than usual, and return again to the eye.

3. A more important favoring circumstance than any other, perhaps, would be a convexity toward the observer of the nearest outline of the shower; so that, while rain was falling within 900 feet of him in a direction precisely opposite to the sun, and thus near enough to form the top of the bow, the nearest rain, on the right and left, where the extremities were seen, might be 5000 or 6000 feet distant. If the light was intense, and the drops sparse, then a much less degree of curvature might be attended with the same result. In the present instance, there can be little doubt,

I think, that the western limit of the shower was more or less convex towards the point of observation.

4. If the river bends eastward at all on the North and South of the observer's position, this circumstance would virtually add so much to its width, since the rays forming the branches of the bow would then fall lower than B F in the vertical projection. Whether this is so, I am not informed.

It is obvious from an inspection of the figure, that most of the reflected bow would disappear sooner than the direct bows, inasmuch as the angular thickness of the luminous stratum, at its intersection with the shower, would rapidly diminish as the shower retired. And, furthermore, as the vertex was produced by the nearest drops, this part must have vanished first, as was observed to be the fact.

Had the sun been about 6° above the horizon, the vertex of the image-bow would have coincided with that of the secondary; and by its opposite arrangement of colors, would have partially neutralized its tints, and made a white segment common to the two. It was in this aspect that the direct and reflected bows presented themselves to the view of Dr. Halley, in 1698, on the bank of the river Dee, of which he published an account in the Transactions of the Royal Society.

To the inquiry, why do not those, who live near a sheet of water, more frequently witness the reflected bow, it may be replied, that, if the water is not of greater width than a fraction of a mile, the favorable circumstances already enumerated will very rarely concur to produce the phenomenon with much distinctness; and if there are several miles of water, so as to reflect a beam of large vertical thickness, yet the surface would very rarely be smooth enough, directly after a shower, to form a single and well-defined image of the sun. And it may be added, that there are few persons, except such as have made the theory of the rainbow a matter of careful study, who would consider a bow as particularly noticeable and worthy of description, simply because it happened to intersect the others, especially if, as must ordinarily be the case, the intersecting bow was only a short and indistinct arch.

ART. III.—*On Changes of the Sea-Level effected by existing Physical Causes during stated periods of time;* by ALFRED TYLOR, F.G.S.*

Introduction.

THE First Part of the ensuing paper is occupied with the details of the probable amount of the solid matter annually brought into the ocean by rivers and other agents, in suspension and solution; and the conclusion is arrived at, that the quantity of detritus thus distributed on the sea-bottom would displace enough

* From the Philosophical Magazine for April, 1853.

water to cause an elevation of the ocean-level to the extent of at least 3 inches in 10,000 years.

In the Second Part an endeavor is made to compute the number of such periods of 10,000 years that must have elapsed during the accumulation of the immense mass of recent freshwater strata said to exist in the valley of the Mississippi.

The calculation as to the latter is made from the *data* collected by observers in America, of the extent of the deposit in question; and it is here supposed, first, that in former periods the *same* quantity of mud as at present has been annually carried into the Gulf of Mexico; and secondly, that the amount of sediment deposited on the delta and plains of the Mississippi does not exceed *one-tenth part* of the solid material which has been carried out (suspended in the water of the river) into distant parts of the Gulf of Mexico, or into the Atlantic Ocean itself.

From recent accounts by Mr. C. Ellet, of the United States, it appears that a column of fresh water, 1¼ mile wide and about 7 feet deep, is constantly entering the Gulf of Mexico at a speed of 2 to 2½ miles per hour, and floats on the surface of a stratum of salt water, to which it partially communicates its own velocity. And below this a stratnm of sea-water is found to be flowing in an opposite direction to that of the two strata of fresh and salt water above it. See figs. 1 and 2.

From the data submitted, it would appear that the accumulation of the alluvial deposit of the Mississippi must have occupied a great number of periods, during each of which an elevation of the sea-level of 3 inches may have occurred.

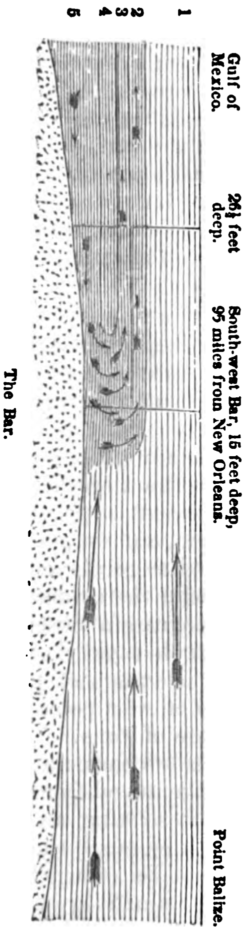
The general conclusion arrived at is, that the sea-level cannot be considered as stationary for practical geological purposes, since the operation of present physical causes would produce a considerable change in its height, even during the construction of a recent deposit like that in the valley of the Mississippi, which may be called small and local compared with those older formations familiar to geological observers.

But the subsidence and elevation of the crust of the earth would be accompanied by alterations of the area of the sea-bed; and the frequency of such movements would therefore furnish additional reasons for not considering the sea-level permanent for the lengthened periods requisite for the accumulation of sedimentary deposits of any magnitude.

In the Third Part of this paper an attempt is made to direct attention to the difficulty of finding any test by which to distinguish strata gradually accumulated during a long-continued upward movement of the sea-level, from those strata formed on a sea-bottom slowly subsiding while the ocean-level was stationary. In either case no change of depth of water may have occurred of sufficient importance to cause the removal of the Mol-

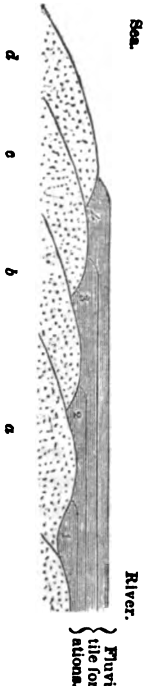
lusca inhabiting the locality, and therefore the discovery of *the same species of organic remains from top to bottom of a thick deposit* is not an absolute proof (as has been supposed*) that

Fig. 1.—*Section of the Bar of the Mississippi.*



1. Stratum of fresh water, 7 feet deep, running per hour from 2 to $2\frac{4}{10}$ miles, and passing over the Bar without losing its velocity.
2. Stratum of salt water, 1 foot deep, running at the same speed as No. 1.
3. Ditto, running at 1 mile per hour.
4. Ditto, supposed to be stationary.
5. Ditto, flowing towards the Bar.

Fig. 2.—*Theoretical Diagram of four successive Bars, with their accompanying fluviatile deposits, showing a section of the junction of a series of Fluviatile and Marine Strata formed during either subsidence of the land or elevation of the sea-level.*



a, b, c, d, 1st, 2nd, 3d, and 4th Bars of marine formation. 1, 2, 3, 4, 1st, 2nd, 3d, and 4th fluviatile deposits.

The marine stratum *a* is contemporaneous with the fluviatile stratum 1; *b* with 2; *c* with 3; and *d* with 4. This holds good whether the land has subsided or the sea-level risen.

* "In formations from a few hundred to a thousand feet and upwards in thickness, the whole of which does actually belong to the same geological age, and is therefore characterized by the same fossils, most curious and important results may be sometimes deduced if the position or relative heights at which the groups of fossils are imbedded be noted; and this is a point usually neglected. For, thanks to the researches of Prof. E. Forbes, the depth of water under which a collection of shells lived can now be approximately told; and thus the movement of the crust of the earth, whilst the strata including the shells were accumulating, can be inferred.

"For instance, if at the bottom of a cliff, say 800 feet in height, a set of shells are buried which must have lived under water only 50 or 100 feet in depth, it is clear that the bottom of the sea must have sunk to have allowed of the deposition of the 700 feet of superincumbent submarine strata; subsequently the whole 800 feet must have been upraised." (Darwin.)

gradual subsidence has occurred during that particular formation; because the condition of equal depth of water during any deposit might be produced either by subsidence of the sea-bottom or elevation of the sea-level, or by both conjointly.

In discussing these questions, the writer has not assumed that during gradual subsidences or gradual elevations, greater denudations or depositions would occur than when the level of the land and sea-bottom was stationary; because it is not certain, either that during such gentle oscillations the forces that would produce denudation are sensibly diminished or increased, or that the rocks which are brought within the reach of denuding forces are necessarily more easily worn away than those which were previously exposed to the same influences.

Part I.

It has long been acknowledged that the quantity of detritus annually carried into the ocean from various sources must displace an equal volume of water, and thus tend to raise the level of the sea. Many years since it was estimated by an Italian that this change might amount to one foot in a thousand years. The general opinion on this subject has been, that the effects produced by the present supplies of detritus would be too minute to be perceptible, and on geological inquiries the ocean-level has been considered as permanent for all practical purposes.* I here propose to offer the evidence of present denudation in certain countries where careful observations have been made, in order to show, that, if such rapid destruction of land occurs in most localities, then the operation of present physical causes must be amply sufficient to effect a perceptible alteration in the sea-level in a moderate space of time.

The mere consideration of the number of cubic feet of detritus annually removed from any tract of land by its rivers, does not produce so striking an impression upon the mind as the statement of how much the *mean surface level* of the district in question would be reduced by such a removal. This information may be obtained by calculation from the published accounts of the quantity of mud annually abstracted from districts of known dimensions by their rivers. In this manner it is found that the Ganges would in about 1751 years, at its present annual rate, carry away from the land it drains (which is supposed to be about 400,000 square miles) as much detritus as would cover that area to the depth of one foot, as the following calculation will show:

Thus, 27,870,400 (superficial feet in a mile) $\times$ 400,000 = 11,151,360,000,000, the number of superficial feet in the area of 400,000 square miles drained by the Ganges. The number

* Manfredi. See Lyell's Principles, edit. 1850, p. 270 and 542.

of cubic feet of detritus discharged annually by that river is 6,368,677,400. (See Lyell's Principles.)

$\frac{6,368,677,400}{11,151,360,000,000} = \frac{1}{1751}$; consequently the reduction of the mean level of the Ganges district is $\frac{1}{1751}$ of a foot annually, or 1 foot in 1751 years.

6,368,677,440 cubic feet of mud discharged $\times$ 856 water to mud = 5,444,074,288,640 = the number of cubic feet of water annually discharged by the Ganges.

$\frac{5,444,074,288,640}{11,151,360,000,000}$ = about $\frac{1}{2}$ a foot, so that the mean annual discharge of water is equal to about 6 inches of rain on the whole area of 400,000 square miles.

The Mississippi, on the other hand, would occupy 9000 years at its present annual rate in reducing to the amount of one foot the mean surface-level of the district it drains, which is computed at eleven hundred thousand square miles. The result is obtained as follows:

If 3,702,758,400 cubic feet of mud are annually carried down by the Mississippi (since the mud is to the water as 1 to 3000), 3,702,758,400 $\times$ 3000 = 11,108,275,200,000 = the number of cubic feet of water annually carried by the river into the Gulf of Mexico. The area of district drained by this river is stated at 1,100,000 square miles — 5280 $\times$ 5280 = 27,878,400 = the number of superficial feet in a mile—27,878,400 $\times$ 1,100,000 = 30,666,240,000,000 = the number of superficial feet contained in the area of 1,100,000 square miles drained solely by the Mississippi.

$\frac{11,108,275,200,000}{30,666,240,000,000}$ foot = $\frac{1}{3}$ foot nearly. Consequently the water carried down by the river is equal to about 4 inches of rain over the surface of land drained.

If it be assumed that the levels of the rivers, lakes, and springs are the same in this district at the same period of two consecutive years, the water sufficient to produce the above-mentioned 4 inches of the total of rain-fall upon the whole of this district must have been annually derived from clouds which have been charged with vapor in parts of the earth beyond the confines of the tract of country under consideration; since, if the 4 inches of rain annually carried into the Gulf of Mexico were not replaced from foreign sources, the levels of the rivers, lakes, and springs must rapidly fall.

The estimate of denudation obtained from these countries may be incorrect when applied to other lands differing in altitude and receipt of rain. Besides, many rivers empty themselves into lakes and inland seas, and other extensive tracts are entirely with-

out rain. Since there must be extensive districts which contribute no detritus whatever to rivers, I propose to *assume* that one half the earth's surface only is drained by rivers flowing directly into the sea,* and that the average supply of detritus does not exceed that afforded by the district through which the Mississippi flows (a country where there are no very high mountains, and only a moderate quantity of rain).

The quantity of soluble salts annually carried into the ocean must amount to a very large volume, particularly as river-water always contains matter in solution, while it is only during two or three months of the year that alluvium in suspension is carried down in large quantities. The proportion of soluble salts in the water of the Thames is 17 to 70,000, or 1 to 4117; while the proportion of alluvium suspended in the water of the Mississippi is as 1 to 3000.†

The level of the land is as much reduced by what is carried away in solution, as if this were mud and sand removed in suspension; and a submarine deposit formed from materials brought into the sea in solution will displace a volume of water equal to their former bulk; and therefore, when the annual supply of soluble salts to the ocean does not exceed the quantity separated from solution, the same effect will be produced upon the sea-level by matter introduced, whether it be in solution or suspension. While the proportion of the land to the ocean remains as 1 to 3,‡ it is evident that a reduction of 3 feet in the mean surface-level of the land must take place by denudation before a volume of detritus would be conveyed into the sea sufficient to displace enough water to occasion an elevation of one foot on the ocean-level. (See fig. 3.)

There is great need of further information respecting the amount of sediment carried down by other rivers besides those mentioned; yet if the rate of denudation obtained from the statistics of the Ganges and Mississippi be any guide to what is occurring on the remainder of the globe, we cannot suppose that an *indefinite* time would be required for the performance of a denudation, which should reduce the mean surface-level of the land 3 feet and raise that of the ocean 1 foot. It was during the contemplation of the changes of level that might have been produced by the operations of ordinary physical agents upon the surface of the earth, that Hutton was led to remark that it was not necessary to suppose the area of the land always maintained the

* By reference to Johnston's Physical Atlas, the calculated proportion of land drained by rivers running into European lakes and inland seas may be seen.

† For the statistics of the Mississippi River, see Sir Charles Lyell's Second Visit to the United States, edit. 1847, vol. ii, p. 249 to 253, and other places.

‡ M. Balbi shows (Atlas, Soc. Diff. Useful Knowledge, 1844) that the land on the globe equals 37,647,000 square geographical miles, the sea equals 110,875,000 square geographical miles.

same extent, but that from time to time new land would be formed by the elevatory movements of the sea-bottom to compensate for what had been carried into the ocean by the continued operations of rivers and breakers.* In speaking of the elevation of the sea-level, I only refer to the intervals between those movements of the land which might neutralize in an instant all that had been effected by the operation of rivers for immense periods of time.

It would add very much to the interest of this inquiry if any proof could be brought forward of a recent gradual upward movement of the sea-level. This would, however, be difficult to observe,† on account of the rise in the water concealing the evidence of its former level, except just at the mouths of rivers, where the deposits of fluviatile alluvium might raise the land from time to time and keep it always above the rising waters.

The deposits situated at a few such localities have been described by the best observers, and I hope to show that in several cases there are appearances which might be partly explained by changes of the sea-level, but that a much greater number of cases and more certain evidence would be needed before such an event could be satisfactorily proved. I propose to make some remarks upon this point, after having submitted the evidence which has induced me to believe that the supply of detritus under present

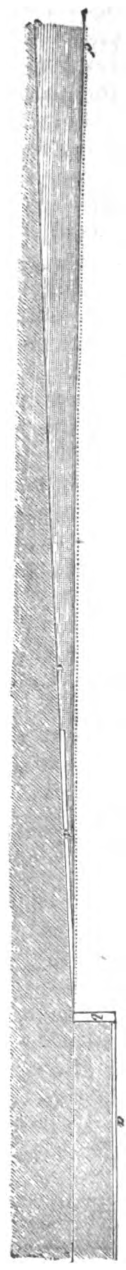


Fig. 8.—*Diagram intended to express the effect upon the sea-level from a waste of $\frac{1}{3000}$dth of a foot per annum (a), over all land (from atmospheric and aqueous action), accompanied by a general waste of 136 feet of detritus from each foot of frontage (b) of coast-line (caused by the action of the sea), supposing there is one mile of coast-line to each 100 square miles of land, and to each 300 square miles of water.*

* "It is not necessary that the present land should be worn away and wasted exactly in proportion as new land shall appear; or conversely, that an equal proportion of new land should be produced as the old is made to disappear." (Hutton's Theory of the Earth, 1795, vol. i, p. 196.)

† See Darwin, Coral Reefs, &c., edit. 1851, p. 95.

physical conditions is sufficient to raise the ocean level 3 or 4 inches in 10,000 years, provided no subsidence or elevation disturbed the result.

To this subject I now proceed. Sir Charles Lyell's published statements of the quantity of mud annually carried down by the Mississippi and Ganges appear to have been made with so much care, that they may be a better guide to the general rate of removal of soil by rivers than information obtained from a greater number of smaller rivers, which of course are more likely to be influenced by local circumstances. Eleven hundred thousand square miles of land are drained by the Mississippi,* which annually discharges a quantity of water equal in volume to 4 inches of rain, or about one-tenth of the total rain-fall over this entire surface, which forms one-fifth part of North America.† From the mean of a great number of observations, the average quantity of alluvium suspended in the water appears to be 1 part in 3000. Consequently, as the water annually drawn off would cover an area of eleven hundred thousand square miles to the depth of four inches, the quantity of mud removed in the water (as measured at or near the mouth of the river) would cover the same extensive surface to the depth of $\frac{1}{3000}$dth part of four inches, or to the depth of $\frac{1}{9000}$dth part of a foot. Or, in other words, the Mississippi at its present rate would occupy 9000 years in carrying away detritus before the mean surface level of one-fifth part of North America would be reduced one foot.

The Ganges discharges into the Indian Ocean a supply of water equal to about six inches of rain on 400,000 square miles, or a much greater volume of water than the Mississippi pours into the Gulf of Mexico, taking into consideration the difference in size of the countries they drain.

The alluvium suspended in the waters of the Ganges‡ is as 1 to 858 by weight; consequently the detrital matter removed in suspension by the water in one year would cover the land from which it is derived to the depth of $\frac{1}{1751}$ of a foot; that is to say, the Ganges might pour out muddy water at its present rate for 1751 years before the mean level of 400,000 square miles would be reduced one foot in height. The great elevation of the Himalaya range, or possibly a greater rain-fall, may probably occasion the difference between the rates of denudation indicated by the Ganges and the Mississippi. As there are also parts of the earth's surface drained by rivers flowing into lakes and inland seas, and other tracts are entirely without rain, I propose to estimate (as before mentioned) that only half the land contributes detritus in

* See art. Mississippi, Penny Cyclopædia, vol. xxv, p. 277.

† The total rain-fall of the United States is 39 inches between 24½° and 45° N. lat. (Berghaus and Johnston.)

‡ See p. 6.

suspension to rivers flowing directly into the sea.* If this area be annually reduced in level at the same rate as the district through which the Mississippi flows, then the mean level of the land on the globe would be reduced 3 feet in 54,000 years, and consequently the level of the ocean raised 1 foot in the same period by means of the detritus suspended in river-water poured into the ocean.†

But in addition to the sediment carried down by means of rivers, we have also to take into consideration the amount of debris washed into the sea from cliffs during so long a period as that mentioned. It is difficult, however, to form any estimate of what this would annually amount to, for old maps and charts are hardly accurate enough to represent the waste of cliffs by breaker-action even within the last 100 years. Capt. Washington has, however, published a report‡ which gives an account of the encroachment of the sea at intervals on one part of the Suffolk coast. This will give a general idea of the contribution of detritus that may be obtained from some points of a coast-line. The following statements are collected from Capt. Washington's Report on Harwich Harbor in 1844, from which also the figures 4, 5, 6, 7, are copied.

The cliff on the western side of the harbor is about 1 mile long and 40 feet high, and the encroachment of the sea appears to have been at the rate of 1 foot per annum between the years 1709 and 1756, so that the annual supply of detritus was equal to 40 cubic feet for each foot of frontage. Between 1756 and 1804 the advance increased to nearly 2 feet per annum; so that the annual removal of cliff amounted to nearly 80 cubic feet for each foot of frontage.

Between 1804 and 1844 the encroachment of the sea averaged 10 feet per annum, and the annual removal of detritus must have amounted to 400 cubic feet for each foot of frontage. It was during this latter period that extensive dredging for cement stone took place at the base of the cliff.

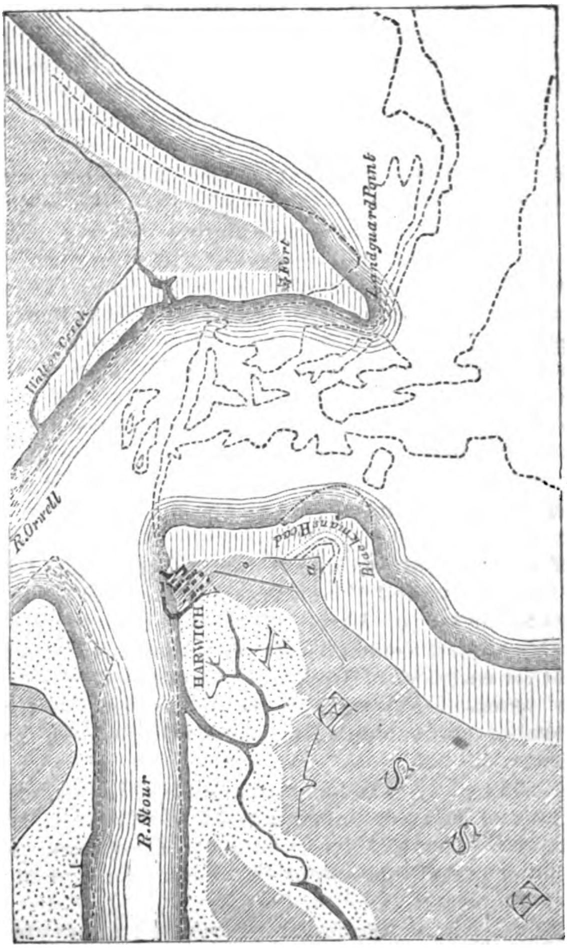
On the eastern side of the harbor events of an opposite character have occurred, for Landguard Point has gained 50 feet per annum in length during the last 30 years. The addition thus made to the land, and to the "littoral zone," presents an interesting example of the rapid accumulation of a local deposit under favorable circumstances. From the appearance of the beach, it would appear that the shingle and sand of which it is formed

* The proportion of land without rain is about $\frac{1}{1200}$dth of the whole. Keith and Johnston say that nearly one-half the drainage-water of Europe and Asia falls into the Black and Caspian Seas. The proportion for Africa and America is not known.

† It is not improbable that the *solvent* powers of rain and river-water are as important agents in the removal of land as the agency above mentioned. Definite calculations on this subject remain to be made.

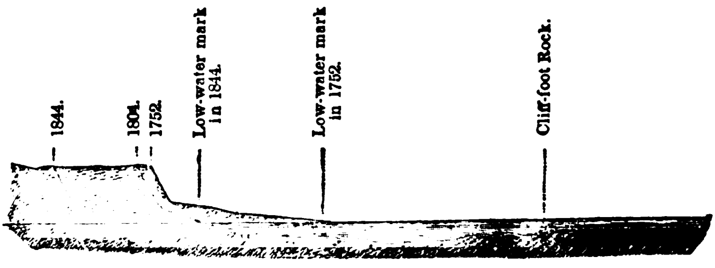
‡ Tidal Harbors' Commission, First Report of 1845.

Fig. 4.—*Map of Harwich.*



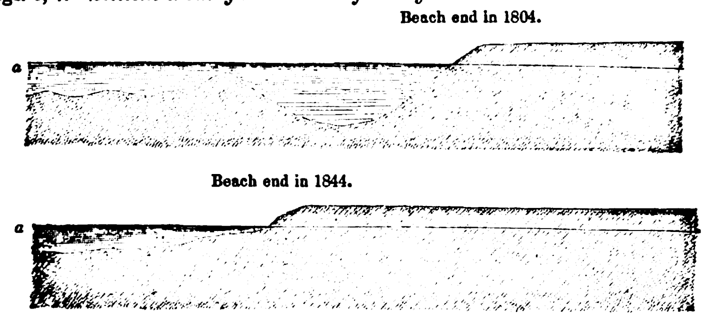
a. Beacon Cliff, with dotted lines indicating the changes that have taken place in high and low-water mark. The dotted line shows the former outline of Languard Point.

Fig. 5.—*Section showing the Destruction of Beacon Cliff between* 1752, 1804, *and* 1844.



have been brought from the north, in which direction there are recorded instances of great destruction of land by storms during the last 300 years. The aspect, however, of much of the coast-line appears as if it had remained unaltered for a very long period, except in the manner Mr. R. A. C. Austen* alludes to when he remarks, "that although the sea for months together, and in places even for whole years, may not acquire any fresh spoil, yet there are few hours when its waters are unemployed in fashioning and abraiding the materials already acquired." In considering the effect upon the sea-level caused by sand, mud, and pebbles washed in by the breakers, it is only necessary to regard those materials that may be brought in from cliffs above high-water mark; for the movement of sand and mud below high-water mark can produce no effect upon the sea-level, because the abstraction of these materials from one part of the shore is exactly balanced by their addition to some other part. For instance, some of the flint-pebbles which have contributed to the recent deposit at Landguard Point have been brought along shore a great distance from their original position on the cliff. These flints formed an addition to the sea-bed, and tended to raise its general level by displacing an amount of water equal to their bulk the moment they fell on the shore below high-water mark; and it is quite clear their subsequent movements, either beneath the wave or on the beach, could produce no further effect upon the sea-level, the spaces they occupied on one part of the coast being balanced by the vacancy left at some other. It is also evident that the beach at Languard Point will go on extending so long as the fresh supplies of shingle and sand from the north exceed the removals southward.

Figs. 6, 7.—*Sections showing the Increase of Landguard Point between* 1804 *and* 1844



a. a. Low-water level of ordinary springs.

In the same manner the continued supplies of pebbles from the westward enables the Chesil Bank to preserve its position. As

* Austen, Quart. Jour. Geol. Soc. vol. vi. p. 71–73; and De la Beche, Geol. Observer, 1851, p. 65.

soon, however, as any disturbing causes interrupt the supplies of new material, the sand and shingle beaches dependent upon them must soon disappear; and in fact the termination of every beach will be at that point where the waste and abrasion by breaker-action are balanced by the supply of pebbles and sand drifted from other places. Although it appears clear that only the detritus obtained from cliffs above high-water mark need be taken into calculation, yet I regret to find that scarcely any data of this kind exist, and therefore it is not possible to ascertain the probable effect upon the sea-level that is being produced by the detritus so derived. In the same manner the per-centage of soluble salts in the water of the few large rivers of which notes have been published has not been given separately from the per-centage of matter in suspension, and therefore we are in ignorance of the supplies that are annually introduced into the ocean from the formation of submarine deposits from materials dissolved in the sea-water. When the rise in the sea-level from the effect of alluvium brought in suspension by rivers was being considered, I supposed that that cause alone might produce an elevation of one foot in 54,000 years; but in order to make some allowance for the similar effects that must be produced by the introduction into the ocean of materials from above high-water mark on coast-lines* by breaker-action, and also by the formation of submarine deposits from materials which were brought into the ocean in solution, I now propose to consider that all these causes together might produce an elevation of the sea-level equal to one foot in 40,000 years, or three inches in 10,000 years.

Mr. Darwin has remarked, that "the knowledge of any result, which, with sufficient time allowed, can be produced by causes, though appearing infinitely improbable, is valuable to the geologist, for he by his creed deals with centuries and thousands of years as others do with minutes." For these reasons, even if, upon further investigation, it should be found that the true rise in the sea-level is much less than three inches in 10,000 years (in periods undisturbed by subsidences and elevation), yet it may still be an important element in accounting for those changes which we are now about to consider.

(To be continued.)

* The rough estimation of the extent of coast-line, kindly supplied by Mr. A. K. Johnston, (Nov. 1852), is as follows:—

	Nautical miles (60 to a degree.)	English statute miles (69½ to a degree.)
Europe, - - -	17,200	20,425
Asia, - - - -	30,800	34,825
Africa, - - -	14,000	16,625
America, - - -	37,600	44,656
	99,600	116,531

Art. IV.—*On the Phosphate of Iron and Manganese from Norwich, Mass*; by Dr. J. W. Mallet.

This mineral, first observed by Dr. E. Hitchcock, Jr., and Mr. Hartwell, and since described by Professor Dana and analyzed by Mr. Craw,* possesses much interest from the distinctness of its crystals (which yet in their angles present unaccountable irregularity), since it belongs to a class of minerals which are in general found massive, or but imperfectly crystallized. The following are the results of a chemical examination of some pure specimens, for which I am indebted to Mr. C. Hitchcock. They do not add much to our knowledge of the mineral, but serve to confirm essentially the former determinations by Mr. Craw.

The crystals are opaque and of a dark brownish black color, and give a beautiful violet streak. Sp. gr. = 3·364, higher therefore than that of the specimen analyzed by Mr. Craw, which he gives as 2·876. Hardness about 5. Before the blowpipe the reactions of phosphoric acid, iron, and manganese, are easily obtained.

A portion of the mineral was pulverized, weighed, and kept for some time at the temperature 100° C. The loss of weight was scarcely appreciable. This portion was then exposed to a bright red heat, and on cooling was found to have assumed a light brownish yellow color, and to have lost 6·33 p. c. In another experiment the loss was 5·97 p. c. To ascertain the amount of water contained in the mineral, a portion, dried as before at 100° C., was heated in a glass tube in a stream of dried air, and the water expelled was absorbed by chlorid of calcium and weighed. It amounted to 1·92 p. c. In another experiment the pulverized mineral was heated in dry hydrogen, and lost 2·18 p. c. of water beyond that formed by the reduction of the peroxyds of iron and manganese to protoxyds.

The phosphoric acid and peroxyds were determined by fusion with carbonate of soda, and the lime, magnesia, and lithia, were estimated in a separate portion. The results of analysis were—

	1.	2.	3.	4.
Phosphoric acid, - -	43·12	43·35	42·65	——
Peroxyd of iron, - -	29·90	29·23	29·37	——
Sesquioxyd of manganese,	23·02	21·98	22·76	——
Lime, - - -	——	——	——	·09
Magnesia, - - -	——	——	——	·73
Lithia, - - -	——	——	——	1·79

* Amer. Jour. of Science, [2] xi, 99, 100.

The iron and manganese appeared to exist altogether as sesquioxyds. The lithia contained a little soda, but the quantity of the latter was too small to separate and weigh.

The mean of these results makes the composition of the mineral—

			Atoms.
Phosphoric acid, - - - -	43·04	·598	
Peroxyd of iron, - - - -	29·50	·369	·654
Sesquioxyd of manganese, -	22·59	·285	
Lime - - - - - -	·09	·003	
Magnesia, - - - - -	·73	·036	·163
Lithia, - - - - - -	1·79	·124	
Water, - - - - - -	2·05	·228	
	99·79		

Hence we have the complex and not very probable formula $5\bar{R}^4\bar{\dot{P}}^3+\bar{R}^5\bar{\dot{P}}^3+7\dot{H}$; but if we consider, as suggested by Mr. Craw, that the iron and manganese originally existed as protoxyds, the above numbers give the equivalents of phosphoric acid, protoxyds, (adding in the lithia and earths), and water, in the ratio 598 : 1471 : 228, or very nearly 2 : 5 : 1, although the water does not amount to quite 1 atom. Hence we have the much simpler formula, $\dot{R}^5\bar{\dot{P}}^2+\dot{H}$, which is that of Damour's alluaudite, if we reduce the per- to protoxyds as above, though that mineral differs from the present in containing soda instead of lithia, and in the manganese actually existing to a great extent as protoxyd, while in the substance from Norwich the peroxydation of the metals has been completed.

There have been already described three phosphates occurring in nature which have this general formula, with the exception of the water, which varies in amount in each—1st, this mineral from Norwich and the Alluaudite from Limoges, the formula of which is $\dot{R}^5\bar{\dot{P}}^2+\dot{H}$; 2nd, Heterosite or Hetepozite, of Dufrénoy, from Limoges, with the formula $\dot{R}^5\bar{\dot{P}}^2+2\dot{H}$; and 3rd, Hureaulite, also from Limoges, and represented by $\dot{R}^5\bar{\dot{P}}^2+8\dot{H}$. Whether any of these minerals deserve to rank as distinct species seems very doubtful. It would seem more likely that they are all the mere products of a gradual alteration, in the course of which the heavy metals were more or less peroxydized, water was taken up, and probably some of the alkaline constituents of the mineral were lost.

This last mentioned action seems indicated as having affected the lithia of the phosphate from Norwich, since the phosphoric acid found is a little in excess of that required by the formula.

Art. V.—*On the Homœomorphism of Mineral Species of the Trimetric System;* by James D. Dana.*

Although many cases of homœomorphism among minerals of the Trimetric System have been pointed out by different investigators, no general review of the species has yet been made. We propose, therefore, to consider the relations in form among all the species, believing that in this way, and in this way alone, we may arrive at the true system among the homologies, and the principles upon which they rest.

In the outset, it is important to ascertain what may be considered true criterions of homology in the comparison of forms. In a trimetric crystal there are often several occurring prisms in the three axial directions, the vertical, macrodiagonal, and brachydiagonal, and as either axis might be assumed to be the vertical axis, and either prism in each direction the fundamental prism,† there are wide limits as to the possible cases of homœomorphism that might be made out. So among rhombohedral forms, in Calcite for example, rhombohedrons occur of a great variety of angles, and homœomorphism may be deduced between it and almost any rhombohedral species, provided any one of these rhombohedrons may for the time be taken as fundamental.

There is obviously one right position for the comparison of two species, and the others are wrong. Hence it is essential to have some basis for deciding upon this point, and especially for ascertaining which is the true vertical axis, in order that we may compare like axes and their planes with one another.

It must be admitted that there are no tests of homology which are of invariable application. As elsewhere in science, the relations of species are to be ascertained rather by the general range of characters, than by the severe application of one single law. But there are important aids, and their exact value should be understood.

1. *Cleavage.*—Cleavage is one of the most important means. In the trimetric system, it may take place parallel, (1) to the axial sections, one or all; (2) to the lateral planes of different rhombic prisms; (3) to octahedral planes.

a. When cleavage is parallel to one or more rhombic prisms, it is generally true that, (1) the vertical axis of the prism of most perfect cleavage is the proper vertical axis of the species, and also that (2) these cleavage prisms for different species are homologous prisms.

* From the Annals of the Lyceum of Nat. Hist. New York, vi, 37, March, 1854.

† A fundamental vertical prism is one which has for its axes *b*, *c*, the ratio 1*b* : 1*c*. The fundamental macrodome and brachydome have the analogous ratios 1*a* : 1*b*, and 1*a* : 1*c*. These are the *unit* prisms.

Hornblende and Augite correspond to the *first* of the two principles just stated, but are well known exceptions to the *second:* the cleavage prism of one has twice the breadth of that of the other. These species, nevertheless, are closely homœomorphous, and hence there may still be homology when the cleavage forms have a simple axial relation, as 1 : 2. Diaspore and Göthite exemplify the same fact; the former has an imperfect cleavage parallel to the prism $i\bar{2}(\infty P\bar{2})$. Staurotide and Andalusite may be viewed as another example. The occurring forms of these species have the same relation as those of hornblende and augite, or a ratio of 1 : 2, in the longer lateral axis, and traces of cleavage correspond; while in topaz, a third homœomorphous species, both forms are common, and indistinct cleavages are described as occurring parallel to each.

In some cases, when there are two cleavage prisms at right angles with one another, we are required by the analogies of the species to take as the vertical axis that parallel to the prism of least perfect cleavage; but such examples are rare.

b. It is common to find a prismatic and a diagonal cleavage existing together. In a single natural group of species, the former may become obsolete, while the latter is highly developed, or the reverse; and therefore *the presence or absence of a diagonal or basal cleavage is no test of identity.* The anhydrous sulphates are a prominent example. In *Celestine* and *Heavy Spar* a basal and prismatic cleavage exist, and the two diagonal cleavages are imperfect; while in *Anhydrite*, of the same group, the basal and diagonal are highly perfect, and no prismatic cleavage has been detected. In rhombohedral forms, a basal cleavage often occurs along with a rhombohedral, and in species actually homœomorphous, it may become the only cleavage, or be wholly obsolete. It is, however, often true, that a particular direction of cleavage characterizes a group of species. In the *Heulandite* group there is a perfect clinodiagonal cleavage; the *Feldspars* have a basal and clinodiagonal; the species of the *Calcite* series have a perfect rhombohedral cleavage, and no distinct basal, while the *Corundum* series have generally a basal cleavage, more distinct than the rhombohedral.

2. *Twin-composition.*—In compound crystals composition takes place in general, parallel to planes or sections of fundamental value. This is well seen in monometric forms, in which the only planes of composition are, (1) the faces of the cube; (2) the faces of the regular octahedron, or planes truncating the solid angles; (3) the faces of the dodecahedron, or planes truncating the edges of a cube. It will be observed that the composition is either at the extremities of the axes (1), or at points exactly intermediate between *three* axes (2), or between every *two* (3). This narrow limit to the possible directions of

twin-composition gives importance to its indications, and therefore ***similarity in modes of composition suggests identical or homologous relations between the planes of composition in different species, and vice versa.*** Thus when we observe different species, as *Aragonite*, *Cerusite*, etc., affording stellate twins and hexagonal forms by composition parallel to the faces of a prism nearly 120° in angle, we infer that the prisms are homologous; and when similar prisms occur in *Chrysoberyl* or *Copper Glance*, we conclude that the prism of 119° in these species, parallel to faces of which the composition takes place, is the true vertical prism, as in Aragonite. The fact that $120° \times 3$ or $60° \times 6$ equals 360°, is evidently the fundamental reason for the occurrence of such twins; and hence in other species a like angle for the vertical prism, especially if the prisms are alike in their other dimensions, would be likely to produce the same result.

Hence we conclude that the sulphates (RO, SO^3), although affording in one direction a prism near 120° in angle, have not this prism as the fundamental vertical prism, for stellate composition, does not occur parallel to it; the true vertical prism is the one usually so assumed—that of 101° to 104°.

Bournonite affords another illustration of this subject. G. Rose has assumed its homœomorphism with Aragonite, on the ground that it has a vertical prism of 115° 58′. But this species, instead of forming twins parallel to the faces of this prism, actually affords cruciform twins parallel to a prism of 93° 40′, the one usually taken as the fundamental prism. The prism of 115° 58′ is $i\,\frac{3}{2}\,(\infty P\,\frac{3}{2})$ and there is no reason for regarding it as other than a secondary prism.

Chrysoberyl has been placed near chrysolite by the author, and also by M. Scacchi, of Naples. In a certain position the resemblance in angle exists. But still the species are rather widely remote, inasmuch as the twins, like those of Aragonite, parallel to faces of the prism of 119° 46′, show that this is the fundamental prism. Chrysolite affords no such twins; the angle of its vertical prism is 94° 3′, and it belongs to a different zone. Chrysoberyl is actually near Aragonite in angle; it has a brachydome of 108° 26′, and Aragonite one of 109° 39′.

Monoclinic prisms near 120° in angle, never present stellate twins like trimetric prisms. Such twins in oblique forms appear to be impossible, since they require a regular symmetrical character in the molecule above and below the middle section. This remark appears to apply also to hemihedral forms of the trimetric system, like those of datholite.

3. *General Habit of Crystals.*—A resemblance in general habit is often to be detected between species related in crystallization. Thus Brookite, as figured in this Journal, vol. xvii, p. 86, resembles Columbite in the general arrangement of its planes; and we

cannot mistake, in comparing them, as to the homologous prisms of the two. Again, it requires but a glance at the forms of feldspar and pyroxene to see that the habit here is wholly opposed to any homœomorphism between the species, while the family resemblance among the feldspars themselves is very striking.

4. *Frequency of Occurrence of Planes, or Zones of Planes.*—This criterion is sometimes of importance, and still it is very likely to lead astray. It is the common principle on which crystals are mathematically described, for that is usually assumed as the fundamental form which will give the simplest mathematical view of the crystallization. But it is well known that in many species *secondary* forms are most common. In Quartz, the fundamental form is rarely seen; in Calcite, the rhombohedron $-\frac{1}{2}R$ and scalenohedron R^3, are of far more frequent occurrence than R; in fluor, cubes are more common than octahedrons, the cleavage form; and octahedrons, when they occur, often have their surfaces made up of the angles of minute cubes; and the same is true of many species. It is consequently no certain evidence, when a prism terminates in a pyramidal summit (as in mesotype), that it is the unit pyramid, or even that the occurring prism in a species is one of the three unit prisms. It is natural to assume that an occurring zone of planes is one having the simplest ratios, and that among them exists one having the axial ratio of unity, $1a:1b:1c$. But this may be far otherwise. Anhydrite is a familiar example. The occurring prisms, according to the view of the author,* are $\frac{2}{3}\bar{i}(\frac{2}{3}P\bar{\infty})$ and $\frac{3}{4}\breve{i}(\frac{3}{4}P\breve{\infty})$, which bring out well the homœomorphism of the species with the other allied sulphates; but the three octahedral planes are then $\frac{3}{4}\breve{\frac{9}{2}}$, $\frac{6}{4}\breve{\frac{18}{8}}$, and $\frac{9}{4}\breve{\frac{27}{8}}$; and in any other view that recognizes the homœomorphism, the expressions for the planes are scarcely less complex.

We cannot be too guarded, therefore, when deducing the form for comparison with another species, in relying on the prevalence of certain planes. Valuable hints are often thus given, but they may lead to error.

The lustre or smoothness of planes is a better guide, though far from certain. The fundamental vertical prism in Barytes is generally less highly polished than many other faces; and as we have above remarked, the octahedrons of fluor have often rough surfaces.

The prevailing direction of the more extended zones of planes, especially the octahedral, often suggests rightly which is properly the terminal plane of the prism, these zones rising towards that plane; and they thereby afford a hint as to which is the vertical axis. In dimetric and hexagonal species, this criterion is a sure

* Am. Jour. Sci., [2] 17, 88.

guide (except sometimes in hemihedral forms); but here it is not needed, as the basal plane is fixed from the nature of the prism. The principle holds true for topaz and many trimetric species. In the rhombic octahedron of sulphur, in which either axis might be made the vertical, the *apical* angles, in which the true vertical axis terminates, are at once distinguished in modified crystals, by the cluster of planes about them. But the ambiguous cases are numerous, and this criterion, like others, is not an unfailing reliance.

When we may succeed in fixing upon the vertical axis in a species, and also the unit vertical prism, it is often difficult to determine which planes about the base should be taken as the unit domes or octahedron; and often there is a choice between two or three planes equal in lustre and size; and consequently it may be altogether doubtful whether the vertical axis equals $1a$, $\frac{1}{2}a$, or $\frac{2}{3}a$. Crystallographers may take whichever is most convenient without any important objection. But when looking to homœomorphous comparisons, it is important that the special claims of each should be duly considered, instead of blindly adopting those which authors have found best to serve them in their mathematics.

5. *Analogies derived from Relations in Composition and Form.*—Similarity in chemical composition has long been known to suggest similarity in crystallization; and among species thus related it is usually safe to assume that prisms approximate in angle are homologous. Other more indirect analogies are often of weight, as illustrated in the case of Leadhillite, in a paper by the writer, on page 210, vol. xvii, of this Journal. We there see that the sulphates and sulphato-carbonates are parallel throughout in their homœomorphisms, and we ascertain with much probability which is the fundamental vertical prism in Leadhillite.

6. *Values and Relations of the Angles of Forms.*—In the series of prisms in each axial direction, the vertical, macrodiagonal, and brachydiagonal, the planes, as is well known, have simple axial ratios, and the more common ratios are 1 : 1, 1 : 2, 2 : 3. If but a single prism occur in either direction, it is easy to calculate the values of the angles of other prisms having the above mentioned relations. This gives a series of angles. If, then, two species correspond nearly with one another in one element of such a series, they are also related in others, and they are evidently related in form. From the exceptions to the several criterions mentioned, it is evident that the absolute relation of the axes may not in many cases be ascertainable. The vertical axis, for example, may be doubled in length without violating any principle that can be laid down; or it may be halved in the same way. But we may with certainty determine whether forms are

related in the *series* of angles, and when so related, the species are in a correct sense homœomorphous. Augite and Hornblende may be regarded as differing in this way, as we can by no criterion decide that the lateral molecular axes of Hornblende and Augite are identical; we know that they are so related that one form might be a secondary to the other,—that the prism of hornblende has its orthodiagonal twice that of augite in length, and that the serial relation of the forms is such that they may be said to belong to one type. This point will be abundantly illustrated beyond. We observe that in all the comparisons made in the following tables, the only changes from the forms assumed by authors made on the above principles to exhibit the homœmorphism of species, are such as depend on the simple ratios, 1 : 2, 2 : 3, 3 : 2, 2 : 1. No torturing of the forms has been required by employing unusual or complex ratios, notwithstanding the hypothetical manner in which the received fundamental forms have been in many cases assumed.

The preceding are some of the methods that are of importance in determining the crystallographic homologies of species. It appears that the *first* point to be determined, is the true vertical axis of species under comparison; and this being ascertained, the *second* is to fix upon the fundamental or unit vertical prism, or that which shall give the relative values of the lateral axes; and *third*, we have to determine upon a unit dome, either a macrodome or brachydome, in a trimetric species, or else the unit octahedron, in order thereby to ascertain the true value of the vertical axis; and *fourth*, to make out the serial relations of forms, for a full comparison where the actual relations of the axes may be doubtful.

While studying forms by the above methods, it is also of interest to compare them as a whole without reference to which is the vertical prism; and only by viewing them thus in every different light can we fully understand their actual dimensional relations. In this point of view, the results of Hausmann respecting the anhydrous sulphates and carbonates are highly interesting, although secondary in importance to comparisons between the forms when placed in homologous positions.

The position of the vertical axis derives special importance from the crystallogenic nature of molecules. In a trimetric molecule, if we suppose three crystallogenic axes, a vertical and two lateral, while the vertical is at right angles to the lateral, from the nature of the form, the lateral may either intersect at right angles, corresponding to the form of a rectangular prism, or at oblique angles, corresponding to the angle of a rhombic prism; that is, in other words, they may connect the centres of the lateral faces of a rectangular prism or of a rhombic prism. Either condition will express the forces as indicated by the form, and result in the

solids of the trimetric system. And when the cleavage prism is rhombic, there is better reason for regarding the lateral axes as oblique in their intersections, than rectangular. The subject of twin crystals affords evidence that this is not mere hypothesis;* and additional proof is shown beyond in the relations of the domes to the angles of the regular octahedron. And still another argument may be derived from the relations of the domes in angle to the vertical prism. If such views may be adopted, it must obviously be essential to correct comparisons of form between species, that the *vertical* axis should be determined on the best possible data.

The preceding remarks are offered as introductory to the following tables of the values of the axes and principal prisms in trimetric mineral species. I have endeavored to apply with fidelity the principles that have been briefly reviewed. The unit prisms, as has been stated, are not in all instances those assumed as such by other authors; but although they are in general well entitled to be so regarded, they are not all supposed to be the unit prisms, as has been explained by referring to Hornblende and Augite as examples. An exhibition of the mathematical relations of the forms is the main point in view. Whenever we have placed in the columns of unit prisms, angles usually regarded as those of other prisms, it is stated by a mention of the form to which they have been commonly referred. Thus, under Chrysolite, the prism taken as $1\bar{\imath}$ is $\frac{1}{2}\bar{\imath}$ of most writers, as mentioned. These forms, as observed, differ from the unit prisms, either by the ratio 1 : 2 or 2 : 3, ratios of the simplest kind.

The trimetric species are naturally divided into *four* grand groups, differing in the angle of the fundamental or unit vertical prism (angle *I* : *I* of the tables, ∞P : ∞P of Naumann), as follows:—

1. Angle *I* : *I* from 90½° to 95°.
2. Angle *I* : *I* near 102°, or from 98° to 105°.
3. Angle *I* : *I* near 110°.
4. Angle *I* : *I* near 120°.

It will be shown that these specific values of the angle *I* : *I* are dependent on a principle of the most fundamental character. The *third* Group may however belong with the *second* as remarked upon beyond.

The angles mentioned in the table are the obtuse angle of the prism *I* : *I* (column 1), and the summit angle of the unit macrodome and brachydome ($1\bar{\imath}$ and $1\breve{\imath}$ or P$\bar{\infty}$ and P$\breve{\infty}$).†

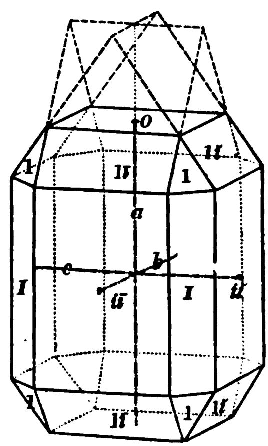
* See the author's Treatise on Mineralogy.

† To avoid any ambiguity in the angles referred to in the following pages, and render the subject intelligible to those who may not be familiar with crystallographic language, a few explanations are here given. The annexed figure represents

TABLE I.

Angle of Vertical Prism near 90°.

	Vertical Prism. $I : I$	Macrodome. 1ī : 1ī	Brachydome. 1ĭ : 1ĭ	Axes. $a : b : c$
I.				
Thomsonite, - - - -	90° 40′	108° 18′	108° 56′	0·72253 : 1 : 1·0117
Mesotype, - - - - -	91°	(2ī)108° 46′	(2ĭ)109° 42′	0·71644 : 1 : 1·0176
Harmotome, - - - -	91° 46′	108° 48′	110° 26′	0·71626 : 1 : 1·0312
Wöhlerite, - - - -	90° 54′	108° 2′	108° 56′	0·7261 : 1 : 1·01583
Pyrolusite, - - - -	93° 40′	104° 22′	(½ĭ)107° 54′	0·77601 : 1 : 1·0661
Andalusite, - - - -	90° 44′	109° 6′	109° 50′	0·71198 : 1 : 1·0129
Lievrite, - - - - -	(⅜)91° 32′	111° 14′	112° 40′	0·68429 : 1 : 1·0271
Staurotide, - - - -	(½)93° 8′	(½ī)108° 12′	111° 10′	0·72388 : 1 : 1·05617
Wavellite, - - - -	(½)90° 34′	(2ĭ)106° 14′	106° 46′	0·75047 : 1 : 1·0099
Olivenite, - - - - } Libethenite, - - - }	92° 30′	108° 28′	110° 50′	0·72034 : 1 : 1·0446
Caledonite, - - - -	95°	(½ī)105° 8′	(½ĭ)109° 54′	0·76568 : 1 : 1·0913
Chondrodite, - - - -	94° 26′	(½ī)106° 52′	(½ĭ)111° 4′	0·74176 : 1 : 1·0805
Antimony Glance, - -	90° 45′	(⅜ī)109° 26′	(⅜ĭ)110° 8′	0·6901 : 1 : 1·0132
Do. do. - -	90° 45′	(1ī) 88°	(1ĭ) 88° 47′	1·0352 : 1 : 1·0132
Polycrase, - - - - -	95°	(1ī) 88° 30′	(1ĭ) 93° 53′	1·02655 : 1 : 1·0913
II.				
Epsomite, - - - - -	90° 34′	120° 4′	120° 33′	0·57657 : 1 : 1·01
Diaspore, - - - - -	93° 52′	115° 16′	118° 42′	0·63398 : 1 : 1·0699
Göthite, - - - - -	94° 52′	113° 6′	117° 30′	0·66063 : 1 : 1·0888
Polianite, - - - - -	92° 52′	115° 26′	118°	0·6317 : 1 : 1·0513
Euchroite, - - - - -	92° 8′	117° 20′	119° 13′	0·6088 : 1 : 1 038
Topaz, - - - - - -	(½)93° 8′	(⅜ĭ)115° 22′	(⅛ī)118° 10′	0·63258 : 1 : 1·05617
Chrysolite, - - - -	94° 3′	(½ī)115° 36′	(½ĭ)119° 12′	0·6297 : 1 : 1·0738
Triphyline, - - - -	94°	(½ī)118° 27′	(½ĭ)121° 55′	0·59549 : 1 : 1·0724
Bournonite, - - - -	93° 40′	(⅜ī)115°	(⅜ĭ)118° 14′	0·63745 : 1 : 1·0662
Do. - - - - -	"	(1ī) 92° 34′	(1ĭ) 96° 12′	0·95618 : 1 : 1·0662
? Warwickite, - - - -	93°–94°			
? Lanthanite, - - - -	93° 45′			



a rectangular prism with replaced edges and angles, and the three axes *a*, *b*, *c*. *O* is the basal plane of the prism; *iī* the larger lateral plane, parallel to the *longer* lateral axis, or macrodiagonal, *c*; *iĭ* the smaller lateral plane parallel to the *shorter* lateral axis, or brachydiagonal, *b*. *I* are planes on the vertical edges of the rectangular prism, which when extended so as to meet one another, would form a vertical rhombic prism, having its axes *b*, *c*, in the ratio of 1*b* : 1*c*. It is therefore the *unit* or *fundamental* vertical prism. 1ī are planes parallel to the longer lateral axis, *c*, having for the axes *a*, *b*, the ratio 1*a* : 1*b*; extended upward they form a *dome*, called the *macrodome*. The planes 1ĭ, in a similar manner, constitute a *brachydome*, or dome parallel to the *shorter* lateral axis, and having the ratio 1*a* : 1*c*. These two domes are therefore the *unit* domes. The planes 1 on the eight angles are planes of an octahedron, having for the axes *a*, *b*, *c*, the ratio 1*a* : 1*b* : 1*c*; it is therefore the *unit* octahedron.

Taking axis $b = 1$, $c =$ tangent of half the angle $I : I$; and $a =$ cotangent of half the summit angle 1ī : 1ī. These two angles alone are a correct exhibition of the degree of homœomorphism between species; all other angles are dependent upon these, and there-

The preceding table is naturally subdivided into two sections:

I. Species having the summit angles of the domes, near 109°.
II. Species having the summit angles of the domes, near 120°.

In the first of these groups there is a remarkable closeness of coincidence to the angle mentioned; and in the second, the variation from 120° in the brachydome is but small. The vertical axis typical of the groups differs therefore theoretically as $\sqrt{3} : \sqrt{2}$, which is nearly as 6 to 5.

In section I, the axes a, b, c, have nearly or typically the ratio $1 : \sqrt{2} : \sqrt{2}$. In Andalusite, the ratio is almost identical with this, and 109° 28′ is exactly a mean between 109° 6′ and 109° 50′, the angles given for the two domes.

In section II the ratio of the axes approaches $1 : \sqrt{3} : \sqrt{3}$, which it is very closely in Epsomite, the domes of which are nearly 120°.

109° is approximately the angle of the regular octahedron, the faces of which solid incline to one another 109° 28′. Moreover the angle of the vertical prism I varies but little from that of a cube, or 90°. Here is an obvious relation to monometric forms not to be overlooked. Moreover, the angle 120°, in section II, is the angle of the dodecahedron.

In the change, therefore, in a case of dimorphism, from the monometric to these trimetic forms, the characteristics of the monometric molecule, or form, are to a considerable degree retained.

It is to be observed that the domes $2\bar{\imath}$ and $2\breve{\imath}$ for the same species afford nearly the angle 71°, the supplement of 109°; in fact, 109° 28′ for $1\bar{\imath}$ would give precisely the supplement 70° 32′ for the summit angle of $2\bar{\imath}$. In several of the species the occurring dome is that of 70°–71°, instead of that of 109°; so that either might be taken as characteristic of the first section in table I. 70° 32′ is the summit angle of the regular octahedron.

If, therefore, we compare the regular octahedron with the *rectangular* octahedron that would result from the united domes $2\bar{\imath}$ and $2\breve{\imath}$ in the species of section I, we find them nearly identical. We observe, further, the important fact, that *the axes of the regular octahedron correspond to diagonals between the apices of the basal angles of the rectangular octahedron.* But these axes in the latter solid, cross at *oblique angles* equal to the angle of the rhombic prism I, instead of at right angles; and they correspond to lines between the centres of opposite lateral faces of the rhombic

fore a long series, for the sake of comparison although often given, is not necessary or even desirable.

As $1\bar{\imath}$ is the unit macrodome, so $2\bar{\imath}$ will be a macrodome with the vertical axis *twice* as long; $\frac{2}{3}\bar{\imath}$, one *two-thirds* as long; $\frac{1}{2}\bar{\imath}$, one *half* as long; and so on. The first figure or letter in a symbol refers always to the vertical axis a, and the other to the *longer* or *shorter* lateral axis, according as it has over it the long or short mark, ¯ or ˘.

prism, I, and not to those between the centres of its opposite lateral edges. In other words, these lines are not the *crystallographic* axes of the Trimetric system, but what the author has called the *crystallogenic* axes. This is one reason alluded to on a preceding page for believing that the crystallogenic axes are not necessarily the same lines with the crystallographic. The latter are lines assumed for the convenience of calculation.

If instead of the domes $1i$ in section I, the species had afforded $\frac{3}{2}i$ as common and dominant forms, and these were taken as the unit domes, then the unit octahedron, in place of the domes, would have the pyramidal angles near 109°, approaching those of the regular octahedron. Could we therefore assume this as the fundamental octahedron for the species, the derivation of the octahedron from the regular octahedron would be a change in the lengths only of the axes, and not in their angles of intersection. But this assumption would do violence to the facts. Still in Antimony Glance, we have an example probably of this form and mode of derivation; the dominant form is an octahedron, with the pyramidal angles 109° 16′ and 108° 10′, and basal 110° 58′. Bournonite and Polycrase may be other examples of a similar nature, though diverging more in their angles.

Although the two sections are strongly marked in the above table, still the species of one may be regarded as homœomorphous with those of the other. Thus Chrysolite of Group II, and Chondrodite of Group I, have been recognised by Scacchi as homœomorphous. So also Andalusite and Topaz are essentially homœomorphous, as well as similar in chemical formulas. In both of these cases, one of the species contains fluorine, and this is evidently the occasion of the wide divergence. Yet in one instance the fluorine species (chondrodite) belongs to section I, and in the other (topaz) to section II.*

The table affords examples, also, of the principle stated on a preceding page, that homœomorphous species, while identical in the particular axis which is the vertical, may vary by a simple ratio (1 : 2 or 2 : 3) in the axes, and that they are to be recognised as species that belong to a specific system of ratios, rather than to definite and identical dimensions.

Andalusite, Staurotide, and Topaz, have this relation. The forms of these species may be referred to a similar type; yet we cannot affirm that the axes have the near identity presented in the table, rather than a multiple ratio of 1 : 2 in some of the axes; we only know that they pertain to a common series.

Staurotide alone offers a choice between three uncertainties. The occurring form is a prism of 129° 20′ and this is usually

* The evidence as to the isomorphism of oxygen and fluorine, as shown by the relations of Andalusite and Topaz both in form and composition, were first pointed out by the author in vol. ix, p. 407, 2nd Series, of this Journal, and afterwards in his Mineralogy, 3d edition, 1850, p. 366.

taken as the unit vertical prism. A prism with the longer lateral axis *half* as long, has the angle 93° 8′, and this approaches the prism of Andalusite; and as the frequency of occurrence of a plane is no sure proof that the plane is necessarily of the fundamental series, we may with some reason assume the prism of 93° 8′ to be the fundamental one. But Staurotide forms twins in two directions, or parallel to two planes, and neither of these planes, referred to the above fundamental forms, has a simple ratio or expression, and this, notwithstanding the general fact that the faces of composition are of the highest value in ascertaining the directions of axial sections: moreover, one of the planes has the unusual symbol $\frac{3}{2}\breve{\frac{3}{2}}$ if referred to the prism of 129° 20′, and $\frac{3}{2}\breve{\frac{3}{4}}$ if referred to that of 93° 8′. Now, if instead of halving the longer lateral axis, we take *two-thirds* for the new axis *c*, then the expression is of the simplest kind in every respect. The following are the angles and symbols of the planes according to these three methods:—

	$a : b : c$
A.—Prism I=129° 20′; $1\bar{\imath}$=69° 16′; $\frac{3}{2}\bar{\imath}$ (one face of composition)=88° 24′; $\frac{3}{2}\breve{\frac{3}{2}}$ other face of composition.	1·4478 : 1 : 2·11233
B.—Prism I=93° 8′; $2\bar{\imath}$=69° 16′; $\frac{3}{2}\bar{\imath}$ (one face of composition)=88° 24′; $\frac{3}{2}\breve{\frac{3}{4}}$ other face of composition; $1\bar{\imath}$=108° 12′; $1\breve{\imath}$=111° 10′.	0·7239 : 1 : 1·05617
C.—Prism I=109° 14′; $1\bar{\imath}$=69° 16′; $1\breve{\imath}$ (one face of composition)=88° 24′; 1 other composition face.	1·4478 : 1 : 1·40822

In the last, the planes, and the faces of composition have all a unit ratio, and it affords the simplest possible view of the crystallization. Whether regarded as the fundamental form or not, the relation to andalusite is shown by the fact of the two belonging to one and the same series or system of ratios.

Topaz has $I : I$=124° 19′ and 55° 41′, and $i\breve{2} : i\breve{2}$=86° 52′ and 93° 8′. The two prisms might either be taken as the fundamental, with nearly equal propriety. If the first be so taken, and the macrodome of 58° 31′ be the unit one, the axes are $a : b : c$ = 1·89774 : 1·05625 : 2 (=1·7587 : 1 : 1·8936), *a* being treble what it is in Table I, and *b* double, the *b* also becoming *c* or the longer lateral axis. If the unit macrodome is that of 96° 2′, the axes are the same except that *a* is half as long.

Lievrite is usually considered as having for its fundamental vertical prism, a prism of 111° 12′. Now this angle is near 109° 14′ for Staurotide, (type C); and taking $i\bar{\frac{7}{5}}$ as the vertical prism I, the angle is near that of Andalusite. Moreover the species has near relations in its domes to the species of Table I, and none to those of Table III. Besides, in composition it resembles Andalusite and the allied species, in having less oxygen in its silica than in its bases. These facts afford some reason for placing the species where it stands in Table I.

The following are notices of other species in Table I:

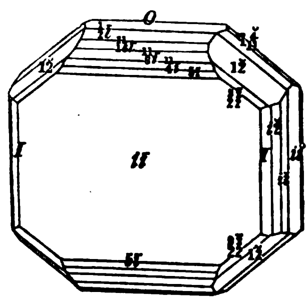
Chondrodite has for the summit angle of 1ī, in its three types, 68° 32, 64° 54′, 70° 29′, giving as the mean 67° 58′, from which the mean for ½ī (taken as 1ī in the table) is 106° 52′, and the extremes 103° 28′ and 109° 26′. The angle for 1ī in the New Jersey chondrodite is 68°. The great difference of angle for these varieties of a single species should be considered, when judging upon the differences among the several species in the table. Taking 1ī above as the unit dome, the vertical axis is twice that given in the table, or 1·48352. In *Chrysolite*, also, we have as good reason for doubling the vertical axis, in which case it becomes 1·2584. In *Caledonite*, the occurring brachydome has the angle 70° 57′, and taking this as a unit dome, axis $a =$ 1·53136.

The relations of *Polianite* to Göthite and Diaspore appear to sustain the conclusion of Volger, cited in the American Journal of Science, vol. xvii, p. 213.

Euchroite is generally placed in a different position, and the prism 117° 20′ (form 1ī) is made the fundamental vertical prism. But it forms no stellate or hexagonal twins like species of that angle, and nothing appears to sustain that view in preference to the one above taken.

Bournonite has the same relation to the species of Section II, that Antimony Glance has to those of Section I. It has very nearly the angles of Topaz.

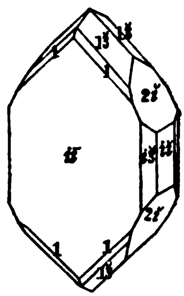
Wöhlerite has quite recently been studied by the able crystallographer of Paris, M. Descloizeaux.* He gives for the vertical prism, the angle 108° 56′. But by comparing the range of angles with those of the above species, it appears that its true relations are exhibited by the position in the annexed figure, which is altered from Descloizeaux. This gives for the vertical prism, the angle 90° 54′, and for the unit domes, the angles 108° 2′ and 108° 56′, very near Andalusite. It appears to be generally true that when a species affords for the prisms of two axes, angles (measured over the extremity of the other axis) nearly alike, this other axis is the true vertical, and the vertical prism is near 90° in angle.



Polymignite is near Wöhlerite in its crystallization. With the fundamental form adopted, the known octahedron is $2\bar{2}$ ($2P\bar{2}$), and the occurring prisms are 1ĭ = 109° 46′, 2ĭ = 70° 50′, 4ĭ = 39° 9′.

Polycrase affords angles in three directions near 90°, whichever position be taken. In the figure annexed, the position and lettering cor-

* Ann. de Chim. et de Phys., vol. xl, 3d series.



respond to the dimensions given in the table. Should we change it, and make the brachydiagonal the vertical axis, then :

$$I = 93° 52', \ 1\breve{i} = 91° 29', \ 1\breve{i} = 95° 2'.$$

And, again, if we make the macrodiagonal the vertical axis :

$$I = 91° 29', \ 1\bar{i} = 84° 58', \ 1\breve{i} = 86° 8'.$$

The symbols of these planes in these three positions (which we may call A, B, C,) are as follows :

A (figure)	1	1$\breve{3}$	2$\breve{i}$	$i\bar{i}$	$i\breve{3}$	$i\breve{i}$
B	1	3	$i\breve{2}$	O(base)	3$\breve{i}$	$i\breve{i}$
C	1	1$\breve{3}$	$\frac{1}{2}\breve{i}$	$i\bar{i}$	$\frac{1}{3}\bar{i}$	O (base)

The form is near that of Bournonite. It is also related distantly to the Columbite species, the prominent difference being *five degrees* in the angle of the vertical prism.

The species of Table II. fall into four sections, depending on the angles of the unit domes.

TABLE II.

Angle of the Vertical Prism I near 102°.

	Prism I.	Dome 1$\bar{i}$.	Dome 1$\breve{i}$.	Axes $a : b : c$.
I.				
Valentinite, - - - -	($\frac{1}{2}$) 103° 30′	58° 17′	70° 32′	1·7934 : 1 : 1·2683
Heavy Spar, - - - -	101° 40′	63° 40′	74° 36′	1·6107 : 1 : 1·2276
Anglesite, - - - - -	103° 38′	62° 42′	75° 29′	1·6415 : 1 : 1·2715
Leadhillite, - - - -	103° 16′	60° 20′	72° 34′	1·7205 : 1 : 1·2632
Celestine, - - - - -	104° 2′	62° 39′	75° 52′	1·6432 : 1 : 1·2807
Anhydrite, - - - - -	102° 56′	61° 25′	72° 38′	1·6836 : 1 : 1·2557
Tantalite, - - - - -	101° 32′	(2$\bar{i}$) 64° 7′	(2$\breve{i}$) 74° 58′	1·5967 : 1 : 1·2247
Mascagnine, - - - -	107° 40′	65° 52′	83° 6′	1·5437 : 1 : 1·3680
Atacamite, - - - -	97° 2′	67° 40′	74° 20′	1·4919 : 1 : 1·1310
Sulphur, - - - - -	101° 58′	($\frac{2}{3}\bar{i}$) 65° 18′	($\frac{2}{3}\breve{i}$) 76° 40′	1·5606 : 1 : 1·2342
" - - - - - -	"	(1$\bar{i}$) 46° 16′	(1$\breve{i}$) 55° 36′	2·3408 : 1 : 1·2342
II.				
Orpiment, - - - - -	100° 40′	73°	83° 30′	1·3511 : 1 : 1·2059
Dimorphine (I.), - - -	98° 6′	75° 40′	83° 40′	1·2876 : 1 : 1·1526
" (II.), - -	100° 32′	74° 2′	84° 24′	1·3262 : 1 : 1·203
Epistilbite, - - - - -	($\frac{1}{2}$) 100° 58′	($\frac{1}{2}\bar{i}$) 70° 50′	($\frac{1}{2}\breve{i}$) 81° 30′	1·4063 : 1 : 1·2121
Childrenite, - - - -	104° 14′	73°	87° 14′	1·3514 : 1 : 1·2853
III.				
Prehnite, - - - - -	99° 56′	(2$\bar{i}$) 89° 45′	99° 41′	1·0044 : 1 : 1·1904
Columbite, - - - - -	100° 40′	86° 45′	97° 28′	1·0584 : 1 : 1·2059
Wolfram, - - - - -	101° 5′	88° 6′	99° 13′	1·0337 : 1 : 1·2149
Mengite, - - - - -	100° 28′	87° 24′	97° 54′	1·0463 : 1 : 1·2071
Brookite, - - - - -	100°	83° 14′	93° 16′	1·1260 : 1 : 1·1918
Scorodite, - - - - -	98° 2′	84° 40′	92° 43′	1·0977 : 1 : 1·1151
Hopeite, - - - - -	101°	86° 38′	97° 40′	1·0607 : 1 : 1·2131
IV.				
Manganite, - - - - -	99° 40′	114° 19′	122° 50′	0·6455 : 1 : 1·1847
Calamine, - - - - -	103° 54′	116° 39′	128° 26′	0·6170 : 1 : 1·2776
Haidingerite, - - - -	100°	118° 32′	126° 58′	0·5945 : 1 : 1·1918
Brochantite, - - - -	104° 10′	(2$\bar{i}$)114° 29′	126° 41′	0·6434 : 1 : 1·2838
Cotunnite, - - - - -	99° 46′	118° 28′	126° 44′	0·5953 : 1 : 1·1868
? Mendipite, - - - -	102° 36′			x : 1 : 1·2482
? Jamesonite, - - - -	101° 20′			x : 1 : 1·2208

I. Angle of macrodome near 60°, and brachydome near 71°.

II. Angle of macrodome 70° to 75°, or near the brachydome of Section I.

III. Angle of macrodome 83° to 90, or near the brachydome of Section II.

IV. Angle of macrodome 114° to 120°.

In Section I, the angles of the domes oscillate from or about the monometric angles 60° and 70° 32′. In Section III, 90° is nearly a mean between the angles of the domes. In Section IV, 120° is a similar mean for the domes. Section II is intermediate between I and III, the macrodome corresponding with the brachydome of Section I, and the brachydome with the macrodome of Section III. The vertical axis in Section III is *two-thirds* that of Section I; and by taking $\frac{3}{2}\bar{\imath}$ as $1\bar{\imath}$, the two groups would coalesce. The vertical axis of Section IV is about *two-fifths* of that of I.

In Section I, a macrodome of 60° and a brachydome of 70° 32′, both monometric angles, necessarily imply a vertical prism of 101° 34′. Hence the important fact, that *prisms approximating to* 101° 34′ *are of common occurrence, and a necessary result of the relations pointed out to Monometric forms.* This affords a sufficient reason for the occurrence of so many species near 102° in angle, just as there are many near 90°, and gives special importance to this value of $I : I$. Such prisms have approximately

$$a : b : c = 1 : \sqrt{\tfrac{1}{3}} : \sqrt{\tfrac{1}{2}}.$$

Valentinite affords an interesting exemplification of the general principle. Oxyd of antimony is a known example of dimorphism, occurring in *regular octahedrons* as *Senarmontite* and in *rhombic prisms* as *Valentinite.* It would hardly be expected that the latter should retain closely any of the angles of the former; and yet there is a brachydome having exactly the angle 70° 32′. The cleavage vertical prism has the angle 136° 58′, which gives for the prism with half the macrodiagonal, 103° 30′,—a relation like that between hornblende and augite. The three unit prisms, 103° 30′, 58° 17′, 70° 32′, very nearly correspond to the typical value of the axes $1 : \sqrt{\tfrac{1}{3}} : \sqrt{\tfrac{1}{2}}$.

It is of interest in this connexion to compare *Epistilbite* with Valentinite. It presents the vertical prism 135°, corresponding to 136° 58′ of Valentinite; and there is a macrodome of 109° 46′, whence another macrodome $2\bar{\imath} = 70° 50′$, or very nearly the angle of the brachydome of Valentinite. It gives for $i\breve{2}$ the angle 100° 58′, as mentioned in the table. The occurrence of these Monometric angles has, beyond doubt, a profound significance. We hereby perceive in what respect Section II is related to Sections I and III. The oscillations from the typical angles of the group amount to about 5°.

Other species in Table II require special remarks.

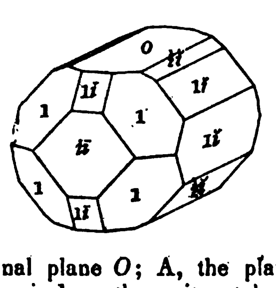
In *Sulphur*, the unit macrodome of authors has the summit angle 46° 18′. But taking $\frac{2}{3}\bar{i}$ as the unit dome, the angle is near that of Barytes and the other sulphates, as in the table; $\frac{1}{3}\bar{i}$ in sulphur is near $\frac{1}{2}\bar{i}$ in Barytes. *The homœomorphism of sulphur and the sulphates* (RO, SO^3) *is hence evident.*

Orpiment (sulphuret of arsenic) differs from sulphur in pertaining to Section II. The sulphur and arsenic compounds present a like amount of difference; and, further, they show that the fundamental vertical prism is that of 100° 40′, instead of that of 117° 49′, adopted by some authors. The difference in the unit domes of sulphur and orpiment is about 7½ degrees, and the difference in the sulphurets and arseniurets or arseniosulphurets is nearly as large, or 5 to 6 degrees.

The arseniuret of iron (Leucopyrite or Lölingite) has been hitherto described as having a vertical prism of 122°, and the arsenio-sulphuret (mispickel) an angle of 112°, and the sulphuret (marcasite) of 106°, But the writer has been informed by R. P. Greg, Jr., that he has measured crystals of the arseniuret, and found the angle the same as for the arsenio-sulphuret. The difference of 6 degrees is, therefore, the full difference due to the arsenic; and where arsenic is present with sulphur in these compounds, the sulphur is wholly overpowered by the arsenic; just as in the sulphato-carbonates, the sulphuric acid dominates completely over the carbonic acid, the angle agreeing closely with that of the sulphates (anglesite), instead of being half way between those of the carbonates and sulphates.

Dimorphine, a sulphuret of arsenic of undetermined composition, falls into the same group with orpiment, and is near it in angle. Professor Scacchi, in describing dimorphine,* recognises the fact that it affords two angles approaching those of orpiment, viz. 83° 40′ and 117° 48′; and he adds correctly, that they do not, however, correspond in position in the two species. But on examining further his type I, and viewing the form in a different position, we find that there are two prisms, which taken as domes give the angles at summit 83° 40′ and 75° 40′ (angles *o* : *o* and *e* : *e* in Scacchi, pl. 12, f. 4, or $1\breve{i}$ and $1\bar{i}$ in the annexed figure 1); and these angles are so near two domes in orpiment that we can hardly hesitate as to regarding this the right position for the figures. We here make B of Scacchi the terminal plane O; A, the plane $\bar{i}\bar{i}$; C, the plane $\breve{i}\breve{i}$; also o^2 is $\frac{1}{2}\breve{i}$, and m is 1 or the unit octahedron. In Scacchi's type II (figure 2, above), the planes referred to the same fundamental form, are $\frac{5}{4}\bar{i}$ (e of Scacchi, fig. 13, pl. 4), $i\frac{5}{4}$ (i), $\frac{5}{4}\frac{5}{4}$ (m), $\frac{1}{2}\breve{i}$ (o^2).

1.



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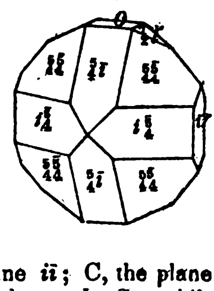


* Memorie Geolgiche sulla Campania per A. Scacchi, Napoli, 1849, p. 120.

In this type, the angles, as given in the table, are almost identical with those of Orpiment. The axes become for

$$b=1 \qquad\qquad a=1$$

Type I, $a:b:c=1{\cdot}2876:1:1{\cdot}1526=1:0{\cdot}77661:0{\cdot}89526$.
Type II, $a:b:c=1{\cdot}3262:1:1{\cdot}2030=1:0{\cdot}75405:0{\cdot}90707$.

The ratio $\frac{3}{4}$ in Type II loses its improbability, if any there be, when it is observed that *the domes of this ratio have approximately the angles of the unit domes of sulphur* or of the section to which sulphur belongs, they being $\frac{3}{4}\bar{\imath}=62°\ 12'$ ($e:e$, f. 13, of Scacchi), and $\frac{3}{4}\breve{\imath}$ (not observed) $=71°\ 56'$. They approach most nearly the unit domes of anhydrite.

Tantalite (FeO, TaO^3) has very nearly the dimensions of Barytes (BaO, SO^3), as seen in the table; and the fact is important, as it sustains the *homœomorphism of tantalic and sulphuric acids.*

Brookite was first observed to be homœomorphous with Columbite by Hermann. It differs by four degrees in its domes from that species, and has its vertical axis about one-twelfth longer.

In *Columbite*, it is of importance to note, that the face of twin composition is a plane of the brachydome $2\breve{\imath}$, in which the *basal* angle is about 120°; and in *Wolfram*, it is the brachydome $\frac{2}{3}\breve{\imath}$, in which the *summit* angle is about 120°.

Leadhillite has been shown by the writer to have close relations in angle to Anglesite, in this Journal, vol. xvii, p. 210. Its dimensions, as given in the table, exhibit still further this similarity of form. We reserve remarks on the forms of Leadhillite for another occasion.

Mascagnine diverges widely from the other sulphates in its vertical prism, and therefore also in its brachydome, while it agrees with them nearly in its macrodome.

We add a word on the unit-octahedrons of the species of Table II. The following are the angles for species in Sections I, III, and IV:

	Pyramidal Angles.		Basal Angle.
Section I.—Barytes, - - -	111° 38′	91° 22′	128° 36′
" Anglesite, - -	112° 13′	89° 41′	128° 54′
Section III.—Columbite, - -	117° 53′	102° 58′	107° 56′
" Brookite, - -	115° 42′	101° 34′	111° 26′
Section IV.—Manganite, - -	130° 49′	120° 44′	80° 22′
" Cotunnite, - -	133° 22′	123° 58′	75° 48′

It will be observed that there is an approximation to the angle of a regular octahedron only in one of the pyramidal angles of Section I, and in the basal angle of Section III.

TABLE III.

Angle of Vertical Prism, near 109° 28′.

	Prism *I.*	Dome $1\breve{\imath}$.	Dome $1\bar{\imath}$.	Axes $a:b:c$.
I.				
Marcasite, - - - - - -	106° 5′	64° 52′	80° 20′	1·5737 : 1 : 1·3287
II.				
Mispickel, - - - - - -	111° 53′	59° 14′	80° 8′	1·7588 : 1 : 1·4793
Leucopyrite, - - - -	111° 30′			
Aurotellurite, - - - -	110° 48′	($2\breve{\imath}$) 58° 52′	($2\bar{\imath}$) 78° 34′	1·7723 : 1 : 1·4496

The angle of the vertical prism in Table III is near the angle of a regular octahedron (109° 28′). As this prism is a cleavage prism, and the only distinct one in the species, it appears to be the true vertical prism.

But if we give the species another position, we may exhibit a relation to Sections II and III of Table II; and as they are all related to the species of those sections in composition, this relation is of fundamental interest. Making the brachydome $1\breve{\imath}$ the vertical prism, then the angle given above for the vertical prism is the new macrodome, and the supplement of that for the macrodome is the new brachydome. This gives for mispickel the angles $I:I=99° 52'$; $1\bar{\imath}:1\bar{\imath}=111° 53'$; $1\breve{\imath}:1\breve{\imath}=120° 46'$. If we now double the length of the vertical axis, the dome 111° 53′ becomes $\frac{1}{2}\bar{\imath}$, and 120° 46′ $\frac{1}{2}\breve{\imath}$; and the three angles will be

$$I:I=99° 52';\quad 1\bar{\imath}:1\bar{\imath}=72° 58';\quad 1\breve{\imath}:1\breve{\imath}=82° 40',$$

which are almost identical with the angles in Orpiment. The following table presents the angles and axes of the species thus changed in position, and also those referred to on Table II.

TABLE III. A.

	Prism I.	Dome $1\bar{\imath}$.	Dome $1\breve{\imath}$.	Axes $a:b:c$.
I.				
Sulphur, - - - - -	101° 58′	65° 18′	76° 40′	1·5606 : 1 : 1·2342
Marcasite, - - - - -	99° 40′	67° 12′	76° 24′	1·5049 : 1 : 1·1847
II.				
Orpiment, - - - - -	100° 40′	73°	83° 30′	1·3511 : 1 : 1·2059
Dimorphine (I.) - - -	98° 6′	75° 40′	83° 40′	1·2876 : 1 : 1·1525
do. (II.) - - -	100° 32′	74° 2′	84° 24′	1·3262 : 1 : 1·2030
Mispickel, - - - - -	99° 52′	72° 58′	82° 40′	1·3520 : 1 : 1·1890
Aurotellurite, - - -	101° 26′	71° 52′	83° 6′	1·3797 : 1 : 1·2225

It appears from the table that *Marcasite*, $Fe\,S^2$, is very near Sulphur in its angles and axes; while *Aurotellurite* (Ag, Au), Te^2, and *Mispickel*, $Fe(S, As)^2$, to which *Leucopyrite*, $Fe\,As^2$, should be added, have the form nearly of Orpiment. It is a question, therefore, whether Table III should not be suppressed, and the species annexed to Sections II and III of Table II. The cleavage constitutes the main reason for regarding the species as a separate Group. But notwithstanding the peculiarity in this respect, the affiliation with Sulphur and Orpiment is undoubted.

In Table IV. we recognize four sections:

I. Angle of macrodome near 70° 32′.
II. Angle of brachydome near 109° 28′.
III. Angle of macrodome near 109° 28′.
IV. Angle of brachydome near 120°.

The vertical axis in Section II is about one-fourth shorter than in Section I; in the latter $\frac{3}{4}\bar{\imath}=85° 40'$, which approaches $1\bar{\imath}$ in the former, being very nearly the angle of Stephanite.

TABLE IV.

Angle of Vertical Prism, near 120° (115½°–120°).

	Prism I.	Dome $1\breve{\imath}$.	Dome $1\bar{\imath}$.	Axes $a : b : c$.
I.				
Sternbergite, - - - -	119° 30′	69° 38′	100° 2′	1·4379 : 1 : 1·7147
II.				
Aragonite, - - - - -	116° 10′	81° 40′	108° 26′	1·1571 : 1 : 1·6055
Cerusite, - - - - -	117° 13′	80° 19′	108° 16′	1·1852 : 1 : 1·6388
Witherite, - - - - -	118° 30′	77° 30′	106° 54′	1·2460 : 1 : 1·6808
Bromlite, - - - - -	118° 50′	77° 18′	107° 5′	1·2504 : 1 : 1·6920
Stephanite, - - - -	115° 39′	85° 5′	111° 8′	1·0897 : 1 : 1·5844
Nitre, - - - - - -	118° 50′	80° 16′	109° 57′	1·1861 : 1 : 1·692
Chrysoberyl, - - - -	119° 46′	($\frac{3}{2}\breve{\imath}$)78° 54′	($\frac{3}{2}\bar{\imath}$) 109° 38′	1·2152 : 1 : 1·7239
Discrasite, - - - - -	119° 59′	81° 22′	112° 12′	1·1633 : 1 : 1·7315
Copper Glance, - - } Stromeyerite, - - }	119° 35′	($\frac{2}{3}\breve{\imath}$)83° 56′	114° 10′	1·1117 : 1 : 1·7176
III.				
Herderite, - - - - -	115° 53′	111° 42′	133° 58′	0·6783 : 1 : 1·5971
IV.				
Iolite, - - - - - - -	119° 10′	94°	121° 38′	0·9325 : 1 : 1·7033
Mica, - - - - - - -	119°–120°			

Chrysoberyl is very near Aragonite in angle, if the plane in the former usually regarded as $\frac{3}{2}\breve{\imath}$ be taken as $1\breve{\imath}$, as adopted in the table: otherwise the relation for the vertical axes of the two species is that of 3 : 2. So also *Copper Glance* approaches Aragonite, if what has been taken by authors as $\frac{2}{3}\breve{\imath}$ be regarded as $1\breve{\imath}$; otherwise the relation between them is that of 2 : 3. Such ratios, as we have elsewhere remarked, and the tables everywhere illustrate, are consistent apparently with homœomorphism in species. We have not sufficient data, at present, to decide whether the relation between Aragonite and Copper Glance is actually that of 1 : 1 or of 2 : 3, yet are inclined to believe the latter the fact; and if so, $1\breve{\imath}$ in Copper Glance has 61° 54′ for the summit angle, and 118° 6′ for the basal; the latter angle is near that of the vertical prism.

Many of the species in Tables II and III afford a horizontal prism or unit dome of 115° to 120°; and consequently, if this dome were taken as the fundamental vertical prism, the species would pertain to Table IV. Although we have not good reason for making the change, it is of some importance to view the species in this way, in order to apprehend more fully all the affiliations and relations of the forms. The author has alluded to Hausmann's comparisons by this method, of the anhydrous sulphates and carbonates; and he would here observe that the general review of Trimetric forms which he has made since his former paper was printed, and which has been here presented, has led him to give more importance to such comparisons than was implied in his paper in this Journal, vol. xvii, p. 210.

In the annexed Table the first column contains a statement of the particular dome in the preceding Tables which is here made the vertical prism; in some cases the angle of this prism is the supplement of that which is given for the dome in those Tables.

TABLE IV. A.

		Prism *I.*	Dome 1ī.	Dome 1ĭ.
From Table I.				
Chrysolite, - - - - - - - - -	1ī	115° 36′	60° 48′	85° 57′
Do. - - - - - - - - -	1ĭ	119° 12′	64° 24′	94° 3′
Triphyline, - - - - - - - -	1ī	118° 27′	58° 5′	86°
Do. - - - - - - - - -	1ĭ	121° 55′	61° 33′	94°
Epsomite, - - - - - - - - -	1ī	120° 4′	59° 27′	89° 26′
Do. - - - - - - - - -	1ĭ	120° 33′	59° 56′	90° 34′
Diaspore, - - - - - - - - -	1ī	115° 16′	61° 18′	86° 8′
Do. - - - - - - - - -	1ĭ	118° 42′	64° 44′	93° 52′
Göthite, - - - - - - - - -	1ī	113° 6′	62° 30′	85° 8′
Do. - - - - - - - - -	1ĭ	117° 30′	66° 54′	94° 52′
Polianite, - - - - - - - - -	1ī	115° 26′	62°	87° 8′
Do. - - - - - - - - -	1ĭ	118°	64° 34′	92° 52′
Euchroite, - - - - - - - -	1ī	117° 20′	60° 47′	87° 52′
Do. - - - - - - - - -	1ĭ	119° 13′	62° 40′	92° 8′
Topaz, - - - - - - - - - -	1ī	$(\frac{2}{3})$ 115° 22′	61° 50′	86° 52′
Do. - - - - - - - - -	1ĭ	$(\frac{1}{3})$ 118° 10′	64° 38′	93° 8′
Bournonite, - - - - - - - -	1ī	$(\frac{2}{3})$ 115°	61° 46′	86° 20′
Do. - - - - - - - - -	1ĭ	$(\frac{2}{3})$ 118° 14′	65°	93° 40′
From Table II.				
Valentinite, - - - - - - - -	1ī	121° 43′	76° 30′	109° 28′
Barytes, - - - - - - - - -	1ī	116° 20′	78° 20′	105° 24′
Anglesite, - - - - - - - -	1ī	117° 18′	76° 22′	104° 31′
Leadhillite, - - - - - - - -	1ī	119° 40′	76° 44′	107° 26′
Celestine, - - - - - - - - -	1ī	117° 21′	75° 58′	104° 8′
Anhydrite, - - - - - - - -	1ī	118° 35′	77° 4′	107° 22′
Tantalite, - - - - - - - - -	1ī	115° 53′	78° 28′	105° 2′
Mascagnine, - - - - - - - -	1ī	114° 8′	72° 20′	96° 52′
Sulphur, - - - - - - - - -	1ī	$(\frac{2}{3})$ 114° 42′	78° 2′	103° 20′
Manganite, - - - - - - - -	1ī	114° 19′	57° 10′	80° 20′
Calamine, - - - - - - - - -	1ī	116° 39′	51° 34′	76° 6′
Haidingerite, - - - - - - -	1ī	118° 32′	53° 2′	80°
Brochantite, - - - - - - - -	1ī	114° 29′	53° 19′	75° 50′
Cotunnite, - - - - - - - -	1ī	118° 28′	53° 16′	80° 14′
From Table III.				
Marcasite, - - - - - - - -	1ī	115° 8′	73° 55′	99° 40′
Mispickel, - - - - - - - -	1ī	120° 46′	68° 7′	99° 2′
Aurotellurite, - - - - - - -	1ī	121° 6′	69° 12′	101° 26′

Comparing the species in Table IV, A, with those of Table IV, we observe the following affiliations:

Marcasite and Aurotellurite are near Section I (Sternbergite).

From Valentinite to Sulphur (from Table II), are near Section II, (Aragonite Section).

From Chrysolite to Bournonite (from Table I), with also Manganite, are not coincident with either Section of Table IV, but they have approximately the ratio to the Aragonite Section of

4 : 3. This is the ratio between Chrysolite and Chrysoberyl. If 119° 12′ in Chrysolite be considered as corresponding to 119° 46′ in Chrysoberyl, then the macrodome of 64° 24′ in Chrysolite, if referred to the form of Chrysoberyl, would be $i\bar{\tfrac{4}{3}}$.*

From Calamine to Cotunnite, the vertical axis is to that of the Barytes series nearly as 5 : 3.

In reviewing the Groups of Trimetric forms, the most prominent fact observed is the prevalent approximation in the values of the angles of the unit prisms, to the three monometric angles, 90°, 109° 28′, and 120°, or their supplements, 70° 32′, and 60°; above all, the angles approaching 109° 28′ and 70° 32′ much predominate. When the vertical prism is near 90°, domes near 109° 28′ and 70° 32′, characterize very many of the species; while domes near 120° belong to the rest of the species. And in the second great group, macrodomes near 70° 32′, and 109° 28′, and brachydomes near 60° and 120°, determine the vertical angle of the prism, which approaches 101° 36′. Another large group has 120° and 60° as approximately the angles of the vertical prism.

The fact that the axial ratios 1 : $\sqrt{2}$ and 1 : $\sqrt{3}$ are typical of certain groups has been mentioned. It is easy to make out, in many cases, simple ratios between the axes, or the sum of two of the axes and the third; but the importance that should be attached to such ratios is questionable. The following are a few examples:

	Axes $a : b : c$	
Epistilbite (I=100° 58′),	1·4063 : 1 : 1·2121	$a+b=2c$.
Calamine, - - - - -	0·6170 : 1 : 1·2766	$2a=c$.
Brochantite, - - - - -	0·6534 : 1 : 1·2838	$2a=c$.
Cotunnite, - - - - -	0·5953 : 1 : 1·1868	$2a=c$.
Haidingerite, - - -	0·5945 : 1 : 1·1918	$2a=c$.
Göthite, - - - - - -	0·6606 : 1 : 1·0888	$3a=b+c$.
Polycrase, - - - -	1·0265 : 1 : 1·0913	$2a=b+c$.
Valentinite, - - - - -	1·7934 : 1 : 1·2683	$2a=b+2c$.
Sulphur ($1\bar{i}$), - - -	2·3408 : 1 : 1·2342	$a=b+c$.
" ($\tfrac{3}{2}\bar{i}$), - - -	1·5606 : 1 : 1·2342	$\tfrac{3}{2}a=b+c$.

In Valentinite this relation is evidently dependent on the more authoritative and equally exact ratio $a : b : c = 1 : \sqrt{\tfrac{1}{3}} : \sqrt{\tfrac{1}{2}}$.

Many conclusions bearing on chemical formulas, and the chemical relations of species, flow from the facts in the preceding tables. But we leave, for the present, that branch of the subject without further remarks.

* Since this paper was first printed, the author has found that M. Kengott of Vienna has recently pointed out similarities in the angles of Discrasite, Copper Glance, Antimony Glance, Bournonite, Chrysolite, Chrysoberyl, Tantalite, Topaz, Electric Calamine, Aragonite and Cerusite. But the true grounds of relation and distinction between these species, brought out in this paper, are not recognised.

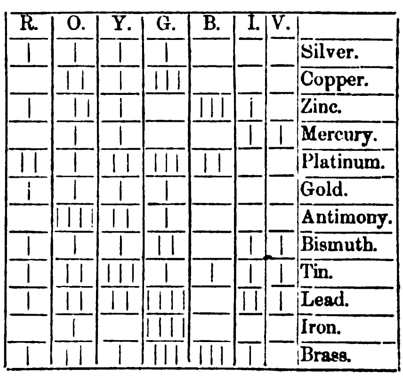
Art. VI.—*On certain Physical Properties of Light, produced by the combustion of different Metals, in the Electric Spark, refracted by a Prism;* by David Alter, M.D., Freeport, Penn.

We are indebted to the celebrated M. Fraunhofer for the fact, that the Solar Spectrum is crossed by numerous fixed lines, and that the light of some of the stars differs from that of the sun, in the number and situation of these lines.

In order to see some of these lines without the aid of a telescope, I ground a prism of flint glass, with large refracting angle, (74°). Viewing a fine slit made in sheet brass, when the source of light was the sky, nearly in the direction of the sun seen through this prism, I could count twelve or thirteen of Fraunhofer's dark lines. In viewing the blaze of a lamp, burning petroleum, I could discover neither dark nor bright lines, although I narrowed the light by passing it through the fine slit of sheet brass. I then tried the blaze of a tallow candle, when I could distinctly see an orange image of the blaze and one of faint yellow and one of green at the base of the blaze. The base of the orange image appeared to be the reflection of the light without any dispersion. When the brass with the fine slit is held in a horizontal position and the refracted light seen through it, is from the base of the blaze, bright bands, one of orange, one of yellow, one of green, one of blue, and one of violet, are seen.

The flame of alcohol is the same, except that the orange band is wanting. A slip of white paper shows the same bands when illuminated by a tallow candle. The jet of a blowpipe shows the five images still more distinctly.

The light from heated wire or charcoal shows no peculiarities. The electric spark from a Leyden Jar gave several bright images, which from optical illusion (perhaps from their brilliancy) appeared to extend beyond the sides of the spectrum, causing it to appear serrated along the edges.



But the most interesting effect of refraction, is from the spark caused by breaking the galvanic circuit, or at the break of a powerful magneto-electric machine. The machine I used produced sparks nearly half an inch in length. These sparks, viewed through the prism, appear almost wholly resolved into separate, colored bands, as illustrated in the annexed figure, where R is the red extremity of the spectrum, and V the violet.

Thus in the silver, there is in the orange a very bright band, one of yellow and one of green—two faint bands in the blue which are not always seen, and are probably caused by an impurity in the metal. The light which is not resolved into these bands is very faint except in the red.

The copper has two in the orange and three in the green—the other light appearing most distinct in the red and yellow. In the zinc there is a strong band of red, two of orange and three of blue with a faint yellow. The lead has two bright orange, and two in the yellow, nearer the orange than appears in the silver, then faint green bands and one bright violet, at the extremity of the spectrum.

Tin has a faint red, two of orange, three faint yellow, and a very faint green band, and also one of blue, indigo and violet.

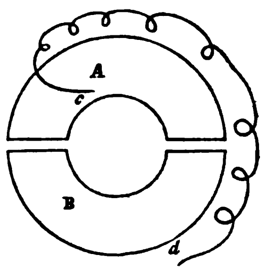
Iron exhibits a bright orange, four faint green, and sometimes two faint blue bands.

Bismuth a bright orange, a very faint yellow, two faint green, and a bright indigo.

Antimony some bright orange, and faint appearances in the yellow, green and blue.

Brass, a compound of copper and zinc, exhibits all the bands that are exhibited by these metals separately, i. e. one of red, one of orange, three of green, three of blue, one yellow and one indigo.

The preceding table presents these results, with some faint bands not above alluded to.



To illustrate better the manner of producing the sparks by the magneto-electric machine, I have annexed the following figure of the break. A and B represent two half circular discs of the metal intended to be used. One of these is connected with one end of the helix, and the other with the other end. They are fixed on the spindle of the revolving armature, and revolve with it. They are so placed, that the extremity *c* of the wire *cd*—which is stationary and rubs on them—is passed from the one to the other at the same time that the armature is passing the poles of the magnets.

In order to produce sparks, the end *d* of the wire is caused to rub on the discs, nearly opposite C—which causes a bright spark at each half revolution of the armature.

When the discs are of zinc, and the extremity *d* of the wire is of copper, the bands are the same as in brass, as also if the discs are of copper, and the wire zinc. When the discs and wire are both of copper, after having used a wire of zinc on the discs—they will still exhibit the same colored bands as the brass, until all the zinc left by the friction is removed, when the characteristic bands of the copper alone appear.

When silver and copper are used the bands are the same as with the silver alone, with the addition of two bands in the green, and so with any two metals, the bands are the same that both the metals exhibit when used separately, and the number of bands in the two metals will be equal to the number in both, except where they have bands that correspond in situation, in which case the bands of the two metals are blended together, producing bands of greater brilliancy. A spark between charcoal points does not show any peculiarity. The orange band appears to be common to all the metals tried—but I have not determined whether it occupies the same situation in the spectrum in all cases. The light of the spectrum not collected into the several bands, differs in intensity with the different metals—that with tin, iron and antimony being strong, while that of silver, copper and zinc, is faint.

Art. VII.—*Chinese and Aztec Plumagery;* by D. J. Macgowan, M.D.

Those natives of Northeastern Asia who in modern times have been drifted to the opposite shores of the Pacific were generally fishermen, mariners, or persons unacquainted with mechanical operations, and it is altogether probable that from the period of the first disaster by which they were driven to America, to that of the last shipwreck on that coast, very few artizans, and no scholars have in this manner changed continents; nor, judging from the low state of civilization of the more northern peoples, could it be expected that adventurers by the way of Behring's Straits or the Aleutian islands would carry with them a knowledge of any arts but the most simple and rudimentary. Hence, we shall look in vain for many resemblances in the industrial operations of the dwellers on the eastern and western coasts of the great ocean. Nevertheless, there is reason to believe that were we better acquainted with the state of arts amongst those farthest advanced in civilization in Polynesia and America, we should recognize modes of operation identical with those of China too numerous to be accounted for either as coincidences, or as independent inventions. A striking illustration is furnished by Capt. Wilkes,* who gives a drawing and description of an instrument for drilling holes, which he found in use by the inhabitants of Fakaafo or Bowditch island. This is undoubtedly a Chinese implement, being the most ingenious of all their tools. It is employed for perforating small holes by all workers in metals, but appears to most advantage in the hands of needle makers, who use it for drilling eyes in the small wires of which these are made.

* Narrative of the U. S. Exploring Expedition, by Capt. Wilkes, U. S. N., vol. v, p. 17.

Whether plumagery or the art of working in feathers, which was formerly practised in this part of the world, and also by the Aztecs of Central America, originated with Asiatics, or Americans, or with both, must be left to conjecture: in any view of the case, the fact is invested with interest. Attention was attracted to this subject by perusing the chapter devoted to an inquiry into Aztec civilization in Prescott's History of the Conquest of Mexico, where the distinguished historian shows that the ancient Mexicans excelled in the arts of plumagery and jewelry, in both of which they appear to have followed the same methods that are adopted by the Chinese.

Confucius informs us that in remote antiquity, ere the art of weaving silk or hemp was understood, mankind were clothed with the skins of beasts and feathers. How the latter were held together is not stated, but it must have been in a rude manner by cords or thread: at a later period feathers were in general demand as ornaments to banners and articles of attire; and subsequently for the manufacture of door-screens and caps. Tradition states that garments made of feathers and resembling fur dresses were presented to the emperor Shauhau, who reigned twenty-five centuries before our era. The earliest allusion to robes *woven* with feathers, occurs in the history of the Tsin dynasty. In the year 272 A. D., Dr. Ching, the court physican, presented the emperor with a gown made of feathers from the golden-headed pheasant. His Majesty being the founder of a new dynasty, was anxious to induce economical habits among his subjects; he therefore immediately ordered the splendid garment to be publicly burnt before the palace door, and issued on the following day stringent prohibitions against the presentation of articles of luxury.

The emperor Wuti, who flourished in the latter part of the fifth century, had a son who was notorious for his extravagance, having among other costly articles, a robe woven with peacocks' feathers. History further informs us that it was the custom of emperors to make presents every eleventh month of robes made out of the feathers of the variegated king-fisher to certain ministers of state. Taitsung, A. D. 976, changed the custom so far as to substitute silk for plumagery. Again, at a later period, the imperial records relate that the princess Ganluh engaged a skillful artificer to collect feathers of every description, to make of them two dresses, which should when looked at in front present one color, when viewed sideways another, and when held up to the light a third. When completed, she presented them to the empress, and they were so much admired that the fabric became very fashionable among officers and people, so much so that the hills and forests were swept clean of down and feathers, and vast numbers of birds were ensnared for their plumage.

More instances might be adduced to show that at different periods extending through many centuries plumagery was well understood. Garments thus manufactured were necessarily rare, their use being confined to persons of rank and wealth, and it may be doubted if even among the Aztecs whose country unlike China had vast forests crowded by the feathered tribe, the material was so abundant as to allow the inhabitants generally to shine in borrowed plumes.

The foregoing examples, drawn from the popular encyclopedias, throw no light on the mode of manufacturing this elegant material. Something however may be gathered on this subject from a work styled "New Conversations on things seen and heard at Canton," by a native of Suchau who spent many years in that city in a mercantile capacity in the latter part of the last century. In a short section devoted to *Bird Clothes*, he says, "There are several kinds of birds, the feathers of which are woven into a peculiar cloth by the southern barbarians. Among them is the celestial goose velvet, the foundation of the fabric being of silk, into which the feathers were ingeniously and skillfully interwoven, on a common loom, those of a crimson hue being the most expensive. Of these wild goose feathers, two kinds of cloth were made; one for winter, the other for summer wear. Rain could not moisten them; they were called 'rain satin,' and 'rain gauze' respectively. Canton men imitated the manufacture, employing feathers of the common goose, blending them with cloth. This fabric though inferior in quality was much cheaper. Goods of the same description were also brought from Hohlih [a state described by geographers as being adjacent to Samarcand, perhaps Bokhara] made of birds' feathers; they were twilled, the crimson colored being most valued. The article was too heavy for garments. The Cantonese also learned to imitate this, making it like plain silk, and inferior to that from abroad. Peacock feathers are employed by Canton manufacturers in making variegated threads which are used in making beautiful capes for females." Another writer states that a tribe of the Miaut, in Kwangsi, manufacture clothes from the fine down taken from the abdomen of geese. The down and tufts of birds were probably the materials which were woven into textile-like fabrics.

From the above, it would appear that the Chinese have lost the art of weaving feathers. Plumagery is still practised, however, in the decoration of metallic ornaments worn by all classes of females, chiefly on the head. When silver is employed, the article is first coated with gold. The gaudy lustre of the gilt is softened by laying over portions of it a covering of blue feathers representing flowers, insects, birds, and the like, which imparts indescribable beauty to the silversmith's elaborate filagrees. The art appears to most advantage as practised by artificers whose occupation is

the manufacture of garlands, chaplets, frontals, tiaras and crowns of very thin copper, on which purple, dark and light blue feathers of gorgeous brilliancy are laid with exquisite taste and skill. From the size of these ornaments great scope is afforded for the display of various figures. Sometimes two dragons extend from below the lobes of the ears, meeting above the forehead, the variegated scales of which are represented by minute portions of feathers of various hues; at others, beautiful flowers are interspersed with elegant mosaic, and then again the head attire appears animated, as with every turn of the fair one, tiny genii, birds, and insects are set in motion from springs and wires which retain them in the midst of the fairy-like garland. A more tasteful, elegant, or gorgeous blending of art and nature than is exhibited in one of these head dresses, perhaps no ingenuity has hitherto devised. To increase the effect, these ornaments are studded with pearls, produced cheaply and in great abundance by artificial means in a fresh water muscle. Commoner articles, such as earrings, and brooches for caps, are generally made of a small wreath of the forget-me-not, encircling one of these pearls. Half a dollar will purchase one of these when of silver, and a few cents the copper ones. The most expensive head dresses cost less than five dollars, unless of silver.

As this elegant art has not hitherto attracted the attention of foreigners, the mode of procedure should be described; this may be done in few words.

On the table at which the workman sits, he has a fasciculus of feathers, a small furnace with a few embers for keeping warm a cup of glue, a small cutting instrument like a screw-driver, a pencil or brush, and the articles, either silver, gilt, copper tinsel, or pasteboard which are to be feathered. The thumb and index finger being smeared with glue, the feathers are gently drawn between them, which stiffens the barbs, causing them to adhere firmly together; and when dry the perpendicular blade is drawn close to the shaft dividing it from the barbed portion. Holding this cutting implement as in writing *à la Chinoise*, the artist by pressing on the strips of barb with the knife, cuts them into the desired size and shape, which is a work of some delicacy—the pieces being very small, in the form of petals, scales, diamonds, squares, and the like, and requiring to be of the same size as the particular spot on which they are to be laid. Besides fingering this tool in the manner described, he holds the pencil nearly as we do a pen, dips it into the glue, brushes the spot to be coated, then expertly reversing it, touches with its opposite point a tiny bit of feather, which is thus lifted up and laid on the part for which it was fitted. Care is requisite also in giving a proper direction to this twilled work, for such of course is the appearance presented by the barbs.

The feathers most in demand for this purpose are from a beautiful species of *Alcedo*, brought from the tropical regions of Asia; they are employed for silver articles. King-fishers of coarser plumage, and less brilliant hue, found throughout the country, are used for ornaments made of copper or pasteboard. Blue always greatly predominates over lighter or darker shades, relieved by purple, white or yellow.

Whether Aztec silversmiths, whose ingenuity is so much lauded by the old Spanish chroniclers, practised this branch of plumagery or not is uncertain. There is reason to believe that what is said of their imitation of animals with moveable wings or limbs, of their representing scales of fish alternately of gold and silver, were nothing more than what is now done by the same craft in China; and which were esteemed very marvellous by Sir J. Mandeville (Voiage and Travaile), motion being communicated by wires and springs, and colors imparted by the plumage of choice birds. The construction of automata proper requires a knowledge of horological mechanism, never attained by either Chinese or Aztecs. Plumagery doubtless attained a far higher degree of excellence in Central America than in China, owing to the greater variety and extreme abundance of the feathered tribe in the dense and luxuriant forests of Mexico. Yet it is not likely that feather fabrics were so easily manufactured as to be worn by other than the nobles and affluent, to whom in China their use was confined. Had the Chinese been as destitute of textile fabrics as the Aztecs, they would unquestionably have engaged in plumagery with greater success.

Art. VIII.—*Binocular Microscope;* by Dr. E. D. North.

Common stereoscopy is a full perception of the solid forms of objects by a peculiar mode of viewing their geometric projections on a flat surface. Flat pictures are thus made to represent objects with a relief as perfect as that of the objects themselves, and, what is more than relief, give us the perception of *looking around and almost behind* each cylindrical or spheroidal form. Not only is a bust or statue thus seen, but several figures in a group stand forward at different distances from each other, while the clear space between is fully perceived and affords an especial gratification. In strict imitation of ordinary vision with both eyes, two drawings or photographs at the same distance are seen and are united by the brain, as if both eyes were looking at a single solid object without the intervention of an optical instrument. The success of the stereoscope may be pronounced perfect; the view by means of two drawings affords the same amount

of knowledge, and with as much accuracy and satisfaction as a view of a real object with both eyes.

At first thought it seems strange that the stereoscope was not sooner invented. A full detail of the reasons why in the progress of the human mind this invention is so late, and how it is that we are able to do without it, would open up a wide field of interesting considerations, and include the philosophical and aesthetical principles of the art of painting.* It will be sufficient to hint at these uses of vision by which the eye is the mind's instrument for mathematical or strictly scientific perception and knowledge. Such knowledge depends on *measurement;* solids are analyzed and measured by means of planes; the third dimension is in a plane at right angles to that of the length and breadth; it is length or breadth in another direction. In judging of length or breadth by the eye, we measure by a process of halving and repeated bisection, and make the eye a mathematical instrument; but in the case of an actual solid whose position we can change, if we would obtain the same accuracy of knowledge in regard to the third dimension, we must *turn the object over* until depth becomes length or breadth. If the object cannot be turned over, the dimension which, as it lies, is that of depth, cannot be measured accurately by the eye, and cannot, so to speak, be mathematically perceived; hence stereoscopic vision, either when it is natural, as being the simultaneous act of both eyes or as that obtained by successive focal changes of a single eye, or when it is effected by suitable optical instruments, cannot be what we call mathematical vision or that which is attended with measurement.

But optical instruments are useful not only for their assistance in measurement or strict science, but also as adjuvants of perception, and in obtaining an endless amount of less definite knowledge which is needed in the mixed sciences. Facility, rapidity, satisfaction and pleasure are also important considerations. This is especially true of the microscope, an instrument as essentially necessary and indispensable in science as the telescope, and equally accurate and reliable with it in all its revelations.

Inexperienced persons are embarrassed, when judging of objects under the microscope, by their appearing as flat pictures; by an inability to perceive depth, and by the care and slowness with which the mind must be employed when the focus is

* When the utmost possible minuteness and accuracy of discrimination is needed by a watch-maker, engraver, by any extremely accurate mechanic, or by a naturalist or other scientific man, but one eye is employed. Spectacles which are lenses of low power for both eyes, are used solely because an individual's eyes do not see at certain average distances as well as mankind's in general—never strictly speaking for magnifying an object too small for good eyes. First the closest inspection by a single eye is employed and at the shortest working distance of the eye; if the object be still too minute, a single hand lens, and never a pair of them, are resorted to. Attempts have been made to introduce watchmaker's magnifiers in spectacle frames; yet they have not come into use.

adapted to different planes; while in the common stereoscope the impression of a solid object is, as we have before said, absolutely perfect; so that when an object is deeper than it is long or broad it will infallibly appear so, and we are enabled even to measure the depth, to a certain extent, by comparing it with the length or breadth as a unit.

Professor Riddell has practically succeeded in adding to the previous powers of the microscope all those of the other instrument now so common and popular; and with this great superiority, that whereas the latter can exhibit only pictures and those of large objects, being thus dependent on the accuracy of the drawing, or the success of the photograph, his binocular microscope exhibits minute objects themselves with perfect accuracy and truth as they exist in nature; presenting them as solids in which the dimension of depth or thickness and their distance in superposition, is of as much importance to truth of perception as that of length or breadth. Fortunate in being able to commit his plans to a truly scientific and accurate workman, one to be depended on for resource and avoidance of error in the minute details necessary for applying a general idea, he has received from Mr. Grunow an instrument of great beauty of workmanship and convenience, the performance of which is most satisfactory in reference to the objects aimed at.

For a full description of the optical arrangement by which binocular vision is attained, we refer the reader to Prof. Riddell's article on the "Binocular Microscope," read before the American Association for the Advancement of Science, July, 1853, and published in No. 5, of the Quarterly Journal of Microscopical Science.

As far as we have been able to observe, this arrangement is not liable, in any degree, to the charge of pseudoscopy, nor is any perceptible chromatic or spheroidal aberration produced by the interposition of prisms, which is sufficiently proved by the examination of minute mercury-globules.

The mechanical arrangement is excellent. The instrument is mounted on a solid, and very steady tripod stand of cast-iron, with a plain stage 4×6 inches. The focussing is performed by a carefully wrought rack and pinion movement with two large milled heads.

The necessary movements for adapting the distance of the eye-lenses to any pair of eyes and for adjusting the prisms, so as to cause the rays from the objective, after being divided, to emerge coincident with the axes of the two compound bodies, are also performed by rack and pinion, and are so arranged that they act equally and at the same time on both the right and left side. The image is inverted in one direction, and two small rect. prisms placed above the eye-pieces, make it appear in its natural position and thus adapt the instrument for dissecting.

As the Professor possesses several higher objectives, by Mr. Spencer, he has had executed by Mr. Grunow, at the same time with the Binocular, only an object-glass of 1 inch focus with which alone we have had an opportunity of trying the performance.

This object-glass has a large angle and is corrected exquisitely for chromatic and spherical aberration. That difficult point in microscopy, a view into deep cavities, is perfectly attained. The eye of a fine needle by incident light, exhibited the walls of a cavity deeper than broad; an opened anther cell preserved in fluid and happening to lie edgewise, exhibited by transmitted light its walls with clear vision to the bottom of the depth; the fibres of the fringed end of a silk ribbon floated in space in different planes of superposition with an enlargement of distance in the perpendicular direction strikingly correspondent to their horizontal separation. No doubling or thickening of lines, mistiness, fog or uncertainty accompanied the views, but vision was brilliant and clear, while thus looking into space instead of being as it were confined by an optical wall of limitation, to a single plane. Neither while thus looking far into space by magnifying distance in proportion to surface, was any thing lost in power of minute discrimination of lines and edges; these were sharply and clearly defined: in short true shape and form were perceived in their actual proportions instead of being flattened, and an entire whole seen, as Schacht expresses it, "in optical sections;" and were exhibited simultaneously with minuteness of detail on surfaces.

Thus may we congratulate ourselves that binocular vision, so long a desideratum in microscopical science, is at length attained; constituting, as it does, the first important step since the solution of the great problem of the achromatising of the microscope, and the power of enormously enlarging the aperture of the objective.

New Haven, March, 1854.

Art. IX.—*Mechanical Action of Heat;* by W. J. Macquorn Rankine.

Gentlemen—I beg leave to address to you the following remarks on a formula referred to in the very able and interesting paper of Professor Frederick A. P. Barnard on "Heated Air considered as a Motive Power," published in the American Journal of Science for March.

The formula in question represents the maximum efficiency of a perfect Thermo-dynamic Engine: that is to say, the greatest fractional portion of the total heat consumed which such an engine converts into motive power; and, in Prof. Barnard's notation, it is as follows:

Let H represent the mechanical equivalent of total heat expended;
W, the motive power developed;
τ'', the absolute temperature at which heat is received by the elastic substance which works the engine;
$\tau_{,}$, the absolute temperature at which heat is given out;
Then the efficiency of the engine is

$$\frac{W}{H}=\frac{\tau''-\tau_{,}}{\tau''} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (A.)$$

This formula is ascribed by Professor Barnard to Professor William Thomson of Glasgow.

The formula originally proposed by Professor Thomson is, however somewhat different in form from the above, being the following:

$$\frac{W}{H}=1-\varepsilon^{-\frac{1}{J}\int_{\tau_{,}}^{\tau''}\mu\, d\tau}; \quad . \quad . \quad . \quad . \quad (B.)$$

where ε is the base of Napier's logarithms; J, "Joule's Equivalent" = 1390 foot-pounds per centigrade degree in liquid water; and μ, "Carnôt's Function;" being an unknown function of temperature only, which has to be determined by experiment.

This formula was deduced by Professor Thomson from a combination of Carnôt's principle, *that the efficiency of a perfect thermodynamic engine is a function solely of the temperatures at which it receives and emits heat*, with the law established experimentally by Joule, of the convertibility of the different forms of physical energy. It appeared in a paper on the Dynamical Theory of Heat, read to the Royal Society of Edinburgh in 1851, published in the 20th volume of their Transactions, and reprinted in the London, Edinburgh and Dublin Philosophical Magazine for 1852, Series 4th, volume 4th.

During the same session of the Royal Society of Edinburgh, there was read the Fifth Section of my paper on the Mechanical Action of Heat, in which, from the hypothesis that heat consists in certain molecular oscillations or revolutions, I deduced the law, *that the efficiency of a perfect thermodynamic engine is expressed by the difference between the absolute temperatures at which it receives and emits heat, divided by the greater of those absolute temperatures diminished by a constant which is the same for all substances;* that is to say,

$$\frac{W}{H}=\frac{\tau''-\tau_{,}}{\tau''-\kappa}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (C.)$$

The constant κ, if not absolutely inappreciable, is so small that no material error in practice can arise from neglecting it in com-

puting the efficiency of engines. This reduces the formula C to identity with A.

The section to which I have referred was published in the 20th volume of the Transactions of the Royal Society of Edinburgh, and re-printed in the London, Edinburgh and Dublin Philosophical Magazine for March, 1854.

Professor Thomson afterwards pointed out, (in consequence of a suggestion by Mr. Joule) that if Carnôt's Function be supposed to have the following value

$$\mu = \frac{J}{\tau} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \qquad \text{(D.)}$$

the formula B is transformed into A.

It is also obvious, that if Carnôt's Function have the value

$$\mu = \frac{J}{\tau - \varkappa} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \qquad \text{(E.)}$$

B becomes identical with C.

Although the law expressed by the equation C, being partly founded on hypothetical principles, was at first to a certain extent conjectural, yet it has subsequently being so closely confirmed by the experiments of Messrs. Regnault, Joule, and Thomson, that it may be regarded as almost, if not altogether, demonstrated. It is still, however, uncertain, whether the constant $\varkappa$ has an appreciable value. The values computed from the experiments range from 0° to 2°·1 Centigrade; absolute temperatures being reckoned from a point 274°·6 Centigrade below melting ice, or 494°·28 below zero on Fahrenheit's scale. The constant $\varkappa$ represents the position, on the scale of a perfect-gas thermometer, of the point of total privation of heat. If the elasticity of a perfect gas arises wholly from heat, then this point coincides with the absolute zero of a perfect gas thermometer, and $\varkappa = 0$.

It gives me much gratification to find, that the conclusion to which Mr. Joule, Professor Thomson, and myself have been led by our researches, as to the great economy of fuel to be expected from the Air-Engine when its practical difficulties have been overcome, is confirmed by the opinion of an investigator who has so carefully examined the subject as Professor Barnard.

59 St. Vincent street, Glasgow, 14th April, 1854.

ART. X.—*On the Resistance experienced by Bodies falling through the Atmosphere;* by ELIAS LOOMIS, Professor of Mathematics and Natural Philosophy in New York University.

AT the Cleveland meeting of the American Association, I presented a paper on the hail storm of July 1st, 1853, and introduced some computations for the purpose of determining the velocity which hail stones acquire in falling through the atmosphere. These results were based upon the experiments of Hutton respecting the air's resistance to bodies in motion, as determined by a whirling machine. Since the case of a body revolving about a fixed axis is different from that of a body descending freely through the atmosphere under the action of gravity, I have endeavored to test these results by experiments upon the direct fall of bodies. For this purpose I have performed various experiments upon the velocity acquired by falling drops of water; also by small spheres made of cork; and have experimented with lumps of ice varying from the size of a pigeon's egg up to masses weighing more than two pounds. These results coincided tolerably well with those obtained by computation from Hutton's data, but I refrain from publishing them at present in the hope of being able to repeat them with greater care and with the advantage of a greater elevation.

In the mean time I have sought for experiments of a similar kind made by other individuals. The experiments made at the request of Newton in St. Paul's Cathedral at London, seemed better suited to my purpose than any others I have found. There were two series of these experiments. In the first series, made in the year 1710, several hollow glass globes of about five inches in diameter were let fall from an elevation of 220 English feet, and the times of descent carefully measured. In the second series of experiments made in the year 1719, several bladders formed into spheres about five inches in diameter, were let fall from a height of 272 feet, and the times of descent carefully observed.

For the purpose of deducing from these experiments the coefficient of resistance, I proceeded in the following manner. It is evident that the resistance to a falling body beginning from zero, continually increases with the increasing velocity of the body; and since the impelling force is constantly the same, while the resisting force always increases, it must happen that the latter will at length become equal to the former. When this result takes place, that is, when the resistance is just equal to the weight of the body, there can be no further increase of velocity, and the body must thenceforth descend with a uniform motion. I found that in the first series of Newton's experiments, the velocity of

descent became sensibly uniform after a fall of 40 feet; and in the second series of experiments after a fall of 10 feet. I carefully computed the time of descent through the space just mentioned, and dividing the remaining distance by the remaining time of descent, obtained the terminal velocity, from which the coefficient of resistance is easily deduced.

In these computations I made use of Hutton's formulæ which are as follows:

$$\frac{4gcx}{w} = \text{h. log.} \frac{w}{w - cv^2} = \text{h. log. N.}$$

$$v = \sqrt{\frac{N-1}{Nc} \cdot w}$$

$$t = \frac{1}{4g}\sqrt{\frac{w}{c}} \times \text{h. log.} \frac{\sqrt{\frac{w}{c}} + v}{\sqrt{\frac{w}{c}} - v}.$$

$$c = \frac{w}{v'^2}$$

Where

$c =$ the coefficient of resistance,
$g = 16\frac{1}{12}$ feet,
$w =$ the weight of the body expressed in ounces,
$x =$ the space fallen through,
$v =$ the velocity acquired in falling through the space x,
$t =$ the time of descent through the space x,
$v' =$ the terminal velocity of the body.

The following Table shows the results deduced from the first series of experiments with glass globes. Column first shows the weight of the globes in grains; column second shows their diameters in inches; column third shows the entire time of falling from a height of 220 feet; column fourth shows the velocity acquired in falling through a space of 40 feet; column fifth shows the time of falling 40 feet; column sixth shows the coefficient of resistance deduced from the time of descent; and column seventh shows the same coefficient reduced to a sphere of 5 inches in diameter by assuming the resistance to vary as the square of the diameter.

Weights of the globes.	Diamet'rs of the globes.	Whole times of falling.	Velocity in falling 40 ft.	Time of falling 40 feet.	Coefficient of resistance.	Do. reduced to a sphere of 5 in.
grains.	inches.	s.		s.		
510	5·1	8·2	28·237	1·9967	·0013845	·0013307
642	5·2	7·7	30·026	1·9386	·0015033	·0013899
599	5·1	7·7	30·070	1·9373	·0014033	·0013488
515	5·	7·95	29·183	1·9650	·0013014	·0013014
483	5·	8·2	28·383	1·9917	·0013133	·0013133
641	5·2	7·9	30·099	1·9364	·0015022	·0013888

In making these computations I was obliged to assume a probable value of c for the purpose of computing v and t; but the value of c deduced from the formula $c=\frac{w}{v'^2}$ is but slightly affected by an error in the first assumed value of c. In order however to eliminate even this small influence, having computed the value of c by these formulæ, I repeated the computation for v and t with the value of c thus deduced, by which means I obtained a second determination of c which is almost wholly independent of the first assumed value.

The mean of the preceding values of c for a sphere of 5 inches in diameter is ·0013455; whence for a velocity of 30 feet, which is about the terminal velocity in the preceding experiments, the resistance on a sphere 5 inches in diameter is 1·2109 ounces.

The following Table shows the results deduced from the second series of experiments with bladders, arranged as in the former table. The entire space fallen through was 272 feet. Columns four and five were computed for a descent of 10 feet.

Weights of the bladders.	Diamet'rs of the bladders	Whole times of falling.	Velocity in falling 10 ft.	Time of falling 10 feet.	Coefficient of resistance.	Do. reduced to a sphere of 5 in.
grains.	inches.	s.		s.		
128	5·28	19	14·200	0·9955	·0013816	·0012390
156	5·19	17	15·581	0·9528	·0013376	·0012415
137·5	5·3	18·5	14·539	0·9844	·0014047	·0012501
97·5	5·26	22	12·375	1·0690	·0014223	·0012852
99·125	5·	21·125	12·911	1·0450	·0013308	·0013308

The mean of the preceding values of c for a sphere of 5 inches in diameter is ·0012693, whence for a velocity of 15 feet, which is about the terminal velocity in the preceding experiments, the resistance on a sphere 5 inches in diameter is ·2856 ounces.

In the year 1802, a great number of experiments on falling bodies were made by Benzenberg in the tower of St. Michael's church at Hamburg. Metallic balls about one and a half inches in diameter were allowed to fall from heights varying from 25 to 340 French feet, and the times of descent were measured by a watch having a hand which made one revolution per second. These experiments were performed with the greatest care; but since the specific gravity of the balls employed was more than ten times that of water, they do not appear to me as well adapted to indicate the amount of resistance, especially for small velocities, as the experiments of Newton.

Newton's experiments have furnished us the resistance on a sphere 5 inches in diameter, at the two velocities of 15 and 30 feet per second. We will now compare these results with those obtained by Hutton with the whirling machine. Hutton determined the resistance upon a sphere of pasteboard 6⅜ inches in diameter, for velocities from 3 to 20 feet per second. I have reduced these results to a sphere 5 inches in diameter by assuming

the resistance on spheres to vary as the squares of their diameters. I have also added the resistance to a velocity of 30 feet per second, computed upon the assumption that the resistance varies as the square of the velocity.

In the following Table, column first shows the velocity in feet per second; column second shows the resistance to a sphere 5 inches in diameter, moving with velocities expressed in the first column, deduced from Hutton's experiments with the whirling machine; column third exhibits the same numbers corrected so as to conform to the results of Newton's experiments.

Velocity per second.	Resistance from Hutton's experiments.	Do. from Newton's experiments.	Velocity per second.	Resistance from Hutton's experiments.	Do. from Newton's experiments.	Velocity per second.	Resistance from Hutton's experiments.	Do. from Newton's experiments.	Velocity per second.	Resistance from Hutton's experiments.	Do. from Newton's experiments.
feet.	ounces.	ounces.	feet.	ounces.	ounces.	feet.	ounces.	ounces.	feet.	ounces.	ounces.
3	·017	·014	8	·100	·080	13	·268	·214	18	·523	·417
4	·029	·023	9	·127	·102	14	·311	·249	19	·584	·467
5	·042	·034	10	·157	·126	15	·357	·286	20	·650	·520
6	·058	·047	11	·191	·153	16	·408	·326	30	1·487	1·211
7	·077	·062	12	·228	·182	17	·463	·370			

The resistances deduced from Newton's experiments at velocities of 15 and 30 feet per second are about one-fifth less than those obtained by Hutton. Diminishing the numbers in column second by one fifth part we obtain the numbers in column third which are presumed to represent the resistance in conformity with Newton's experiments.

If now we assume the specific gravity of ice to be 0·865 as given in the French Annuaire, the weight of spheres of ice 2, 1 and ½ inches in diameter will be as follows; and the greatest velocities which they can acquire by falling through the atmosphere are shown in the following Table.

Diameter of sphere.	Weight of sphere.	Terminal velocity.
2 inches.	2·0908 ounces.	98 feet.
1 "	0·2614 "	70 "
½ "	0·0327 "	49 "

Art. XI.—*The Brandon Tornado of January* 20*th*, 1854; by O. N. Stoddard, Prof. Chem. and Nat. Phil., Miami University, Ohio.

The whole breadth of the State of Ohio from Southwest to Northeast, was swept on the 20th of January, 1854, by a storm of unusual violence.

Traces of the same storm have been obtained from a point 27 miles N. E. of Little Rock, Arkansas, also from the western part of Pennsylvania. The whole length cannot be less than 800

miles. The breadth, I have not been able to determine. At Dubuque, Iowa, on the 20th of January, it was clear and very cold, with the wind from the N. W. At the point named in Arkansas, heavy rains from the southwest occurred on the 19th, followed by a clear and cold atmosphere on the morning of the 20th. On this day, the 20th, the storm passed over Ohio.

The temperature became mild on the 19th, and on the next day at noon, the thermometer stood at 70° in Cincinnati, and 68° in Oxford; the latter place more elevated than Cincinnati and about 30 miles from it, N. by W. The barometer fell gradually during the 19th, and rapidly on the 20th; and at 45 minutes past 12 M. the time when the storm began at Oxford, it stood 28·21; lower than at any period during the last twelve months. The air was saturated with vapor, and the walls of brick buildings were dripping with moisture. Three strata of clouds were distinctly observed: the highest cirri light and fleecy, moving towards the N. E.; the second, the proper storm-cloud in dark heavy masses, moving rapidly in the same direction; the third and lowest, the scud of sailors, flitting violently past, a little east of North. Along the track of this wind, there were at different times during the day, violent rains, vivid lightning, heavy thunder; and in some places large hailstones fell, though not in great quantity. In the northeastern part of the state the storm assumed the form of a tornado of great violence.

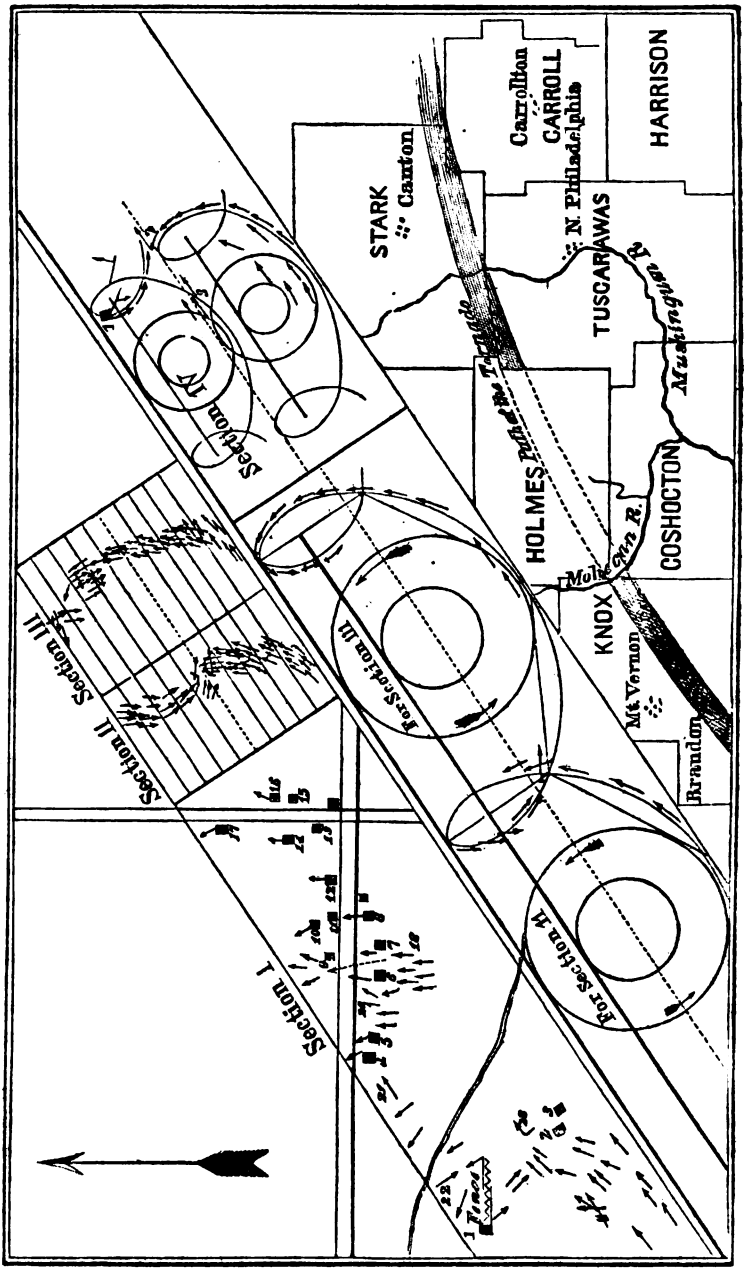
It first struck the earth in the S. W. part of Miller township, Knox County; N. Latitude 40° 18′, and Longitude 5° 30′ West of Washington.

Its course in that county was N. 56¼° E. Traces of it are found in some of the counties farther east; where its path gradually curved more towards the east; presenting its convex side to the north. The tornado in Washington Co. Pa., on the same day, was not probably a continuation of that in Ohio, as its location was several miles farther south. The track pursued in this state is given in the accompanying diagram.

It appears to have passed over one tier of counties without touching the earth, and subsequently to have descended again with its original force. The dotted lines exhibit its supposed path above the earth; the shaded part, the localities of its ravages on the surface.

The examination by the writer commenced one and one-fourth mile southwest of Brandon, at Dr. Baxter's barn, (marked 1, see diagram) the north gable end of which was crushed in, and the eastern half of the roof taken off and scattered to the S. East.

On the right were a barn and house (2, 3,) of E. Coleman, both of which were unroofed, and a rafter from the barn was driven through the side of the house and into a chest standing within.


STARK
Canton
Carrollton
CARROLL
N. Philadelphia
HARRISON
TUSCARAWAS
Muskingum R.
HOLMES
Path of the Tornado
COSHOCTON
Mohican R.
KNOX
Mt Vernon
Brandon
Section IV
Section III
Section II
Section I
For Section III
For Section II
Fence

The tornado then crossed the valley of Sycamore creek, and ascending a gentle slope on the eastern side, struck Dr. Wheaton's house and barn, (4, 5,) which were utterly demolished. A brick church (6), a brick school-house (7), and a log house (8), were entirely swept from their foundations. A Presbyterian Church (9) was unroofed; a small frame house (11) had the roof and ceiling raised but not thrown off; an out-house (10) was destroyed; a stable (12) was also unroofed; a large frame house (13) was moved 18 inches from its foundations; a stable (14) carried 12 feet; a blacksmith and wagon shop (15, 16) and E. Squire's brick house (17) were prostrated. Some small buildings on the right, not represented, were unroofed or otherwise injured. About one-fourth of a mile east of Brandon it struck a dense forest. At this point a careful survey was made across the track; represented by Section II. For nearly three miles its course was mainly through the forest, with intervals of cleared land, uprooting or breaking almost every tree, and crushing the buildings which unfortunately stood in its way. Crossing the Newark and Mt. Vernon railroad, it swept over cultivated fields, destroying the few trees which had been left, and razing to the ground a stable and brick house. Three-fourths of a mile beyond this, an open grove of very large trees, mostly oak, stood on rising ground and in the line of the storm's axis. They seemed like an advanced guard to the forest a little farther in advance. The tornado struck them with appalling fury, and appeared well nigh irresistible. Scarcely one was left standing; some were uprooted, others broken, and split into fragments.

Near this place where it entered the forest, another survey was made (see Section III). In this survey, as well as the other explorations made during this day, the writer was aided by Rev. R. C. Colmery, of Mt. Vernon. A part of the forest here passed through was heavily timbered and covered with a dense undergrowth. Though the action of the wind was less symmetrical while struggling through and entangled among so many obstacles, yet the renewal of the velocity as fast as destroyed, and the force with which it plunged down and clung to the earth, were exceedingly interesting features of its workings.

Its path was followed several miles farther, in all about eleven miles, and occasional bearings taken, but as they corresponded entirely with the previous ones, no special record was made of them. The breadth of the track became somewhat less, but without any decrease in violence. Houses included in its vortex were still demolished; a horse was lifted into the air and carried over a fence; a cow was blown twelve rods against a tree, striking it twelve feet from the ground. In the vicinity of Gambier, after a course of twelve miles, its destructive influence was for a while suspended, till it again struck the earth in another county.

Having made these general statements, we may now examine more particularly the bearings of objects prostrated by the wind. The following table contains the bearings of a survey across the path of the tornado, 1¼ mile S. West of Brandon, commencing on the right. The plot of these bearings will be found in the left part of Section I.

	Course of the storm,	N. 56¼° E.	
1.	A tree, - - -	N. 56¼° E.	
2.	" - - -	N. 45° E.	
3.	" - - -	N. 56¼° E.	
4.	" - - -	N. 22½° E.	
5.	Several trees, -	N. 45° E.	
6.	A tree, - - -	N. 22° E.	
7.	" overlying 6,	N.	
8.	" - - -	N.	
9.	" across 8, -	N. 22° E.	
10.	" across 9, -	N. 56¼° E.	
11.	A fence, - -	N. 6° W.	This was near the middle of the track.
12.	A tree, - - -	N. 11¼° W.	
13.	A hickory, diagram (23)	N. 45° W.	The top after falling broken off and turned round to the East.
14.	Three oaks, - -	N. 45° E.	Near the hickory, but much smaller.
15.	Trees in orchard, -	E. 10° S.	
16.	Roof of Baxter's barn,	E. 45° S.	

A fence at this point, running east from Baxter's barn, was found to have been thrown along the western half towards the south, while the eastern half was prostrated northerly. This prostration of the halves of the fence in opposite directions, together with other peculiarities in the position of objects, induced the writer to believe that a secondary or double whirl existed at this place, the one on the right being in advance of the other.

The hickory was thrown down (Section 4, No. 2) across a ravine, and afterwards half of the tree, including the top, was turned round towards the east. The same current which produced this effect, prostrated three small oaks (3). The shingles from Baxter's barn (1) were first carried southeast, and then strewed for fifty rods in a curved line gradually bending in towards the course of the storm. The curved arrows from the barn correctly represent the arrangement of the shingles. Trees in an orchard near by were thrown down E. 10° S., coinciding in direction with the fragments from the barn.

The central whirl of the storm passing over the hickory and oaks, might account for their position, but not for that of the fence. The evidence from the latter of a secondary whirl, was as clear and explicit as well could be. Had the rails been removed by hand and laid on opposite sides, the result could not have been more regular. A secondary whirl about 60 rods in diameter, lagging a little behind the more extended one on the right, seems undeniable from the facts in the case.

To what extent this may have disturbed the general action, it is impossible to say. Its existence, if admitted, did not however

continue long, for all trace of it is lost in the forest one-fourth of a mile east of Brandon.

We will now examine briefly the action of the tornado at Brandon. The right hand part of this storm seems to have slid over that division of the town, merely prostrating fences and unroofing some small buildings. Its destructive effects were felt mainly in the middle and left hand portions.

A careful examination on the right of the path through the town, was not made; the approach of night cut it short: and the subsequent observations were given to a part of the track farther east. Section I represents that portion of the town most severely visited; and the arrows represent the direction in which objects were prostrated. The cluster of arrows (18) mark the position of trees in an orchard. Their direction was from N. to N. 10 W. Casting the eye from this point along the track of the storm, the buildings 7, 8, 12, 14, 15, 16, give the same general result. Combining them we obtain the mean N. 5° W. If we take another section farther to the left, commencing with the brick church (6) and extending nearly to Dr. Wheaton's house (5), we obtain a mean result of N. 15° 5′ W. From this estimate a tree (20) and the church (9) are rejected. No. 20 was an apple tree which had been twisted more than 45° after falling. The roof of the church was torn to fragments and scattered between N. E. and N. W. The direction in which the wind struck the church, can be made out with sufficient accuracy from other data. 1st, The south gable end was crushed in, and 2d, A bier was lifted from the grave yard, carried across the street along the line of the dotted arrow, and set down in the church yard.

The remaining objects on the left including Dr. Wheaton's house and barn, and the trees near the creek give a bearing of N. 33° 45′ W. From this 21 was rejected. It was a small oak, broken partly off, and turned round 90°. No. 22 was a limb from a tree carried S. 10° E.; supposed to be due to the reverse action of the storm. In the tabular view of these bearings given below, in order to fill it out for the right of the track, for which no materials existed in Brandon, the first three are interpolated from a survey farther west.

1.	N. 56¼° E.	5.	N. 15° 5′ W.
2.	N. 50° 37′ E.	6.	N. 33° 45′ W.
3.	N. 33° 45′ E.	7.	S. 10° E.
4.	N. 5° W.		

The bearings from this table are remarkable; and if the reader will construct a curve to suit them, he will find a singular correspondence with the cycloids which were planned with special reference to Sections II and III.

To these sections we will now turn. Section II represents a survey across the path of the storm, one-fourth mile east of Brandon, where it first struck the forest. The distances from bearing to bearing were not measured with a chain: it would have been impossible to use one in the midst of such a mass of fallen timber. They were determined, as nearly as could be, by the eye. On the right of the dotted line, the number of bearings might have been increased to any extent, as the ground was covered with fallen trees. The largest and straightest trees were selected. Those which were partially decayed or which had been thrown down by others were rejected. The arrows do not in every instance represent the relative distances of the trees from each other; in many cases they lie side by side in contact. On the left of the dotted line, they are sometimes placed nearer in the plot than was actually their position. The direction is accurately given. The arrows with cross-bars point out trees which lay across others. The prostrations appear to have been made almost entirely by the front of the tornado, with the exception of the left hand portion of the path, where the reverse action was frequently the most violent. This front action rendered the position of the trees more symmetrical, and the mode of action of the wind easier of solution. Objects thrown down at the first stroke of the current would accurately represent a tangent to the curve at that point, whatever the nature of the curve might be. As the trees which fell above others, must have been struck down after a part of the storm had passed, and cannot therefore exhibit the front action, they are on this account either rejected in obtaining the mean of the bearings, or included with others in the reverse action. The parallel lines were drawn to aid the eye in grouping the trees. Where the trees lie across them, they are included in that interval whose bearings most nearly correspond.

A tabular view of Section II, one-fourth mile east of Brandon,—½ mile wide.

Course of the storm,	N. 56½° E.
1 group, - -	N. 45° E.
2 " - -	N. 34° E.
3 " - -	N. 8° 53′ E.
4 " - -	N. 10° 7′ W.
5 " - -	N. 23° W.
6 " - -	E.
7 " - -	E. 34° S.
8 " - -	E. 45° S.
9 " - -	E. 78° S.
10 " - -	E. 67° S.
11 " - -	S.

A tabular view of Section III, six miles east of Brandon,—½ mile wide.

1 group, - -	N. 56° 15′ E.
2 " - -	N. 29° 24′ E.
3 " - -	N. 11° 15′ E.
4 " - -	N. 6° E.
5 " - -	N. 10° W.
6 " - -	N. 22° W.
7 " - -	N. 33° 45′ W.
8 " - -	N. 78° W.
9 " - -	W.
10 " - -	W. 45° S.
11 " - -	S.
12 " - -	S. 22° E.
13 " - -	S. 33° E.
14 " - -	S. 45° E.

The curtate cycloids were constructed to represent approximately these tabular views. The radius of the generating circle is to that which describes the curve as 1 : 2. The blanks in the

loop of either section could be supplied by interpolation from the other.

The barred arrows on the left of the loop in Section III designate trees which, though lying side by side with those on the opposite part of the same loop, yet rested with their limbs above the latter. The barred arrow in Section II which lies at the intersection of the curve was resting upon six large trees. An involute converging rapidly towards the centre might answer tolerably well the conditions of Section II, but it fails entirely when applied to Section III.

The involute action does not seem to have been distinctly marked. While passing rapidly over another part of the track, the bearings of scattering trees which were taken afforded more significant indications of the involute form near the axis. The relative distances in the latter case were not noted with sufficient accuracy to justify a projection.

The data thus far given are believed to be sufficient to enable the reader to form an intelligent opinion of the mode of action of this tornado. A few additional observations may afford some further aid.

1st. The space between the dotted line and the right hand border of the storm includes the path of most destructive violence. Within this limit almost every tree was prostrated or broken. This limit was clearly defined, especially on the left where the tornado first encountered the forest; but after plunging into it for some distance, its action became more obscure, and less symmetrical.

2d. On the left of the dotted line the force was much less violent, but trees enough were prostrated to determine the direction of the wind.

3d. No case was found of an object on the right thrown inward more than 11° or 12°. The mass of the trees on the right border lay parallel with the course of the storm.

4th. Along the dotted line, the trees generally lay N. 23° W.; making an angle of 79¼° with the general course of the storm.

5. Very few cases were observed of objects in the centre thrown forward in the direction of the path.

6th. There was no distinct case of the outward explosive action in buildings. The ascending current was, notwithstanding, very violent, for large objects were raised and transported several miles.

7th. Persons just outside of the path describe the storm as a column of vapor or smoke, whirling in indescribable confusion, accompanied with a deafening roar, so that the thunder, if any, was undistinguishable amid the general din and confusion.

8th. The atmosphere on the borders of the track appeared to suffer but little disturbance from the passage of the storm; and no current could be observed setting in towards it.

9th. There was scarcely any hail, but torrents of rain followed the tornado, equally, and perhaps, more abundant outside of the track than within it.

10th. The temperature sunk so rapidly that the next and succeeding days became marked as among the coldest in the month.

Rate of Progress.

Great discrepancy existed between different observers in reference to the progress of the storm. The estimated time of its passage over any one place varied from one-half to one and a half minutes. Mr. Coleman stated that he saw the whirling mass coming when three-fourths of a mile distant, and that he took scarcely five steps before he was struck down. If we take one minute as the time of its passage over any point, and three-fourths of a mile as the diameter of the storm, we obtain a velocity of 45 miles per hour.

I have not been able to obtain the barometric minima along the track of this extended S. West current; but if the lowest depression of the atmospheric wave passed near Little Rock, Arkansas, at noon on the 19th, then to have reached Oxford, Ohio, at 12½ P. M. on the 20th, would have required a velocity of nearly 29 miles per hour. My own opinion is that the first estimate is nearer the truth. The clouds during the forenoon of the 20th, flew past with a velocity which attracted special attention.*

In reference to the velocity of rotation an approximate estimate only can be reached. If the ratio of the progressive and rotary velocities adopted in the construction of the cycloid be correct, and 40 miles per hour be taken as the rate of progress, then the velocity of the wind on the right would have been 120 miles per hour. This velocity would be increased, especially near the axis, by the involute form of the curve; but to what extent this operated cannot be stated.

Another mode of estimating the force of the wind may be adopted. Among the oaks previously named as standing on rising ground, was one, a giant among giants. Its trunk was three feet in diameter and straight; its top symmetrical, and the whole sound to the core. It was shivered to fragments near the ground.

* Since writing the above, a communication from Mr. Raiff of Sandyville, northeastern part of Tuscarawas Co., states, that the tornado passed about 1 mile south of that place at *half past three* o'clock P. M.; moving in a direction north of east, but gradually curving towards the east. It commenced southwesterly about 2½ miles from Sandyville, and passed on to the east; in all a course of 17 miles. The direction corresponds with what has already been stated.

The roar of the tornado burst upon Brandon while the clock was striking 2 P. M. From this to Sandyville is 67 miles, which the storm swept over in an hour and a half; which gives a progressive velocity of 44⅔ miles per hour. This corresponds very closely with the previous estimate, the difference being only one-third of a mile. The mode of action seems to have been the same; parts of buildings, it is stated, were carried to the north, some to the southeast, and others to the west of the foundations.

Let us estimate the force necessary to break it. We will call the diameter 2½ feet, the height 80 feet, the outline of the top a rectangle 50 feet by 40; and let us suppose the whole surface exposed to the wind to equal one-fourth of that included in the outline, or 500 square feet. Under these conditions the resultant of all the forces would act on the tree at a distance of 55 feet from the point of fracture. Taking the strength of oak at the usual standard, a force of 73,636 lbs. would be required to break the tree. If the surface exposed to the action of the wind be estimated at 500 square feet, the pressure upon each square foot would be 147 lbs. This gives a velocity of 172·9 miles per hour, equal to 253·5 feet per second. This is about one-fourth the velocity of a cannon ball. Though the above estimate gives an enormous force to the wind, yet I cannot perceive that any of the assumptions are exaggerated. The most doubtful point is the amount of surface supposed to be presented to the wind by the limbs, &c. The estimate is believed to be a large one, as the trees were at the time destitute of foliage.

Other circumstances are not wanting to sustain this view of a high rate of velocity in the tornado. A mass of brick cemented together, 4 feet by 3 and 1 foot thick, containing at least 12 cubic feet, and weighing more than 1000 lbs., was carried 15 feet from the wall of a house. A board was driven 3 inches into a charred oak stump. The writer pulled a shingle from an oak tree which had been driven into it one inch. An estimate of the force, in the aggregate, of this tornado, if made clear to the mind, by comparison with some well known standard, would excite astonishment. If any one will take the trouble to make the calculation, he will probably assent to the following: a section of it one half mile wide and 100 feet high, exerted a force equal to half the steam power of the globe. More than 50,000 trees were prostrated or broken by it in less than one half hour.

It is not my purpose to speculate upon any cause or causes which could generate such an enormous force. Ohio has been remarkable for the number and violence of its tornados during the winter. It is important to state another fact in this connection. A southwest wind has prevailed to an unusual degree this winter, and the tornados have been found imbedded in and moving with the general current. The writer has no theory to present in regard to their causes, or their mode of action. In the present case he has adopted that curve which the data seemed to demand.

In conclusion, the writer would take this occasion to acknowledge his obligations to those gentlemen at Brandon and Mt. Vernon, who afforded every aid in their power; also to the superintendents of the Cincinnati and Columbus, and the H. and D. railroads, who generously tendered him a free ticket for more than 300 miles of travel.

Art. XII.—*Considerations on the Group of Small Planets situated between Mars and Jupiter;* by M. U.-J. Le Verrier.*

We cannot doubt that our solar system is far more complicated than has hitherto been supposed. To say nothing of the immense number of comets which apppear to belong to this system, or of the meteors whose path lies near the orbit of the earth, we can still find in the asteroids situated between Mars and Jupiter, the catalogue of which goes on increasing each day, a fruitful subject of reflection and research.

In regard to the small planets discovered at the beginning of the present century, Olbers threw out the idea, that they were derived from the ruins of one larger planet. This hypothesis, which was based upon no very precise data, and which is inconsistent with the great inclination of the orbit of Pallas, must be abandoned; especially since the numerous discoveries of the last few years. Instead of explaining the existence of these bodies by supposing an alteration in the primitive system of the universe, we are now led to believe rather, that they have been formed regularly, like the others, and according to the same laws.

If these views are just, we ought to expect the discovery of a very large number of small planets; in proportion to the zeal with which observers shall extend their researches and to the possibility of employing more powerful instruments. The liberality with which astronomers, who have been recently engaged in these examinations, have submitted to the public their facilities of discovery, in the publication of costly ecliptic charts, has rendered this labor henceforth easy. The multiplicity of discoveries in this field, instead of diminishing the interest in them, is of a nature rather to enhance their importance. For if it has been necessary to give up the hypothesis of Olbers, it may be hoped at least that the knowledge of a great number of small planets will lead to the discovery of some law in their distribution, and to the determination of the configuration of their principal groups. It is hardly credible that these asteroids should be scattered promiscuously in all parts of the heavens: besides their having been discovered hitherto only in a single zone, we must suppose that the same cause which has brought together so much matter in each of the principal planets, has distributed all the rest also into separate and distinct groups.

We are acquainted at present with the orbits of *twenty-six* asteroids (omitting in these remarks the twenty-seventh just discovered by Mr. Hind). These twenty-six asteroids having been found under diverse circumstances and by different observers, are as we believe capable of furnishing some grounds for generaliza-

* From the Comptes Rendus, vol. xxxvii, p. 793.

tion upon the whole group to which they belong. This is what we propose here to examine.

The small planets revolve in a zone which begins at the mean distance of 2·20 from the sun and extends to the distance of 3·16; unity being here the mean distance of the earth from the sun.

The excentricities of the orbits are quite considerable; their mean rising to 0·155. The amount of excentricity in each one seems to bear no relation to the mean distance from the sun, or to the longitude of the perihelion.

The inclinations of the orbits, both with reference to each other and to the ecliptic, are also quite large. The mean of the sines of the inclination to the ecliptic is 0·155. The amount of the inclination in each one does not appear to depend either upon the mean distance from the sun, or upon the direction of the ascending node.

The perihelia and the ascending nodes present some special peculiarities. The perihelia of *twenty* of them, having their longitudes between 4° and 184°, are embraced in an extent of the heavens less than a semi-circumference. The ascending nodes of the orbits of *twenty-two*, whose longitudes are between 36° and 216°, are also comprised within less than half of the circumference of the heavens, and nearly coincident with the space occupied by their perihelia. Possibly we may trace a regular difference between the mean direction of the ascending nodes of the planets nearest to the sun and of those more remote, and so have ground for the conjecture that these asteroids belong really to two distinct groups. But we pass this by for the present. What has been said suffices for our present purpose; viz., *the determination of a superior limit of the total mass of matter that can exist in the zone of the heavens which we are considering.*

Such an investigation can be based only upon a close examination of the nature and amount of the influences exerted by this matter upon the nearer planets, Mars and the Earth. The different terms into which we commonly resolve these influences are not equally well suited to our object. The periodic terms, depending upon the relative situation of the planets influenced and the small masses which act upon them, neutralize each other, if there are a great number of asteroids situated at each instant in every part of the heavens: so that the sum total of the disturbing masses may be very considerable and yet neither Mars nor the Earth suffer annual and sensible perturbations.

The secular variations of the elements of the orbits do not depend upon the relative positions of the bodies, and consequently are not subject to the inconvenience which I have pointed out. Those of the terms of secular variation, which depend upon the longitude of the perihelia and nodes, may however present analo-

gous difficulties, which we can eliminate only by considering the terms in which the longitude of these elements does not enter, if such terms exist. Now the motion of the perihelion, whether of Mars or the Earth, actually contains a sensible term of this kind: this term depends only upon the mean distance of the asteroids from the sun and upon the excentricity of the disturbed planet; moreover it is essentially positive whatever may be that of the small planets whose action upon Mars and the Earth we are here considering, so that all these small masses combine by their action to impress direct motions upon the perihelia of the two principal planets here referred to. If then we suppose that the zone in which these small planets are found contains a very great number of others like them, we should conclude that the whole group acts upon the perihelia very nearly as if they were collected into a single mass situated at a proper mean distance, and we should deduce therefrom the means of determining the total mass, or at least a limit which it cannot exceed.

This subject however presents other difficulties. Besides the term upon which we have been reasoning there is a second in the expression of the motion of the perihelion, of the same mathematical order of magnitude as the first, but which depends upon the direction of the perihelia of the several disturbing masses: it is important to inquire whether it can modify the results furnished by the first term.

If the perihelia of the asteroids, known and unknown, were distributed uniformly in all parts of the zodiac, the second term of the motion of the perihelion of Mars or of the Earth might be neglected; because the action of those masses, whose perihelia are situated in one half of the heavens, would be destroyed in this second term by the action of those masses whose perihelia are in the other half. But we have seen that there is great liability to error in reckoning upon such a uniformity in their distribution; the perihelia of twenty out of twenty-six being placed in one-half of the heavens, a result doubtless not of chance and seeming to indicate that the matter whose mass we are investigating is nearer the sun on the side of the summer solstice than of the winter. This circumstance must be taken into consideration, not for the purpose of introducing it as an essential condition into the solution of the problem, but on the contrary of arriving at a result which shall be independent of it.

This consideration will lead us not to make use of the motion of the earth's perihelion although it is better known than that of Mars. The earth's perihelion being in fact situated in that very portion of the heavens occupied by the perihelia of more than three-fourths of the asteroids, the second term which enters into the expression of its motion may become appreciable as compared with the first and of the contrary sign: inasmuch as these

terms are respectively proportional to the excentricities of the terrestrial orbit and the orbits of the small planets, and as the excentricities of these last are at the mean nine times greater than that of the earth.

The perihelion of Mars is situated much more favorably in relation to the mean direction of the perihelia of the asteroids; and besides the excentricity of its orbit is greater. As a result of these two conditions united, the second term which enters into the expression of the motion of the perihelion is only one *fourth* of the first. Now this superiority of the first term may be expected to continue after the discovery of a great number of new asteroids, whether this predominance of the perihelia in the mean direction of the summer solstice shall be confirmed, as it probably will be, or whether we shall be obliged to return to the idea of a uniform distribution of them through every part of the heavens.

In accordance with these remarks I have found that if the mass of the whole group of asteroids was equal to the mass of the earth, it would produce in the heliocentric longitude of the perihelion of Mars an inequality which in a century will amount to eleven seconds. Such an inequality, supposing it to exist, surely could not have escaped the notice of astronomers. If we reflect that this inequality will become strikingly sensible at the moment of the opposition of Mars, we must believe that at present and although the orbit of Mars has not been determined with perfect accuracy it cannot nevertheless admit of an error in longitude greater than one-fourth of the inequality which we have pointed out. Hence we conclude that *the sum total of the matter constituting the small planets situated between the mean distances* 2·20 *and* 3·16 *cannot exceed about one-fourth of the mass of the Earth.*

Similar conclusions may be reached by considering the motion of the plane of the ecliptic; the result will depend however in that case upon the hypothesis that the ascending nodes of more than three-fourths of the orbits are situated in a semi-circumference. The limit moreover which we should reach in this way would not be so narrow. We stop then for the present with the result furnished by the consideration of the perihelion of Mars. More precision may be given to it by perfecting the theory of Mars, and by the discovery of new asteroids. Such as it is, it seems fitted to throw some light upon a subject in regard to which we have hitherto had no reliable data.

In a second memoir* the author establishes by a thorough investigation of the secular variations of the elements of the orbits the following propositions.

1. The excentricities of the orbits of the known asteroids can suffer only very small changes as the effect of perturbation.

* Compt. Rend., t. xxxvii, p. 965.

These excentricities which are now quite large, have then always been and will always remain large.

2. The same is true of the inclination of their orbits. So that the amount of excentricity and inclination answers to the primitive conditions of the formation of the group.

3. These propositions are true only for distances from the sun above 2·00. An asteroid situated between Mars and the distance of about 2·00 would not be *stable* in the meaning which is attached to that word in celestial mechanics.

Flora, which is nearest to the sun of the known asteroids, is 2·20 distant. The author observes that it is quite remarkable that a planet has been found almost up to the line which theory assigns as the limit of stability, and that none have been found beyond it. Must we believe that the same cause which has given origin to so many asteroids above the distance 2·00 has also distributed them below this distance? but that, the excentricities and inclinations of these last being considerably increased, it is at present difficult to discover them, especially because towards their perihelion, they will be immersed in the light of the sun, and that coming to their opposition only near their aphelia, they will then be too far from us?

4. Owing to the magnitude of the excentricities and the inclinations and the smallness of their variations, the mean motions of the perihelia and of the nodes are proportional to the times.

Art. XIII.—*On Electric Induction—Associated cases of Current and Static Effects;* by Professor Faraday, D.C.L., F.R.S.*

Certain phenomena that have presented themselves in the course of the extraordinary expansion which the works of the Electric Telegraph Company have undergone, appeared to me to offer remarkable illustrations of some fundamental principles of electricity, and strong confirmation of the truthfulness of the view which I put forth sixteen years ago, respecting the mutually dependent nature of induction, conduction, and insulation (Experimental Researches, 1318, &c.). I am deeply indebted to the Company, to the Gutta Percha Works, and to Mr. Latimer Clarke, for the facts; and also for the opportunity both of seeing and showing them well.

Copper wire is perfectly covered with gutta percha at the Company's works, the metal and the covering being in every part regular and concentric. The covered wire is usually made into half-mile lengths, the necessary junctions being effected by twisting or binding, and ultimately soldering; after which the place

* Phil. Mag., 4th Ser., vii, 197, March, 1854.

is covered with fine gutta percha, in such a manner as to make the coating as perfect there as elsewhere: the perfection of the whole operation is finally tried in the following striking manner by Mr. Statham, the manager of the works. The half-mile coils are suspended from the sides of barges floating in a canal, so that the coils are immersed in the water whilst the two ends of each coil rise into the air: as many as 200 coils are thus immersed at once; and when their ends are connected in series, one great length of 100 miles of submerged wire is produced, the two extremities of which can be brought into a room for experiment. An insulated voltaic battery of many pairs of zinc and copper, with dilute sulphuric acid, has one end connected with the earth, and the other, through a galvanometer, with either end of the submerged wire. Passing by the first effect, and continuing the contact, it is evident that the battery current can take advantage of the whole accumulated conduction or defective insulation in the 100 miles of gutta percha on the wire, and that whatever portion of electricity passes through to the water will be shown by the galvanometer. Now the battery is made one of intensity, in order to raise the character of the proof, and the galvanometer employed is of considerable delicacy; yet so high is the insulation, that the deflection is not more than 5°. As another test of the perfect state of the wire, when the two ends of the battery are connected with the two ends of the wire, there is a powerful current of electricity shown by a much coarser instrument; but when any one junction in the course of the 100 miles is separated, the current is stopped, and the leak or deficiency of insulation rendered as small as before. The perfection and condition of the wire may be judged of by these facts:

The 100 miles, by means of which I saw the phenomena, were thus good as to insulation. The copper wire was $\frac{1}{16}$th of an inch in diameter; the covered wire was $\frac{4}{16}$ths; some was a little less, being $\frac{7}{32}$nds in diameter; the gutta percha on the metal may therefore be considered as 0·1 of an inch in thickness. 100 miles of like covered wire in coils were heaped up on the floor of a dry warehouse and connected in one series, for comparison with that under water.

Consider now an insulated battery of 360 pairs of plates (4×3 inches) having one extremity to the earth; the water wire with both its insulated ends in the room, and a good earth discharge wire ready for the requisite communication: when the free battery end was placed in contact with the water wire and then removed, and afterwards a person touching the earth discharge touched also the wire, he received a powerful shock. The shock was rather that of a voltaic than of a Leyden battery; it occupied *time*, and by quick tapping touches could be divided into numerous small shocks; I obtained as many as forty sensible

shocks from one charge of the wire. If *time* were allowed to intervene between the charge and discharge of the wire, the shock was less; but it was sensible after two, three, or four minutes, or even a longer period.

When, after the wire had been in contact with the battery, it was placed in contact with a Statham's fuse, it ignited the fuse (or even six fuses in succession) vividly; it could unite the fuse three or four seconds after separation from the battery. When, having been in contact with the battery, it was separated and placed in contact with a galvanometer, it affected the instrument very powerfully; it acted on it, though less powerfully, after the lapse of four or five minutes, and even affected it sensibly twenty or thirty minutes after it had been separated from the battery. When the insulated galvanometer was permanently attached to the end of the water wire, and the battery pole was brought in contact with the free end of the instrument, it was most instructive to see the great rush of electricity into the wire; yet after that was over, though the contact was continued, the deflection was not more than 5°, so high was the insulation. Then separating the battery from the galvanometer, and touching the latter with the earth wire, it was just as striking to see the electricity rush out of the wire, holding for a time the magnet of the instrument in the reverse direction to that due to the ingress or charge.

These effects were produced equally well with either pole of the battery or with either end of the wire; and whether the electric condition was conferred and withdrawn at the same end, or at the opposite ends of the 100 miles, made no difference in the results. An intensity battery was required, for reasons which will be very evident in the sequel. That employed was able to decompose only a very small quantity of water in a given time. A Grove's battery of eight or ten pairs of plates, which would have far surpassed it in this respect, would have had scarcely a sensible power in affecting the wire.

When the 100 miles of wire in the air were experimented with in like manner, not the slightest signs of any of these effects were produced. There is reason, from principle, to believe that an infinitesimal result is obtainable, but as compared to the water wire the action was nothing. Yet the wire was equally well and better insulated, and as regarded a constant current, it was an equally good conductor. This point was ascertained by attaching the end of the water wire to one galvanometer, and the end of the air wire to another like instrument; the two other ends of the wires were fastened together, and to the earth contact; the two free galvanometer ends were fastened together, and to the free pole of the battery: in this manner the current was divided between the air and water wires, but the galvanometers were affected to precisely the same amount. To make the result more

certain, these instruments were changed one for the other, but the deviations were still alike; so that the two wires conducted with equal facility.

The cause of the first results is, upon consideration, evident enough. In consequence of the perfection of the workmanship, a Leyden arrangement is produced upon a large scale; the copper wire becomes charged statically with that electricity which the pole of the battery connected with it can supply;* it acts by induction through the gutta percha (without which induction it could not itself become charged, Exp. Res. 1177), producing the opposite state on the surface of the water touching the gutta percha, which forms the outer coating of this curious arrangement. The gutta percha across which the induction occurs is only 0·1 of an inch thick, and the extent of the coating is enormous. The surface of the copper wire is nearly 8300 square feet, and the surface of the outer coating of water is four times that amount, or 33,000 square feet; hence the striking character of the results. The intensity of the static charge acquired is only equal to the intensity at the pole of the battery whence it is derived; but its quantity is enormous, because of the immense extent of the Leyden arrangement; and hence when the wire is separated from the battery and the charge employed, it has all the powers of a considerable voltaic current, and gives results which the best ordinary electric machines and Leyden arrangements cannot as yet approach.

That the air wire produces none of these effects is simply because there is no outer coating correspondent to the water, or only one so far removed as to allow of no sensible induction, and therefore the inner wire cannot become charged. In the air wire of the warehouse, the floor, walls, and ceiling of the place constituted the outer coating, and this was at a considerable distance; and in any case could only affect the outside portions of the coils of wire. I understand that 100 miles of wire, stretched in a line through the air so as to have its whole extent opposed to earth, is equally inefficient in showing the effects, and there it must be the distance of the inductric and inducteous surfaces (1483), combined with the lower specific inductive capacity of air, as compared with gutta percha, which causes the negative result. The phenomena altogether offer a beautiful case of the identity of static and dynamic electricity. The whole power of a considerable battery may in this way be worked off in separate portions, and measured out in units of static force, and yet be employed afterwards for any or every purpose of voltaic electricity.

I now proceed to further consequences of associated static and dynamic effects. Wires covered with gutta percha and then inclosed in tubes of lead or of iron, or buried in the earth, or sunk

* Davy, Elements of Chemical Philosophy, p. 154.

in the sea, exhibit the same phenomena as those described, the like static inductive action being in all these cases permitted by the conditions. Such subterraneous wires exist between London and Manchester; and when they are all connected together so as to make one series, offer above 1500 miles; which, as the duplications return to London, can be observed by one experimenter at intervals of about 400 miles, by the introduction of galvanometers at these returns. This wire, or the half or fourth of it, presented all the phenomena already described; the only difference was, that as the insulation was not so perfect, the charged condition fell more rapidly. Consider 750 miles of the wire in one length, a galvanometer *a* being at the beginning of the wire, a second galvanometer *b* in the middle, and a third *c* at the end; these three galvanometers being in the room with the experimenter, and the third *c* perfectly connected with the earth. On bringing the pole of the battery into contact with the wire through the galvanometer *a*, that instrument was instantly affected; after a sensible time *b* was affected, and after a still longer time *c*: when the whole 1500 miles were included, it required two seconds for the electric stream to reach the last instrument. Again; all the instruments being deflected (of course not equally, because of the electric leakage along the line), if the battery were cut off at *a*, that instrument instantly fell to zero; but *b* did not fall until a little while after; and *c* only after a still longer interval,—a current flowing on to the end of the wire whilst there was none flowing in at the beginning. Again; by a short touch of the battery pole against *a*, it could be deflected and could fall back into its neutral condition before the electric power had reached *b*; which in its turn would be for an instant affected, and then left neutral before the power had reached *c*; a wave of force having been sent into the wire which gradually travelled along it, and made itself evident at successive intervals of time in different parts of the wire. It was even possible, by adjusted touches of the battery, to have two simultaneous waves in the wire following each other, so that at the same moment that *c* was affected by the first wave, *a* or *b* was affected by the second; and there is no doubt that by the multiplication of instruments and close attention, four or five waves might be obtained at once.

If after making and breaking battery contact at *a*, *a* be immediately connected with the earth, then additional interesting effects occur. Part of the electricity which is in the wire will return, and passing through *a* will deflect it in the reverse direction; so that currents will flow out of both extremities of the wire in opposite directions, whilst no current is going into it from any source. Or if *a* be quickly put to the battery and then to the earth, it will show a current first entering into the wire, and

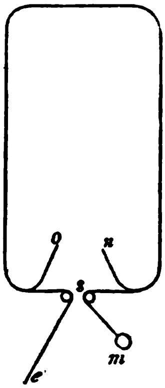
then returning out of the wire at the same place, no sensible part of it ever travelling on to *b* or *c*.

When an air wire of equal extent is experimented with in like manner, no such effects as these are perceived; or if, guided by principle, the arrangements are such as to be searching, they are perceived only in a very slight degree, and disappear in comparison with the former gross results. The effect at the end of the very long air wire (or *c*) is in the smallest degree behind the effect at galvanometer *a*; and the accumulation of a charge in the wire is not sensible.

All these results as to *time*, &c. evidently depend upon the same condition as that which produced the former effect of static charge, namely, *lateral induction;* and are necessary consequences of the principles of conduction, insulation and induction, three terms which in their meaning are inseparable from each other (Exp. Res. 1320, 1326,* 1338, 1561, &c.). If we put a plate of shell-lac upon a gold-leaf electrometer and a charged carrier (an insulated metal ball of two or three inches diameter) upon it, the electrometer is diverged; removing the carrier, this divergence instantly falls, this is *insulation* and *induction:* if we replace the shell-lac by metal, the carrier causes the leaves to diverge as before; but when removed, though after the shortest possible contact, the electroscope is left diverged,—this is *conduction.* If we employ a plate of spermaceti, instead of the metal, and repeat the experiment, we find the divergence partly falls and partly remains, because the spermaceti insulates and also conducts, doing both imperfectly: but the shell-lac also conducts, as is shown if time be allowed; and the metal also obstructs conduction, and therefore insulates, as is shown by simple arrangements. For if a copper wire, 74 feet in length, and $\frac{1}{12}$th of an inch in diameter, be insulated in the air, having its end *m* a metal ball; its end *e* connected with the earth, and the parts near *m* and *e* brought within half an inch of each other, as at *s*; then an ordinary Leyden jar being charged sufficiently, its outside connected with *e* and its inside with *m*, will give a charge to the wire, which

* 1326. All these considerations impress my mind strongly with the conviction, that insulation and ordinary conduction cannot be properly separated when we are examining into their nature; that is, into the general law or laws under which their phenomena are produced. They appear to me to consist in an action of contiguous particles, dependent on the forces developed in electrical excitement: these forces bring the particles into a state of tension or polarity, which constitutes both *induction* and *insulation;* and being in this state, the contiguous particles have a power or capability of communicating these forces, one to the other, by which they are lowered and discharge occurs. Every body appears to discharge (444, 987); but the possession of this capability in a *greater or smaller degree* in different bodies makes them better or worse conductors, worse or better insulators: and both *induction* and *conduction* appear to be the same in their principle and action (1320), except that in the latter an effect common to both is raised to the highest degree, whereas in the former it occurs in the best cases, in only an almost insensible quantity.

instead of travelling wholly through it, though it be so excellent a conductor, will pass in large proportion through the air at *s*, as a bright spark; for with such a length of wire, the resistance in it is accumulated until it becomes as much, or perhaps even more, than that of the air, for electricity of such high intensity.



Admitting that such and similar experiments show that conduction through a wire is preceded by the act of induction (1338), then all the phenomena presented by the submerged or subterranean wires are explained; and in their explanation confirm, as I think, the principles given. After Mr. Wheatstone had, in 1834, measured the velocity of a wave of electricity through a copper wire, and given it as 288,000 miles in a second, I said, in 1838, upon the strength of these principles (1333), "that the velocity of discharge through the *same wire* may be greatly varied, by attending to the circumstances which cause variations of discharge through spermaceti or sulphur. Thus, for instance, it must vary with the tension or intensity of the first urging force, which tension is charge and induction. So if the two ends of the wire in Professor Wheatstone's experiment were immediately connected with two large insulated metallic surfaces exposed to the air, so that the primary act of induction, after making the contact for discharge, might be in part removed from the internal portion of the wire at the first instant, and disposed for the moment on its surface jointly with the air and surrounding conductors, then I venture to anticipate that the middle spark would be more retarded than before; and if these two plates were the inner and outer coating of a large jar, or a Leyden battery, then the retardation of that spark would be still greater." Now this is precisely the case of the submerged or subterraneous wires, except that instead of carrying their surfaces towards the inducteous coatings (1483), the latter are brought near the former; in both cases the induction consequent upon charge, instead of being exerted almost entirely at the moment within the wire, is to a very large extent determined externally; and so the discharge or conduction being caused by a lower tension, therefore requires a longer time. Hence the reason why, with 1500 miles of subterraneous wire, the wave was two seconds in passing from end to end; whilst with the same length of air wire, the time was almost inappreciable.

With these lights it is interesting to look at the measured velocities of electricity in wires of metal, as given by different experimenters.

	Miles per second.
* Wheatstone, in 1834, with copper wire made it	288,000
* Walker, in America, with telegraph iron wire	18,780
* O'Mitchell, do. do. do.	28,524
* Fizeau and Gonnelle (copper wire) - -	112,680
* Do. (iron wire) - - -	62,600
† A. B. G. (copper) London and Brussels Telegraph	2,700
† Do. (copper) London and Edinburgh Telegraph	7,600

Here the difference in copper is seen by the first and sixth results to be above a hundred fold. It is further remarked in Liebig's report of Fizeau and Gonnelle's experiments, that the velocity is not proportional to the conductive capacity, and is independent of the thickness of the wire. All these circumstances and incompatibilities appear rapidly to vanish, as we recognize and take into consideration the lateral induction of the wire carrying the current. If the velocity of a brief electric discharge is to be ascertained in a given length of wire, the simple circumstances of the latter being twined round a frame in small space, or spread through the air through a large space, or adhering to walls, or lying on the ground, will make a difference in the results. And in regard to long circuits, such as those described, their conducting power cannot be understood whilst no reference is made to their lateral static induction, or to the conditions of intensity and quantity which then come into play; especially in the case of short or intermitting currents, for then static and dynamic are continually passing into each other.

It has already been said, that the conducting power of the air and water wires are alike for a constant current. This is in perfect accordance with the principles and with the definite character of the electric force, whether in the static, or current, or transition state. When a voltaic current of a certain intensity is sent into a long water wire, connected at the further extremity with the earth, part of the force is in the first instance occupied in raising a lateral induction round the wire, ultimately equal in intensity at the near end to the intensity of the battery stream, and decreasing gradually to the earth end, where it becomes nothing. Whilst this induction is rising, that within the wire amongst its particles is beneath what it would otherwise be; but as soon as the first has attained its maximum state, then that in the wire becomes proportionate to the battery intensity, and therefore equals that in the air wire, in which the same state is (because of the absence of lateral induction) almost instantly attained. Then of course they discharge alike, and therefore conduct alike.

A striking proof of the variation of the conduction of a wire by variation of its lateral static induction is given in the experi-

* Liebig and Kopp's Report, 1850 (translated), p. 168.
† Athenæum, January 14, 1854, p. 54.

ment proposed sixteen years ago (1333). If, using a constant charged jar, the interval *s*, page 90, be adjusted so that the spark shall freely pass there (though it would not if a little wider), whilst the short connecting wires *n* and *o* are insulated in the air, the experiment may be repeated twenty times without a single failure; but if after that, *n* and *o* be connected with the inside and outside of an insulated Leyden jar, as described, the spark will never pass across *s*, but all the charge will go round the whole of the long wire. Why is this? The quantity of electricity is the same, the wire is the same, its resistance is the same, and that of the air remains unaltered; but because the intensity is lowered, through the lateral induction momentarily allowed, it is never enough to strike across the air at *s*; and it is finally altogether occupied in the wire, which in a little longer time than before effects the whole discharge. M. Fizeau has applied the same expedient to the primary voltaic currents of Ruhmkorff's beautiful inducting apparatus with great advantage. He thereby reduces the intensity of these currents at the moment when it would be very disadvantageous, and gives us a very striking instance of the advantage of viewing static and dynamic phenomena as the result of the same laws.

Mr. Clarke arranged a Bain's printing telegraph with three pens, so that it gave beautiful illustrations and records of facts like those stated: the pens are iron wires, under which a band of paper imbued with ferro-prussiate of potassa passes at a regular rate by clock-work; and thus regular lines of prussian blue are produced whenever a current is transmitted, and the time of the current is recorded. In the case to be described, the three lines were side by side, and about 0·1 of an inch apart. The pen *m* belonged to a circuit of only a few feet of wire and a separate battery; it told whenever the contact key was put down by the finger; the pen *n* was at the earth end of the long air wire, and the pen *o* at the earth end of the long subterraneous wire; and by arrangement, the key could be made to throw the electricity of the chief battery into either of these wires, simultaneously with the passage of the short circuit current through pen *m*. When pens *m* and *n* were in action, the *m* record was a regular line of equal thickness, showing by its length the actual time during which the electricity flowed into the wires; and the *n* record was an equally regular line, parallel to, and of equal length with the former, but the least degree behind it; thus indicating that the long air wire conveyed its electric current almost instantaneously to the further end. But when pens *m* and *o* were in action, the *o* line did not begin until some time after the *m* line, and it continued after the *m* line had ceased, *i. e.* after the *o* battery was cut off. Furthermore, it was faint at first, grew up to a maximum of intensity, continued at that as long as battery con-

tact was continued, and then gradually diminished to nothing. Thus the record *o* showed that the wave of power took time in the water wire to reach the further extremity; by its first faintness, it showed that power was consumed in the exertion of lateral static induction along the wire; by the attainment of a maximum and the after equality, it showed when this induction had become proportionate to the intensity of the battery current; by its beginning to diminish, it showed when the battery current was cut off; and its prolongation and gradual diminution showed the time of the outflow of the static electricity laid up in the wire, and the consequent regular falling of the induction which had been as regularly raised.

With the pens *m* and *o*, the conversion of an intermitting into a continuous current could be beautifully shown; the earth wire, by the static induction which it permitted, acting in a manner analogous to the fly-wheel of a steam-engine or the air-spring of a pump. Thus, when the contact key was regularly but rapidly depressed and raised, the pen *m* made a series of short lines separated by intervals of equal length. After four or more of these had passed, then pen *o*, belonging to the subterraneous wire, began to make its mark, weak at first, then rising to a maximum, but always continuous. If the action of the contact key was less rapid, then alternate thickening and attenuations appeared in the *o* record; and if the introductions of the electric current at the one end of the earth wire were at still longer intervals, the records of action at the other end became entirely separated from each other; all showing most beautifully how the individnal current or wave, once introduced into the wire, and never ceasing to go onward in its course, could be affected in its intensity, its time and other circumstances, by its partial occupation in static induction.

By other arrangements of the pens *n* and *o*, the near end of the subterraneous wire could be connected with the earth immediately after separation from the battery; and then the back flow of the electricity, and the time and manner thereof, were beautifully recorded; but I must refrain from detailing results which have already been described in principle.

Many variations of these experiments have been made and may be devised. Thus the ends of the insulated battery have been attached to the ends of the long subterraneous wire, and then the two halves of the wire have given back opposite return currents when connected with the earth. In such a case the wire is positive and negative at the two extremities, being permanently sustained by its length and the battery, in the same condition which is given to the short wire for a moment by the Leyden discharge (p. 90); or, for an extreme but like case, to a filament of shell-lac having its extremities charged positive and negative.

Coulomb pointed out the difference of long and short as to the insulating or conducting power of such filaments, and like difference occurs with long and short metal wires.

The character of the phenomena described in this report induces me to refer to the terms *intensity* and *quantity* as applied to electricity, terms which I have had so frequent occasion to employ. These terms, or equivalents for them, cannot be dispensed with by those who study both the static and the dynamic relations of electricity; every current where there is resistance has the static element and induction involved in it, whilst every case of insulation has more or less of the dynamic element and conduction; and we have seen that with the same voltaic source, the same current in the same length of the same wire gives a different result as the intensity is made to vary, with variations of the induction around the wire. The idea of intensity, or the power of overcoming resistance, is as necessary to that of electricity, either static or current, as the idea of pressure is to steam in a boiler, or to air passing through apertures or tubes; and we must have language competent to express these conditions and these ideas. Furthermore, I have never found either of these terms lead to any mistakes regarding electrical action, or give rise to any false view of the character of electricity or its unity. I can not find other terms of equally useful significance with these; or any, which, conveying the same ideas, are not liable to such misuse as these may be subject to. It would be affectation, therefore, in me to search about for other words; and besides that, the present subject has shown me more than ever their great value and peculiar advantage in electrical language.

Note.—The fuse referred to in page 86 is of the following nature:—Some copper wire was covered with sulphuretted gutta percha; after some months it was found that a film of sulphuret of copper was formed between the metal and the envelope; and further that when half the gutta percha was cut away in any place, and then the copper wire removed for about a quarter of an inch, so as to remain connected only by the film of sulphuret adhering to the remaining gutta percha, an intensity battery could cause this sulphuret to enter into vivid ignition, and fire gunpowder with the utmost ease. The experiment was shown in the lecture-room, of firing gunpowder at the end of eight miles of single wire. Mr. Faraday reported that he had seen it fired through 100 miles of covered wire immersed in the canal by the use of this fuse.

Art. XIV.—*Reclamation of Borocalcite, as distinct from a mixture of minerals, found near Iquique, South Peru;* by Aug. A. Hayes, M.D., Assayer to the State of Massachusetts.

This mineral which was early analyzed and described by myself, has under the name of Teza, been confounded by Messrs. Ulex and Lecanu with other minerals which occur with it in the same bed. These chemists have apparently analyzed the minerals, constituting distinct species, together, without that careful separation mechanically, which should always precede a chemical analysis.

The parcel I originally received from John H. Blake, Esq., weighed several hundred pounds, and the existence of other salts with the borocalcite, was pointed out by me in detail. Yet, there were nodular masses frequently found, containing mere traces of other compounds, and quite as pure as commercial salts in general.

Supposing that future explorations had shown a more intimate union of the different salts, I have refrained from noticing the discordant results obtained by others, until the mineral as an article of commercial importance, should come to my hands.

Within a few weeks, the "muster" samples of two hundred tons have been sent to me, and I give the results obtained by careful analysis of these, as follows:

Water of crystallization,	27·16
Anhydrous borate of lime,	41·34
Glauberite,	23·20
Chlorid of sodium,	6·40
Sand,	1·90
	100·00

Some samples could be differently reported, but the point which I deemed essential, after reading the statement of M. Lecanu was, the absolute proof obtained by different modes of analysis, of the *entire absence of any borate of soda*, in the samples. The Glauberite and common salt are mere mixtures, apparent on inspection, and most easily separable by washing,—the borate of lime in the form of silky fibres being suspended in the water, and when collected and analyzed, giving the same proportions of acid and base, as the salt artificially formed.

As a source of boracic acid, in the manufacture of borax, I have already called public attention to this mineral, and it is now seeking the markets of the world, in large quantities.

16 Boylston St., Boston, 3d Apr. 1854.

ART. XV.—*On the present condition of the Crater of Kilauea, Hawaii;* by Rev. TITUS COAN.*

KILAUEA is still quiet. Many parties have visited the crater during the past year, and of these I have made special inquiries, as I have not been able to visit it myself. Changes have been slowly taking place within the crater. The aperture in the summit of the great dome which covers the igneous lake,† is gradually enlarging by the falling of avalanches from its walls, and thus revealing more and more of the fiery abyss below. But the melted lava does not approach the top of the dome. It is still 150 feet below, and you see it as you see the fire in the bottom of a coal-pit, by looking down through an orifice in the summit. The dome is probably two miles in circuit at its base, and some 400 feet high. Steam and smoke are constantly escaping from numerous holes and fissures in the dome, and the whole resembles a vast coal-kiln in the process of charring. On the western side of the dome a large fissure has been opened from the base to near the rim of the orifice at the summit, and from this rent, near the base, streams of lava have been occasionally poured out, inundating the area around the dome. This lateral outlet has prevented the lake from rising and flowing off at the summit orifice. Several other small lakes have been opened and closed at different points in the crater during the past year. These have varied from one-fourth of an acre to two acres in area, and their action has varied in intensity. A number of cones have also been thrown up in different parts of the crater, and there are evidences of partial overflowings in many places over the bottom of Kilauea. But these changes have occurred when no one was present to witness them.

A gradual rising is going on in the whole floor of the crater. This is effected by two causes—first, by the lifting forces below, as gases, igneous fusion, etc., and second, by eruptive overflowings: the former is more uniform and general—the latter irregular and partial. Every thing is silently preparing for another grand eruption; but when this will occur, at what point, under what circumstances, and with what effects, no one can tell.

You are aware that all the central part of the floor of the crater, embracing nearly one half of its area, is an elevated plateau, the highest points of which are now some 600 feet above what was the floor after the eruption of 1840. This central elevation rises in some places gradually, in others abruptly, from the surrounding floor. On the east and southeast its mural walls are perpendicular, presenting a dark, lofty, and frowning rampart,

* From a letter to J. D. Dana, dated Hilo, Hawaii, Jan. 30, 1854.

† The lake referred to occupies the southern end of the large area that forms the bottom of the crater of Kilauea.—D.

which no human foot can scale. At some points, immense avalanches have fallen from these high battlements, forming a steep inclined plane of confused and toppling debris, from the base of the walls two-thirds of the way up to their summits. On the north and west the elevation is less and less abrupt, and the plateau can be ascended from these points.

This central "table-rock," as will be seen, is surrounded by what used to be called "The Black Ledge," embracing all that portion of the crater which was sunk to the depth of 400 feet during the eruption of 1840. Of course "the Black Ledge" is now a lower plane than this central table, at its highest points, by about 200 feet. Many parts of "the Black Ledge," have also been elevated by local overflowings, and all of it, probably, by subterranean forces.

Occasionally the light of Kilauea is seen along the shores of Hawaii; but these exhibitions have been faint and few since 1840—a result of the roofing over of Halemaumau (the great lake). It may be doubted whether Kilauea will ever again assume its former activity and grandeur. From repeated agitations of the sea around our shores,—instances of which have recently occurred—we are led to think that submarine eruptions are occasionally taking place on the submerged portions of Hawaii, or from volcanic mountains and cones covered by the waters of the Pacific. These hidden eruptions will, doubtless, add to the dry land in our ocean in due time. Should there be a connection and a sympathy between these lower eruptive points and those above us, we may argue a diminution of the upper fires in proportion to the intensity of the submarine; and yet this may not follow, as we have magnificent eruptions on Mauna Loa, while Kilauea sleeps. These phenomena have puzzled me; but the solution you have given in your admirable work on geology satisfies my mind better than any theory I have seen.

All is quiet at Mauna Loa. The eruption of 1852 still steams a little at a few points in the woods back of us, not half so much however, as the eruption of 1840. There are points on this line, (that of 1840,) midway from Kilauea to the sea, where there are still much smoke and heat.

You ask if there were any small cones thrown up along the stream in the eruption of 1852. There were a few, but none of much size, so far as I explored. You are aware that the eruption commenced on the 17th of February, on the summit of Mauna Loa. In two days this valve closed, and the summit action ceased. On the 20th the lateral valve opened, 4000 feet below the top of the mountain, and the thundering torrent of fire rushed out and continued its awful disgorgement for twenty days. Between the summit point and the lower one, the ribs of the moun-

tain were rent, and seams and fissures were opened by the expansive force of the internal fire, as it forced its way in subterranean chambers down the sides of the mountain to the point of final eruption. Along the line of this covered duct an occasional jet was thrown up through a fissure to the surface, and in some places small cones were raised like warts on the side of the mountain. Many such were formed by the grand eruption of 1843, an account of which you have probably seen,—and the exploration of which was one of the most arduous and perilous I have ever undertaken. You will recollect that this eruption, (1843,) when it had flowed down the northern surface of the mountain for some two weeks, extending about thirty miles in length, with a breadth of from one to three miles, suddenly solidified on its surface, like a frozen river, still continuing its flow for weeks in a subterranean canal from the top of Mauna Loa to the base of Mauna Kea, throwing off lateral streams to the west and northwest. On this stream cones were thrown up from thirty to seventy feet high, usually with an orifice in the top, down which you could look and see the noiseless river of igneous rock, fused to whiteness, and gliding under your feet at the rate of a steam ship.

Art. XVI.—*Note on the genus Buckleya*; by Asa Gray, M.D.

In a foot-note on page 170 of the 45th volume of this Journal (eleven years ago) is published a brief character of an interesting Santalaceous genus, founded on the *Borya distichophylla* of Nuttall, and named *Buckleya* by Dr. Torrey, in compliment to Mr. S. B. Buckley, who first collected and communicated specimens with the fructification. Although specimens were collected about the same time by Mr. Rugel, and doubtless distributed in Europe, as were fruiting specimens gathered by Mr. Sullivant and myself in the autumn of 1843, I am not aware that the genus has been mentioned in any botanical work, or that any further knowledge has been gained respecting it, except the little which I have now to communicate.

In the year 1843, I was fortunate enough to procure and transport to Cambridge several living plants. Some of them were immediately sent to the Royal Botanic Gardens at Kew; but they probably perished in the voyage. Of those retained only one survived in our botanic garden; where it is perfectly hardy, and forms a shrub of 6 or 7 feet in height, remarkable for its neat habit, slender green boughs, and pretty distichous leaves. It proved to be a female plant; and has flowered and produced abortive fruit for the last two or three years, in the form of a fusiform pericarp, crowned with four elongated and foliaceous calycine lobes. But the greenish flowers, although not very small,

are so inconspicuous that they have until now escaped my notice at the proper season for examining them.

The point of principal consequence is that the female flowers prove to have a *double perigonium;* one, moreover, in which the exterior or "accessory calyculus," far from being minute or rudimentary, is much more conspicuous than the inner, being of more than twice its length! The divisions of this accessory perigonium are regularly alternate with the inner or normal perigonium of the family, that which is opposed to the stamens, and which manifestly answers to the single floral envelope in the allied *Pyrularia.* But they so perfectly resemble the leaves in shape and texture, (although narrower as well as smaller,) as also in vernation, expanding with the leaves some days before the inner perigonium opens, that they do not perhaps suggest much argument, in addition to what is already furnished by *Olacineæ* and *Loranthus,* for changing the generally received view of the nature of the floral covering in *Santalaceæ.* Their foliaceous nature is further evinced by their distinct articulation with the summit of the calyx-tube. These long and narrow lobes are the "perigonium" of Dr. Torrey's character of the genus, at least of the female flowers; the proper perigonium having probably fallen in the specimens he examined, although it is by no means very deciduous. I have not seen the male flowers.

A second peculiarity of *Buckleya* relates to the æstivation of the inner or proper floral envelope; which is not valvate, as in all other known Santalaceous genera, but *imbricative,* decidedly, though not very strongly so.

A third but slighter discrepancy from the general character of the *Santalaceæ,* as commonly stated, is found in the opposite or sub-opposite leaves. These are said, in the published character of Buckleya, to be alternate: but they are placed, for the most part, in pairs on the slender branchlets, the whole much resembling the leaflets of a pinnate leaf, and the two leaves of each pair are either somewhat disjoined, or as frequently exactly opposite, at least on the upper part of the branchlets. The leaves are more strictly opposite in *Darbya, Fusanus,* and *Santalum* itself.

It might be urged, apparently on very strong grounds, that a plant whose floral envelopes (at least in the male flowers) are distinctly double, and the inner series imbricated in æstivation, cannot belong to the order *Santalaceæ;* the more so as it is said in the published character to have a uniovulate ovary. This character, however, may have been merely inferred from the solitary seed, or *fertilized* ovule; and it is evident from the absence of the inner perigonium that the female flowers examined were in an advanced state. A dissection of the unimpregnated flowers in the living plant brings to view in *Buckleya* the ordinary struc-

ture of the Santalaceous ovarium. The small cell, if it may be so called, where there is no empty cavity, is filled by a short, oval or olong, erect central column, which bears near its free apex four very minute and simple ovules, evidently reduced to naked nuclei. This genus therefore undoubtedly belongs to the small alliance or class that comprises the *Olacineæ* and the *Santalaceæ;* and as respects these two orders I should not hesitate to refer it to the latter, notwithstanding the double floral envelopes. The moderate imbrication of the divisions of the perigonium in the bud is surely a discrepancy of slight importance in comparison with this characteristic ovulation; and I cannot but recognize in this case something like a direct confirmation of the opinion I had already ventured to express (Botany of the U. S. Expl. Exped. i, p. 301), namely, that Mr. Miers, in excluding on such grounds *Bursinopetalum* and *Pleuropetalon* from his order *Icacineæ*, has over-estimated the importance of a character which, however valuable, is seldom perfectly stable throughout all the members of a natural order.

As respects the female flowers of *Buckleya*, it therefore appears that the published character should be modified as follows:

Fl. fœm.: Perigonium basi quadribracteolatum; tubo clavato cum ovario connato; limbo duplici, utroque quadrisecto, laciniis exterioris (calyculi accessorii) linearibus foliaceis tubo sublongioribus diu persistentibus, interioris triangulari-ovatis æstivatione modice imbricatis exterioribus plus dimidio brevioribus deciduis. Discus epigynus planus, quadrangulatus, angulis parum liberis perigonii interioris laciniis alternantibus. Stamina nulla. Stylus breviusculus: stigma cruciato-quadrilobum, lobis perigonii interioris laciniis oppositis. Ovarium uniloculare. Ovula 3 vel 4, minima, simplicissima, ex apice placentæ centralis crassæ liberæ (loculum parvum implentis) pendula.

Art. XVII.—*On a mode of giving permanent flexibility to brittle specimens in Botany and Zoology;* by Prof. J. W. Bailey, U. S. Military Academy, West Point, N. Y.

The excessive fragility in the dry state, of many plants, and particularly of those which secrete carbonate of lime is well known to botanists. There is no herbarium in existence in which the specimens of Amphiroa, Jania, Corallina, Halimeda, Liagora, Chara, &c. are not in a more or less mutilated condition, which becomes worse every time the plants are examined. In studying a large collection of the stony Algæ I was led to remark their perfect flexibility while moist, which passed to great brittleness when dry, and it occurred to me that if they could be kept permanently moist they would remain permanently flexible.

I then remembered that General Totten, of the U. S. Engineers, had mentioned to me, some years ago, his success in preventing the cracking and peeling off of the epidermis of various shells, by impregnating them with chlorid of calcium. I also remembered Boucherie's experiments with the same substance in giving flexibility to wood. The principle that *a substance which is flexible when moist, will remain permanently moist, and therefore permanently flexible, when impregnated with a deliquescent salt,* is so obviously true that it needed no experiments to convince me of its applicability to the fragile plants above mentioned, and to many other specimens in natural history. But as practical difficulties often occur in the application of correct principles, I have tested the process by numerous experiments in which chlorid of calcium was employed to give flexibility to various vegetable and animal products, and the results have fully equalled my expectations. My specimens of Amphiroa, Jania, Corallina, &c. after being impregnated with this salt, and then exposed for months to the air, can be handled as freely as if just taken from the water, and they permanently retain almost the utmost degree of pliability they are capable of receiving. Species of dry, crisp and brittle Lichens when treated in the same way became soft, elastic, and flexible, so as to bear very rough handling with perfect impunity. Many of the common Algæ which shrink much in drying, and therefore assume a very unnatural appearance, and besides are apt either to become cracked or torn, or to wrinkle up the paper to which they adhere, retain after immersion in this salt, nearly their normal degree of distention, and preserve a much more natural appearance than when dried in the usual way. Many dried specimens of plants whose leaves, flowers or fruit, drop off almost at a touch from specimens in my herbarium, became permanently pliable when immersed for a short time in a solution of chlorid of calcium, and could then at any time be handled freely, while their appearance was in no degree injured.

In the animal kingdom, the results obtained in restoring permanent flexibility to dry and brittle specimens of Crustaceans, Insects, Gorgonias, Sponges, &c. were equally satisfactory, and have convinced me that almost every naturalist will, in his own department, find many useful applications for this process.

The *mode of application* which I have employed is to immerse the dry specimen for some time in a neutral saturated solution of chlorid of calcium, (which any one can make for himself by saturating chloro-hydric acid with marble,) and then after the specimen has become sufficiently softened to bend easily, remove it and let it drain in the open air. In some cases where the specimens do not imbibe the salt readily, it is well to soften them in warm water before immersion in the salt. A speedy impregnation will then take place, after which the specimens, if plants,

may be subjected to moderate pressure in the usual way, and restored to the herbarium, while other specimens may be kept on shelves or in any way usually employed for similar objects, and all will for any length of time retain sufficient moisture to prevent brittleness. The salt being neutral, no fear need be apprehended of its injuring color or texture, while its antiseptic properties will aid in the preservation of matters liable to decay.

Art. XVIII.—*Caricography;* by Prof. C. Dewey.

(Continued from vol. ix, p. 30, Second Series.)

No. 243. *Carex aristata*, R. Br., var. *longo-lanceata*, Dew.

Pistillate scale oblong, long-awned or long-cuspidate, longer than the fruit; leaves, sheaths, and bracts scabro-pubescent.

Collected in the Mauvaises Terres (Bad Lands) of Nebraska Territory in 1853 by F. V. Hayden.

The common forms of C. aristata, R. Br. and of C. trichocarpa, Muh., appear abundant over that western country, with some tendency to unusual length of their scales, but in the above marked variety the scales are considerably longer than the fruit, and sometimes only very elongated lanceolate. Perhaps this plant should be held to be a distinct species,

No. 244. *C. nebrascencis*, Dew. .

Spicis 4–6; staminiferis binis apicem approximatis oblongis brevibus densis, inferiore sessili parva, cum squamis oblongis subobtusis; pistilliferis 2–4, oblongis brevi-cylindraceis densifloris, superioribus apice staminiferis sessilibus, inferiore brevi-pedunculata; bracteis lanceolatis sessilibus culmum æquantibus; fructibus *distigmaticis* convexis obovatis vel ellipticis basin teretibus brevi-apiculatis ore sub-bilobis, squama ovata acuta vel lanceolata paulo brevioribus: culmo acuto triquetro lævi basin foliato.

Culm 1-2 feet high, usually 16–20 inches, sharp-triquetrous, smooth, scarcely rough between the spikes, with lanceolate leaves towards the base shorter than the culm and soft, glabrous, serrulate on the edges, and with sessile bracts under the spikes as long as or longer than the culm above; staminate spikes two, oblong, close-flowered, short, lower one very short and sessile, with oblong and acutish scales: pistillate spikes 2–4, usually three, short, oblong, thick, erect, rarely an inch long, cylindric, densely flowered, sub-approximate, two upper sessile and often staminate at the apex, the lowest short pedunculate; stigmas two; fruit elliptic or obovate, slightly tapering at the base, short rostrate or round-apiculate, entire or sub-bilobed at the orifice, glabrous and double

convex; pistillate scale ovate, acute, or mucronate, sometimes lanceolate, narrower and once and a half longer than the fruit, tawny with a white line on the back.

No. 245. *C. Haydenii*, Dew.

Spicis 4–6, cylindraceis tenuibus; interdum quatuor, suprema staminifera, reliquis fructiferis; nunc staminifera unica vel binis, inferiore basin fructifera: pistilliferis 3–5, distigmaticis longo-cylindraceis erectis gracilibus laxifloris subremotis foliato-bracteatis, superiore vel pluribus apice staminifera, et infima brevi-pedunculata basin rariflora; fructibus ellipticis utrinque convexis apiculatis ore integris lævibus, squama lanceolata nigrescente dorso alba sub duplo brevioribus; culmo triquetro lævi basin foliaceo apicem scabro 2–3 pedali.

Culm triquetrous, erect, 2–3 feet high, smooth, scabrous above, leafy towards the base; spikes 4–6, cylindric, slender, erect; sometimes four, one staminate, and others nearly destitute of fruit; sometimes two staminate, the lower with fruit at the base, and four pistillate with the upper staminate at the apex, all long cylindric, 2–4 inches long, slender, erect, loose-flowered, leafy-bracteate, the lowest short-pedunculate and quite loose-flowered at the base; stigmas two; fruit oval or obovate, tapering at base, convex on both sides, short-rostrate or apiculate, entire at the orifice, smooth, often dark brown or nearly black in maturity; pistillate scale lanceolate, blackish with a white dorsal line, and nearly twice the length of the fruit.

On Missouri River near Fort Pierre; collected by Ferdinand V. Hayden in 1853, among the 1200 species of plants obtained while collecting fossils, &c. for Prof. James Hall, State Geologist, Albany. With the preceding three species of Carex were many others, all of which Prof. Hall politely put into my hands for examination.

Note.—The following species, just alluded to, were also collected by Dr. Hayden in that western region: C. eburnea, Boott; C. Meadii, Dew.; C. lanuginosa, Mx.; C. aristata, R. Br.; C. lacustris, Willd.; C. riparia, Good.; C. Shortii, Torr.; C. Muhlenbergii, Schk.; C. rosea, Schk. and its var. radiata, Dew., which was figured by Kunze for C. disperma, Dew., and was a great mistake; C. anceps, Muh.; C. straminea, Wahl.; C. Steudelii, Kth.; C. trichocarpa, Muh.; C. vesicaria, L.; C. festucacea, Schk.; C. grisea, Wahl.; C. stricta, Lam.; C. strictior, Dew.; C. mirabilis, Dew.; C. leporina, L., credited before by Boott to Arctic America; C. stenophylla, Wahl., found also by Dr. Richardson at Carlton House, and on Rocky Mountains, and abundant in Nebraska Ter.; C. marginata, Muh., exactly the form figured by Schk. in his fig. 143, and from which C. pennsylvanica, Lam. differs in its fruit and leaves; C. vulpinoidea, Mx.; C. stipata,

Muh.; C. vulpina, L., so exactly the European plant, that there can be no doubt of its correct determination in Ohio, and that the hesitation expressed by Dr. Boott in Richardson's Arctic Expedition, p. 466, is groundless, especially from any supposed resemblance to C. stipata, Muh., as the two are vastly different; C. Davisii, Tor.; C. longirostris, Tor.; and C. recta, Boott.

Art. XIX.—*Reviews and Records in Anatomy and Physiology;* by Waldo I. Burnett, M.D., Boston.

I. Psorospermia.*

Lieberkühn has furnished a communication of considerable length and detail on this subject, to which we refer particularly from its interesting relations. As a key to the article we may give his conclusions, as follows:

"The kidneys of some frogs contain cysts which enclose contents of very manifold characters, namely: 1. A peculiar, uniformly divided, granular mass. 2. This mass encompassed by small groups of an oval or fusiform shape. 3. Fusiform bodies, invested with a structureless membrane. 4. Developed psorospermatic corpuscles. These different objects are found wholly or partially in one and the same cyst. The mature psorospermial corpuscles usually contain three to five baton-like or ellipsoid or globular diaphanous corpuscles which are structureless; they usually have also a rather large nucleus. The diaphanous corpuscles are seen moving and springing in their capsules, and the nucleus is thereby moved about hither and thither. Such kidneys contain also free amœba-like corpuscles, and gregarina-like bodies largely nucleated."

These formations are by no means common,—our author having found them in four cases only out of a thousand specimens he has examined.

But in other animals, and in other organs he has found similar formations, as have others, such as Müller, Gluge, Leydig, before him. In fact, these psorospermial forms occur in both a free and a cystic state in different tissues.

The question is certainly a most interesting one: What signification shall be put upon these singular animal-like forms? But this is only one passage in the comprehensive question, What are most infusorial forms not evidently of a vegetable nature which every microscopist meets with perhaps daily in his studies? The subject certainly is not yet ripe for decision, but we may allude to it in a suggestive point of view. Both Kauffmann and Lieberkühn, in watching these Psorospermia in glasses on their tables,

* *N. Lieberkühn.* Ueber die Psorospermien, in *Müller's* Arch., 1854, Hft. 1, p. 1–25.

have observed that they multiply by a segmentation of their nucleus, and that the product of this division resembles precisely the parent. In some specimens observed by Lieberkühn, taken from a dog, he found the parent vesicle to contain sometimes 16 segmented globules. But here the observations unfortunately ceased, and we are not aware that in any case or instance they have been extended beyond this point; that is, so as to show that the offspring of this segmentation which so closely resembles its parent pursues the same course and produces by segmentation a third series. The doctrine that individual animal forms may be unicellular or that an animal may be composed solely of a single cell, as advanced by Siebold and Kölliker,* we regard as wholly untenable in the present state of science; for, aside from its being against the general analogy of individual zoological forms, it has not yet facts enough to sustain it merely as a point of observation. The cell is indeed typically the primordium of all organized forms, but true individual animal life seems to involve a cycle of relations not implied in single cells; in other words, these last must always lose their character as such in a definite form which belongs to the individual. Extended researches in Microscopy in all directions of the organic world,—all tend to establish the doctrine that sex lies behind all true individual forms,—that the ovum is the point of departure on one side, and the spermatic particle on the other,—and that by the conjunction of the two a new individuality is produced. There is indeed a most striking and beautiful uniformity between the simple cell and the ovum in a morphological point of view, or between the cell and the parent sperm-vesicle, thereby indicating a unity of idea and place in the first expression of life and the functional means of its cycle of actions; but without wishing to be mystical, it appears to us that life as expressed under the individual whether in its first or last forms,—as an egg ready to develope, or as a complete animal, rises high above and implies a great deal more than simple cell-conditions. We argue, then, that all true animals arise primarily or secondarily from ova, and therefore have sex, and that those animal-like forms so often seen as parasites or entozoa in animals must for the present stand undetermined, or if any interpretation is to be put upon them, they may be regarded as undeveloped forms of true animals, passing through various metamorphotic changes to which we have some clew in the remarkable phenomena of *alternation of generations.*

If the contents of the lower portion of the intestine of a frog be examined under the microscope, there will usually be found innumerable moving particles which give a very life-like aspect to the whole field. These belong to the infusorial genus *Bodo* of

* *Siebold* and *Kölliker.* See their Zeitsch. für wissenschaftliche Zoologie, I, p. 270, and ibid p. 1.

Ehrenberg. Examined with the highest and best microscopic powers, they are found to be composed of a more or less globular head to which is attached a thread-like tail of considerable length. This head taken by itself has all the appearances of a simple cell, —it is nucleated and even nucleolated; yet the whole body moves about by means of its tail in a most animal-like manner and in studying the field one can hardly divest the mind from the opinion that they are true animals.

The intestinal canal of many insects likewise, especially those feeding in damp, moist places, will often be found teeming with forms so large and numerous that it is singular that the insect should live. Many of these forms are composed of a more or less globular sac filled with a punctiform matter in which lies a round nucleus; at one extremity of this sac is an orifice surrounded by a circle of cilia. Others are more vermiform, regularly wrinkled, but apparently non-nucleated. These belong to the Gregarinæ, and are the forms upon which Siebold and Kölliker have based their doctrine of unicellular animals. Other instances might be cited, equally prominent, which almost daily come under the eye of the microscopist in his studies in the lower department of the animal kingdom.

Our own observations upon these objects have not led us to the view that they are, any of them, perfect individual forms;* on the other hand, research is constantly reducing their generic numbers, on the one side, by showing that many so-called genera are only different developments of one and the same form; and by removing them, on the other, from that *Receptaculum omnium animalium et plantarum*—the Infusoria—it being shown that they are only germs or larval forms of some of the inferior classes.† The recent investigations of Siebold and others, in Helminthology, to which we shall soon allude, have shown how varied may be the larval forms, how dissimilar from the true adult animal, and how remarkable the localities in which they occur, in the case of many of the Entozoa. In the case of the Psorospermia, or the Gregarinæ, or other forms which have been grouped under special genera or species, we must wait further research, and they will then, we think, be shown to be undeveloped forms. As to the question of the animal or vegetable nature of such organisms, it seems equally obscure, for the older criteria by which animals were separated from plants have long since been regarded as invalid; and some of those which in late years have been regarded as among the most constant, have quite recently been declared equally unsound.

* For *Bodo*, see Boston Journal of Natural History, vi, No. 3, 1853, p. 319.

† Thus *Agassiz* has shown that *Paramœcium* and *Bursaria*, &c. are only larval forms of Planariæ. See Annals of Nat. Hist., vi, 1850, p. 156. *Euglena*, *Amœba*, and others, will most probably meet with a like resolution.

Robin has argued that the Psorospermia are plants because they contain cellulose; but as the investigations of Kölliker, and of Löwig and Schacht have shown that this substance occurs indubitably in animal tissues, this can no longer be considered as a criterion.* We are reduced to *voluntary motion* as the only now known differential criterion on this subject, and as this must be a point on which different observers will vary, the subject must still remain unsettled; but we protest against any fusion of the two organic kingdoms on this obscure arena.

II. On the Mermithes.†

A memoir of great worth has recently appeared upon these singular parasites, which has a double importance of quite clearing up the history of these animals in all their stages, and of furnishing a contribution to the histology of the lower animals of a most valuable character. This memoir is by G. Meissner of München, under the direction of Siebold who furnished him with specimens and other opportunities for its successful prosecution. Seldom have we met with a paper of more careful and extended detail, and which leaves so little behind for investigators in the same direction. Added to this textual detail, each and every anatomical point is illustrated by admirably executed figures. With our limited space we can at best notice only a few of the more prominent points of this paper.

In the first place it should be remarked that the natural history of the Gordiacei was for a long time quite obscure and little understood, and many detached observations not of a parallel character did not improve the subject. To the sagacity of Siebold we are indebted for the successful resolution of the whole enigma, and the results he has obtained are as singular as new.‡ It appears that these animals live part of their life as a regular entozoon, and the rest as an independent being. And what is most remarkable, they enter the animals in which they are for a time parasites, not in the form of eggs, as do other Helminths, but as more or less developed forms. The animals in which they live as parasites are almost exclusively Insects of different orders in both the larva and imago state. In the abdominal cavity of the larvæ of *Yponomeuta albicans*, Siebold found numerous undeveloped forms of *Mermis albicans*. Watching them he found that after further growth, they perforated the skin of these larvæ and

* For full reference to the subject of cellulose in the tissues of the lower animals, see *Siebold* and *Stannius's* Comparative Anatomy, Amer. ed., vol. i, § 172.

† Beiträge zur Anatomie und Physiologie von Mermis albicans. Von Dr. *Georg Meissner*. In *Siebold* and *Kölliker's* Zeitschrift für wissenschaftliche Zoologie, v, 1853, p. 207–285.

‡ *Siebold.* See the Entomol. Zeitung zu Stettin, 1848, p. 292, 1850, p 329, also Beiträge zur Naturgeschichte der Mermithen, in *Siebold* and *Kölliker's* Zeitsch. für wissensch. Zool., v, 1853, p. 211.

made their escape. These freshly-escaped individuals were all sexless, but contained each a considerable *corpus adiposum*, at the expense of which their sexual parts were subsequently developed. These animals crawled about, and soon entered some damp earth, where they remained several months, during which time they were further developed, changed their skin, copulated and laid their eggs. The embryos hatched from these eggs, had the filamentoid form of the adults, and as Siebold conjectured that they intended to come to the surface for the sake of entering in their turn young insects, he procured quite young larvæ of this same insect and put them in an hour-glass together with the young Mermithes. In a few hours they had entered the body of these larvæ, two or three in each. Siebold had the precaution to make this point certain by carefully examining these larvæ previously and determining that their bodies were free of these parasites. After this, the same round of life is again passed. It would appear, then, that these animals pass their earlier (but not their embyonic) conditions of life whereby they attain their development—in fact a proper larval state—in the bodies of insects, and that their life as distinct sexed individuals is free and non-parasitic. Siebold found this species in very many genera of Lepidoptera, also in different species of Orthoptera, Coleoptera, and Diptera. We may mention that the common cricket as also some other Orthoptera, are frequent recipients of *Mermis*, and we have seen many specimens of this kind. Until Siebold's recent contributions we had supposed in common with other naturalists, that these Helminths merely hibernated in these insects, but this is now quite improbable.

So much for a brief reference to the mode of life of these animals; we will now turn and glance at some of the important histological points as wrought out by Meissner.

Cutaneous System.—Omitting the very full details given of the structure of the skin in these animals, its composition of three distinct layers, &c., we will allude only to the fact that *Chitine* enters likewise into its formation. This fact is important as corroborative of other observations. Chitine was formerly supposed to belong exclusively to the teguments of the Arthropoda, being particularly prominent in the skin of insects; but recent chemical analyses of the teguments of lower animals show that it occurs in nearly every class of the Invertebrata.* It can therefore no longer be regarded as having diagnostic characteristics for certain classes, but sustains relations to the external dermic skeleton of the Invertebrata generally, analogous to those of bone in the four classes of Vertebrata.

* Besides the present case we would refer to the following: *Grube*, *Müller's* Arch. 1848, p. 461, and *Wiegmann's* Arch. 1850, p. 253; *Schultze*, Beitr. zur Naturgesch. d. Turbellarien, p. 33; and *Leuckart* in *Siebold* and *Kölliker's* Zeitsch., 1851, p. 192, and *Wiegmann's* Arch. 1852, p. 22.

Muscular System.—This was found quite developed, and it is a singular fact that all the muscles have a longitudinal direction. Transverse muscles do not exist. But Meissner has indicated a histological feature of muscular tissue in these animals, which deserves notice. It is well known that striated muscular fibre is rather limited in its distribution among the Invertebrata. As stated in another place,* we have not observed it below the Articulata, and have regarded it as actually absent in the remaining classes —the Cephalopoda, Cephalophora, Acephala, Annelides, Turbellaria, Helminthes, Echinodermata, Acalephæ and Polypi. Now, we have hitherto supposed from observation that the fibre being the true embryological element of muscle, a further division into fibrillæ occurred only in the higher form of this fibre, the so-called *striated* muscle; in other words, that a fibrillated structure of muscular fibre was found only in the striated form. But Meissner describes the fibre of *Mermis* as readily capable of being split up into longitudinal fibrillæ of the most regular and delicate character, and yet neither these fibres nor fibrillæ are properly transversely striated. He remarks however, that an appearance like striation is sometimes observed by a wave-like contraction of the fibre.† Results of this character which the more careful research of the present day is developing, in studies of the lower animals especially, fully indicate that the subject of muscular tissue is not well understood as to its manifold variations of form; at least, after we have left the typical forms of the higher animals. Thus, as company for the present instance, I may mention that Leydig‡ found the muscles of the alimentary canal of *Artemia* among the Crustacea, composed of spindle-shaped instead of disc-like elements, so arranged, points and bases alternating, as to form a symmetrical fibrilla. In conclusion upon this system, we may remark that Meissner found here no sarcolemma, and no perimysium of the muscular layer.

Nervous System.—The researches of this investigator in this direction have particular interest, because this system has been generally denied to the Gordiacei, and if seen by previous observers it was only most unsatisfactory.§ But the histology of this system is quite as interesting.

Meissner found it so developed that he divides it into three portions: a central, a peripheric, and a splanchnic portion.

* This Journal, Jan. No., 1854, p. 89.

† We suspect it is this same wave-like aspect that has been often mistaken for striation in the muscles of some of the lowest animals, thereby leading to no little discrepancy among observers in their statements. See this Journal, Jan. No., 1854, p. 92, notes.

‡ *Leydig.* Ueber Artemia salina und Branchipus stagnalis, in *Siebold* and *Kölliker's* Zeitsch., iii, p. 280, Taf. viii, fig. 6.

§ *Berthold* and *Blanchard* both supposed they saw cords which might be nerves, but their observations were wholly unsatisfactory;—for references see *Siebold* and *Stannius'* Comp. Anat., Amer. ed., vol. i, § 104, note 5.

The *central portion* is divided into two parts, one at the cephalic, the other at the caudal extremity of the body. In the first, are two anterior and two posterior cephalic ganglia, and an œsophageal ring composed of a superior and an inferior ganglian united by lateral commissures. In the second part, situated in the tail, there are three fusiform ganglia, of like character but smaller than those of the head.

The *peripheric portion* consists of six filaments given off from the upper part of the anterior cephalic ganglia which go to as many papillæ on the head and probably organs of sense,—of two lateral cords arising from the superior œsophageal ganglion, which traverse the sides of the body, giving off filaments to the muscles, the skin, &c., and of some smaller twigs from the cephalic centres for the muscles of that region.

The *splanchnic portion* consists of two lateral trunks arising from the œsophageal ganglion, which soon meet and unite on the median line of the body, forming one cord which extends to the tail. From this cord are given off filaments to the organs of vegetative and reproductive life.

The three cords thus formed, having traversed the body, end each in one of the three ganglia above described. We can here allude to only one more point in the disposition of the nervous system; this is the final termination of the nerve-filament in muscle. According to our author, a twig enters the muscular fibre at right angles to the course of the latter, and upon its entrance divides into two twiglets, one of which runs with the fibre one way and the other the opposite, and is lost in the muscular tissue.*

The histology of this system in so minute animals as these, worked out by an observer so expert and faithful as Meissner, presents many note-worthy points.

The ganglia in question are composed exclusively of ganglion-cells or globules which appear to be the infundibuliform expansion of as many nerve fibres that compose the nervous cord connecting these ganglia with the general system. There are none of the so-called nerve-cells usually found in nervous centres—in fact these central masses rather resemble true ganglionic formations, excepting that they are terminal instead of on the course of a nervous cord. The description and figures, especially the latter, of Meissner are so good, as to leave no doubt that there is here a direct continuity of the nerve-fibre with the ganglionic vesicle.

In a former review† we alluded to some discrepancy on this point, and as this continuity had been observed by some, and yet

* As *Meissner* observes, a similar disposition is mentioned by *Doyère* (Ann. d. Sci. Nat., 1840, xiv, p. 346) in the muscles of the Tardigrada, and by *Quatrefages* (ibid, 1843, xix, p. 300) in the Eolidina, some Annelides and Rotatoria.

† This Journal, Sept. No., 1853, p. 253.

not seen by others who had searched carefully for it, we suggested that this direct connection, when present, might be an exceptional condition. But numerous researches since published, and especially the very complete memoir of Axmann,* represent this as a very common form of disposition of the elements of nervous centres in man and the mammalia. The subject is indeed somewhat obscure in a functional point of view, for what is the interpretation of this direct continuity of the vesicular with the tubular portion of this system? Certainly it is not the essential condition of function between the two, or all nerve fibres would terminate in this manner and there would be no ganglionic vesicles but those having this connection. But this, as is well known, is far from being the case. We leave the subject until another time. As to the structure of the peripheric nerves, our author describes them as having at first a distinct fibrillated structure as usual, but that this last gradually disappears and the nerve appears as a homogenous cord. But from our own investigations upon the terminal nerves of some insects, we suspect that this disappearance of the true fibrillæ may have been apparent and not real; for we have, in the cases referred to, thought that the like was true, but using higher powers with some reagents, the fibrillæ were seen. We think therefore that whatever may be made of termination of the nerve-fibre, the fibrilla structure is never lost.

Digestive Apparatus.—This structure, according to Meissner, presents so many peculiarities and is so widely different from any thing observed in other animals, that we almost relinquish any attempt to give even a brief description of it, without the aid of figures. In the first place, the alimentary canal has no anal or excretory passages, and therefore the food and assimilation must be such as to leave little or no so-called fæcal matter.

From the circular buccal orifice proceeds a semi-canal a short distance, when it passes into another structure. This semi-canal is the œsophagus. The structure into which it passes is a tube quite small at first but which soon expands and is filled with a finely granular spongy-like substance, and is alternately dilated from side to side into sacs. Through this laterally varicose tube the semi-canal of an œsophagus extends to its very end. Suppose then a tube with alternate lateral dilatations, filled with a spongy substance, and through which runs a semi-canal or half-tube like an œsophageal groove. Each of these dilatations has an inversion—a folding in of its internal membrane, producing an infundibuliform body in the dilatation itself. This body opens through a prolongation of the external membrane of the dilatation, which is continuous into a tube connecting with some adipose receptacles.

* *Axmann*, Beitr. z. mikroskop. Anat. u. Phys. d. Ganglien-Nervensystems des Menschen u. d. Wirbelthiere. Berlin, 1853.

To perhaps make the matter more clear, fancy the human alimentary canal without an anal opening, with alternate stomachs throughout its course, filled with a semi-solid granular substance; and that directly through it ran a half-tube; and that each stomach had a folding in of its internal lining forming a globular body, the neck of which passed off at right angles by a continuation of the peritoneum, into a tube which connected with receptacles of nutrition;—this would convey some idea of the most singular structure of the digestive canal of these animals.

The food passes along the semi-canal or groove, is gradually absorbed by the spongy substance filling the dilatation, thence passes into the invested body by endosmotic absorption and is then conveyed as assimilated material into the fat-receptacles which lie in the cavity of the body. These receptacles are storehouses of nutriment and are particularly enlarged and developed during the larval condition,—their contents being used for the formation of the sexual parts afterwards. Now as there is no vascular system in these animals, the dispersion of the nutrient material for the growth and substance of the various tissues must occur by permeation and endosmosis from the fat-bodies which extend over and between all the organs. This assimilation without any particular excretion is a remarkable fact; but it appears more conceivable when we bear in mind the economy of the animal. Its larval or parasitic state is like that of insects—merely preparatory for the ulterior changes of its full development. During this time its food is probably mostly pure fat which has only to be taken up and stowed away for material of the development of the reproductive organs. This last ensues during a quiescent state, and after the full discharge of the sexual functions, the animals probably die.

Genitalia.—Males.—The disparity in numbers of males and females was remarkably wide—our author having found only three males among several hundred specimens examined. He divides the internal organs into *testis*, *vas deferens*, *vesicula seminalis*, and *ductus ejaculatorius;* but these are all continuous, forming a cæcal tube stretching from the anterior portion of the body to the caudal extremity. The testis consists of the infundibuliform cæcal extremity of this tube and is lined with nucleated, epithelial (?) cells.

The external organs consist of two penises situated one on each side of the *Ductus ejaculatorius* in a sheath. They are composed of two somewhat curved half-canals disconnected when unprotruded with the internal organs; but when protracted, they form a more or less closed tube projecting beyond the external orifice of the duct.

Females.—Meissner divides the internal female organs, which are double, into five portions: *ovary*, *vitellus-organ*, *albumen-*

sac, *tuba*, and *uterus*. Their names indicate their respective functions and we can here enter into no description of their intimate structure.

In this connection should be noticed one point not a little remarkable, that is, a kind of hermaphroditism occurring in these animals.

Meissner found individuals which had perfectly well-formed internal female genital organs, but whose caudal extremity was wholly male. Thus, there were the penises, with their protractor and retractor muscles, their sheaths—in fact, all the external organs of the male, yet in these individuals no trace of internal male or of external female organs could be found. Moreover these organs present precisely the same characteristics as though in proper males and females, and had also a functional activity,—eggs being found in the ovaries, &c. But this anomaly was not ever found in the inverse sense, that is female external and male internal organs. Here then is presented the striking peculiarity of an animal having double systematically developed internal organs of one sex, and at the same time perfectly formed external organs of the other sex. This hermaphroditism, it will be seen, is like that of other animals only in name; for in these last the double sex is at the expense of the symmetry, one side being female and the other male, or it is due to modifications of analogous facts by different grades of development, thereby destroying generally the functional perfection and completeness of each or one of the forms of the sexual organs. But here we have a perfectly symmetrical female internally, with an equally symmetrical male externally, with no fusion of parts.

In regard to the development of the spermatic particles, our author's researches have been minute and quite complete. His results confirm the doctrines of Kölliker, Wagner, and our own: That is, parent sperm-cells in which are formed daughter-cells; in each of these last there is formed a spermatic particle. But Meissner is undecided if this formation occurs through a metamorphosis of the nucleus of the daughter-cell. Our own observations have led us fully to think that this nucleus is thus metamorphosed, as we have expressed in a former review.*

The development of the egg is quite note-worthy, as it shows, what we have before never clearly understood, viz: how botryoidal ovular masses are formed, and moreover carries out the beautiful analogy existing even to minute details, between the functions of the parent sperm-cell and the ovular cell. An ovular or egg-cell from the ovary is seen; it increases in size and its nucleus segments, several nuclei being formed. These nuclei approach the surface of this which we will now call the parent egg-cell; deverticula are given off from the cell-wall by a protrusion contain-

* This Journal, Nov., 1853, p. 393.

ing each a nucleus. These protrusions become constricted and at last appear as little, or daughter-cells, on the surface of the parent-cell. They now increase at the expense of this last, become pedunculated, and finally appear as larger pedunculated cells attached around a common, insignificant centre. These are the ova and form groups of variable number—Meissner having observed even twenty, though there are generally less. Thus formed, their peduncles break off and they pass from the ovary proper into the other sections of the genital tube.

There is one other point taken up in this connection by Meissner, and to which we briefly allude as it has been a subject of discussion on a former page.* We refer to the wonderful *Micropyle* of Keber, whereby it is alleged that the spermatic particles penetrate the interior of the egg and impregnate it. Meissner has seen nothing to justify the view that such a structure exists in the eggs of *Mermis* excepting the remains of the peduncle above mentioned, and this he is not sure of being hollow. Moreover even if it were hollow, it appears to us wholly different from the special structure insisted upon by Keber.

As to the embryonic development of *Mermis*, our author found nothing essentially different from what had been described by previous observers upon this order, (Grube, Leidy, &c.) There appears to occur no proper metamorphosis, and therefore the newly hatched embryos more or less closely resemble in form, &c., the adults.

In conclusion, we repeat what we said in the beginning, that this memoir is one of the most excellent of its kind we have ever seen, and the care, patience and fidelity displayed therein will ensure attention towards its author as one from whom much may be expected.

Art. XX.—*Correspondence of M. Jerome Nicklès, dated Paris, April* 30, 1854.

Death has recently made great havoc among the ranks of science in France. In the month of March alone, the Academy of Sciences lost two navigators, an astronomer, and a celebrated surgeon. Three of these eminent men died at an advanced age; the fourth, M. Mauvais, the astronomer, was but 45 years old, and came to a tragical end. They have been co-laborers in the common field, and we shall do a public service by giving some details of their life and labors.

Doctor Roux was born in 1780 at Auxerre. In 1795 his attainments in surgery were already so great that he was admitted as an assistant surgeon in the armies of the Republic. In 1797 he came to Paris and

* See a review on the doctrines of impregnation, in this Journal, Nov. No., 1853, p. 393.

gained the friendship of Bichat. In 1806 he entered as surgeon into the hospitals of Paris, and from that time he devoted himself with much distinction to the progress of his art. Endowed with great ardor and extraordinary activity, he was equally successful in scientific literature and in the amphitheater of surgery. He contributed much to the progress of this branch of the curative art, and was one of the first to give due prominence to the department of surgical anatomy. One of the most important processes brought forward by him, was the method of producing a union of the palate, (Staphyloraphy) an operation for the purpose of giving powers of speech to persons with a divided palate. The first person upon whom he performed this operation was an American physician, Dr. Stephenson, who after his cure, exhibited the operation to the Academy of Sciences.

A large number of new modes of surgical operation were contrived by M. Roux. He showed how to treat cases of lacerated perinæum, an affection before his time regarded as incurable. One of his most important specialties was the treatment of cataract, which he practised quite recently with as steady a hand as in youth. He preserved to the latest moment his characteristic vigor, and died suddenly in an apoplectic attack, distinguished as one of the greatest operators of the age.

Admiral Roussin.—The Admiral Albert Reine Roussin was born at Dijon, on the 21st of April, 1781. In 1793, at the age of twelve years, he was admitted as cabin boy to the floating battery "*République*," charged with the defense of Dunkirk. He commenced his career in the navy in the midst of engagements; and it was not until 1801 that he was able to devote himself to his studies. We will pass over the battles in which young Roussin bore a part, and which Capt. Duperrey has enumerated in detail upon the monument of the deceased. We will speak only of the services which M. Roussin has rendered to science and humanity. He made the hydrographical survey of the western coasts of Africa, rectifying the positions of the coast, and especially that of the shoal of Arguin, rendered famous by the melancholy shipwreck of the Medusa. During sixteen months on this commission, he explored about 400 leagues of coast, and published detailed charts, together with sailing directions, thereby rendering the navigation secure. In 1819, he explored the coast of Brazil, and furnished notes and directions relating to more than 900 leagues along the coast of eastern South America, thus forming "*le Pilote du Brésil*," a work which procured for him the distinction of a member of the Academy of Sciences. Admiral Roussin has added political honors to his scientific distinctions. He was Ambassador and afterwards Minister of Marine. He forced the entrance of the Tagus in 1831, and dispelled the notion then entertained, that the Tagus could not be attacked from the sea.

Admiral Roussin was a man of superior intellect, and of consummate skill in naval affairs, as expert in the art of producing as in the art of destroying.

Beautems Beaupré, the navigator, had not the double talent of Roussin. All his attention was devoted to the progress of navigation. He was born the 6th of August, 1766. He early evinced a decided taste

for naval life; and in 1791 he went with Admiral d'Entrecasteaux in the capacity of hydrographical engineer, in search of La Pérouse, on which voyage, he made a great step in the art of navigation, by substituting astronomical observations for the magnetic needle. He used the Reflecting Circle of Borda, and with much talent applied the problem relating to the angular capacity of a segment, which had been long familiar to geometricians, but had not been brought into practical use.

This long voyage was fertile in discoveries. To him is due the reconnoissance of the Kermadec Islands, the Archipelago of Santa Cruz, and of the Salomon Islands; of the coast of New Caledonia; of the island of Bougainville; of Boughton straits; nearly 300 leagues along the south coast of New Holland, and a small-boat survey of the bays of Van Diemens land, &c. &c.

These operations were finished just before the two frigates were captured by the Dutch. M. Beautems Beaupré was sent prisoner to the Cape of Good Hope, where he remained until 1796. Upon his return to France he resumed the continuation of the *Neptune de la Baltique*, which he had commenced before his departure. He afterwards published a survey of the Scheldt, and demonstrated that the seaport of Antwerp was accessible for vessels of the line of the largest class. Among the other labors of M. Beautems Beaupré, we mention only one,—namely, the hydrographical exploration of the southern and eastern coasts of France, a work which has commanded the admiration of all nautical men, and which won for its author from the English the distinguished title of the *Father of Hydrography*. The volume which records the results of this exploration received from its author the modest title of "Nouveau Pilote Francais." To him thanks are due, that the whole extent of the French coast may now be navigated with safety. In addition to these labors he collected the necessary materials for the publication of new charts of the French coasts, and brought together all the documents which might be useful hereafter in case of any new projects relative to navigation. This last labor comprises at present 527 volumes in 4to, and embraces all the documents necessary for preparing upon a gigantic scale, a plan of all the coast of France.

After the completion of these great labors in 1838, and their publication in 1843, the distinguished author aspired to a well merited repose, but he still continued to the end of his days to assist at the sittings of the Academy of Sciences, of which he had so long been one of the most assiduous members. He died in his 88th year, with the just renown of a good man.

Victor Mauvais, the astronomer, commenced life with the study of the law, but having an irresistible passion for the mathematical sciences, he renounced the duties of an advocate, and sought admission to the Observatory of Paris, to which he was nominated in 1836 by M. Arago. From that moment he gave himself exclusively to science. He discovered, successively, four comets, whose path through the heavens he watched with great assiduity during the whole time of their appearance. In the long series of observations which constitute the Archives of the Paris Observatory, the name of Mauvais is found in-

scribed upon almost every page. Of the 150,000 observations there recorded, over 30,000 are due to Mauvais.

In 1848 he was called by the department of Doubs to represent the people, and he remained in that capacity a member of the National Assembly until its dissolution. These duties did not interrupt his astronomical observations. He passed the day in parliamentary labors, and the night in observing the heavens. He had undertaken a serious labor, in the absolute determination of the position of the fundamental stars. Struck by the discrepancy which had been remarked between the right ascension of certain stars, he conceived the idea of a series of observations with the meridian circle. He had chosen two groups each of twenty stars, succeeding each other on the meridian, after an interval of twelve hours, and had observed their passage at intervals of six months, proposing to compare them afterwards with the sun in order to deduce the position of the equinoctial points. This important labor remains unfinished.

Incessant fatigues and night watchings had broken down the health of Mauvais. He suffered much from a disease of the intestines. The death of Arago and the unexpected separation of the Bureau of Longitudes from the Observatory affected him deeply. Disapproving the course taken in this case, he left the Observatory with MM. Mathieu and Laugier, son-in-law and nephew of Arago, and determined to suspend for some time his researches. Effort was made to induce him to resume his position in the Observatory; but being the friend of Laugier he preferred to share his fate. The care and anxiety which sprung from these circumstances sadly affected the health of Mauvais. From the stomach the malady went to his head, and in a paroxysm of burning fever he took his life by the discharge of a gun.

Mauvais was born at Maiche, a little village of the department of Doubs, on the 7th of March, 1809. He died the 23d of last March, and was consequently 45 years old.

The Paris Observatory.—Before the death of Arago the director of the Observatory was chosen annually. In consequence of the new measures, the Observatory will hereafter have a permanent director, and the Bureau of Longitudes will not have a voice in this nomination. Owing to these changes and to many others made in the regulations, some of the astronomers (MM. Mathieu, Mauvais and Laugier,) gave in their resignations and left the Observatory. The corps of the observers has thus been considerably changed. According to the new regulations it is to comprise, 1, A Director, 2, Four Astronomers, 3, A variable number of adjunct astronomers, pupils, and computers proportioned to the demands of the service. The director is M. Leverrier; MM. Faye and Yvon Villarceau have been named astronomers; and MM. Babinet and Goujon adjunct astronomers. A third adjunct has been lately appointed, M. Chacornac, a pupil in the Marseilles Observatory, discoverer of *Massilia* and *Phocéa*, and also author of a valuable chart of the Ecliptic. M. Chacornac entered on his duties on the afternoon of the 2d of March, and on the night of the 3d–4th of March he discovered, near the star *Spica Virginis*, a small planet which he had found at Marseilles on the night of the 4th of February, and marked upon one of the charts of the Ecliptic. The same planet was observed

on the second of March, by Mr. Marth, at the Observatory of Mr. Bishop, in London, and named *Amphitrite.*

Electricity.—Apropos to the notice which we have given in the No. for March, p. 265, on the passage of two unlike electrical currents over the same wire, the Abbé Moigno states the following fact mentioned in his treatise on the electrical telegraph and established by MM. Breguet and Gounelle, the 7th of April 1847. These experimenters attempted to send at the same moment telegraphic signals in inverse order, over the line from Paris to Rouen. The signals were reproduced on either side with the most perfect exactness. This experiment was many times repeated, and before a Commission of the Chamber of Deputies, and the circuit was always complete. It may be objected to this fact that the signals were not exchanged at the same indivisible instant, and that it only proves that the currents were of unequal intensity. Notwithstanding the adverse opinion of many physicists, it does not appear to us impossible that two electrical currents may circulate over a conducting wire at the same time, the proof of which will be found in the experiment cited in this Journal for March, p. 266.

The decomposition of water, the process of which is not always as simple as it would seem to be from theory, has received new light through a recent discussion. When care is taken thoroughly to refrigerate the acidulated water, which is the subject of experiment, it is remarked that the volume of the gases is no longer in the relation of 1O : 2H, as theory requires; but the hydrogen is apparently in excess, owing to a notable portion of oxygen being retained by the water. This fact has been simultaneously and separately determined by MM. Jamin, Leblanc, and Sozet.

M. Ch. d'Almeida, Professor of Chemistry in the Lyceum of Henry IV, has investigated the question *whether the deposit of copper found in a solution of the sulphate of that metal when subjected to electrolysis is the product of electrolytic decomposition, or of the decomposition of the copper salt by nascent hydrogen?* By very ingenious experiments, the details of which cannot be given here, M. d'Almeida has proved that in a neutral solution, the neutrality of which is carefully preserved during the operation, the metal deposited at the negative pole is essentially derived from the direct decomposition of the salt. In acid solutions, on the contrary, the reduction is the result of nascent hydrogen. These results lead to the important practical conclusion, that in the electro-chemical decomposition of a metallic salt by the battery, the solutions should be preserved as neutral as possible. This observation is specially applicable in the study of electro-chemical equivalents.

M. Gaugain has studied some of the forms of batteries with reference to the causes which produce variation in the electro-motive force. In comparing the several couples of a thermo-electric battery, he detected between them a variation equal to 12 or 14 per cent. of the mean electric force. He ascribes this variation to a difference in the texture of the Bismuth, which is more or less crystalline according as the metal has been cooled more or less rapidly. These comparisons were made by means of a galvanometer interposed between the couples placed in opposite relations, so that each cup of the galvanometer receives the + pole of one couple and the — pole of the other.

M. Gaugain has also compared the battery of Wheatstone with that of Daniell, adopting the method of the opposition of piles. According to this observer, in Wheatstone's battery, the cause which has the most influence upon the electro-motive force is the diaphragm. This cause does not affect the electric force of Daniell's battery, which like other batteries employing two fluids, is modified by the phenomena of chemical affinity. The author has observed that the electro-motive force of Daniell's battery is diminished by the agitation of the solution in which the zinc plate is placed.

Various Memoirs.—Among the numerous communications which have been made to the Academy, there are new facts relating to the absorption or non-absorption of the nitrogen of the atmosphere by plants. M. Boussingault rejects the theory of absorption, which his numerous experiments have failed to verify, and M. Dumas agrees with him in opinion. But a young chemist has appeared, who sustains with courage the opposite view, and appeals to many facts and experiments in his support. On which side is the truth? The question is too nearly poised, soon to be solved. The debate has produced a sensation equalled only by that of another memoir—one on the preparation of Aluminium, by M. H. St. Claire Deville. Deville, who is skillful in applying heat above all French chemists, has contrived a method of preparing aluminium economically, similar to the process for obtaining potassium and sodium, using a very high temperature. We propose in another communication to give views of the lamp and forge which Deville uses in his laboratory for fusing the most refractory bodies. The note of Deville has brought out a number of communications on the subject of the economical preparation of Aluminium, none of which have yet been verified, and we wait for positive facts before touching upon them.

While Deville has been occupying himself with Aluminium, his assistant, M. Debray, has been studying Glucinum, which metal (as well as Aluminium) M. Wöhler was the first to obtain separate, although in an impure state, if we may judge from the properties of this metal mentioned by M. Debray. According to this chemist, Glucinum is lighter than Aluminium; its specific gravity is 2·1. It looks like zinc, but is less fusible, non-volatile, unalterable at the ordinary temperature and oxydizes on the surface at the blowpipe temperature without affording the phenomena of ignition produced under the same circumstances by zinc and iron. Concentrated nitric acid attacks it only when hot, and diluted acid under no circumstances. Chlorhydric and sulphuric acids, even diluted, dissolve it, disengaging hydrogen. Potassa dissolves it even when cold; ammonia is without action.

Table Turnings.—Much interest has been excited by a paper of M. Chevreul's to the Academy of Sciences, taken from the introduction of a work now in press, in which he treats of the phenomena of table-turnings. This distinguished chemist does not confine himself to this subject alone, but connects with it, the "Exploring Pendulum," and "Divining Rod," and he endeavors to reduce these phenomena to certain rational facts. In 1812, he noted the phenomena of the pendulum in a letter addressed to Ampère, and showed that the pendulum movement was produced only when the eye of the experimenter was fixed

on the instrument; and he endeavored to prove thereby that the motion was due to a play of the muscles. The work of M. Chevreul should properly be read and submitted to a commission; but some members of the Academy have objected to the consideration of a subject connected to such an extent with superstition. M. Chevreul believes that the question may be treated without going out of the domain of true science, agreeing with Arago and Faraday, and regards it not unworthy of a man of science to occupy himself with any demonstrated facts in order to search out their relation to other facts.

Works of Arago.—The works of Arago have begun to appear, published by Gide and Baudry. The first volume is on sale. It opens worthily the remarkable series of papers which will appear successively in the 12 volumes to be issued, as the posthumous works of a man who like Franklin was distinguished both in the State and in Science. This two-fold character of the man will appear throughout the published pages. The first article is entitled "The History of my Youth." It is in fact a romance, in which are interwoven the adventures of Arago in Spain, and also piquant details relating to many of the principal scientific subjects of the epoch. It contains also his Eulogies on Fresnel, Volta, Young, Fourier, Watt, and Carnot. Each of these eulogies is at the same time a complete treatise on one or several branches of science, illustrated by the life of the person of whom it treats. Thus under that of Fresnel, we find the most recondite questions in optics treated with a perspicuity and charm which render it attractive and intelligible to persons in the least familiar with this branch of study; as for instance, *Double Refraction*, *Polarisation*, *Diffraction*. In the eulogy of Watt, the *Steam Engine* is the subject; in that of Volta, it is the *Galvanic pile;* in that of Fourier, it is *Heat;* that of Carnot, it is mechanics in general, and military engineering; in that of Young, the subject of the interference of light, and Egyptian hieroglyphics. There are also other points touched upon, which give the eulogies a historic interest; as with Carnot, who organized the victorious republican armies, and with Fourier, the friend of Kleber in Egypt, who took a part too direct to be passed unnoticed, in the great events at the close of the last century and commencement of this. No one better than Arago appreciated these great characters. He has depicted with the hand of a master, the services they rendered to the cause of humanity, their discoveries in science, and their virtues.

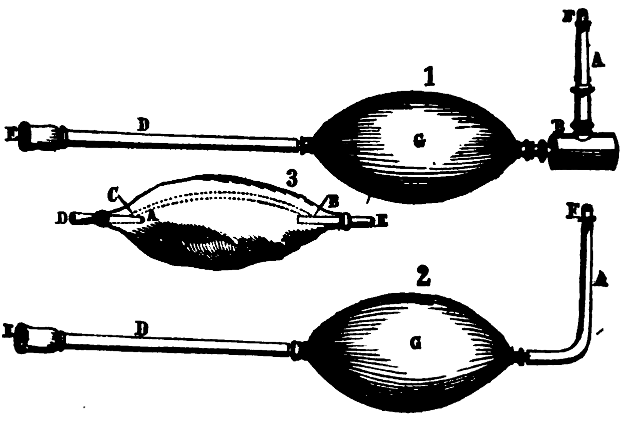
The volume is preceded by an introduction by M. Alexander von Humboldt, whose friendship for Arago dates back nearly half a century.

Poisonous effects of Carbonic Oxyd.—At the World's Fair at London, there were exhibited certain samples of iron and steel, of peculiar property, prepared by means of spongy iron, which was obtained by reducing the ore by using a mixture of hydrogen and carbonic oxyd produced by the action of steam on incandescent charcoal. The iron thus prepared, is very oxydizable, and decomposes water with an energy far exceeding ordinary iron. The author of the process, M. Chenot, proposes to use this spongy iron in obtaining aluminium, and he promises even to prepare this metal at the order of the Academy, by the kilogram for industrial purposes. We state this by way of introduction, in order to justify the observation communicated by this same

chemist, relating to the poisonous effects of carbonic oxyd. Having been for many years occupied with this gas, he has proved upon himself its deleterious qualities; and he announces that he is at this very time suffering from a new poisoning with this gas. Mixed with atmospheric air, the gas is little dangerous, but in the pure state it is a violent poison. The person taking it, says the author, falls as if struck by lightning; his eyes roll up in their sockets, his members become stiffened, the skin discolored, the veins inflated and looking blackish under the skin; the sensibility becomes extreme, and life is exalted in activity: the brain seems to be compressed, and terrible pains are felt in the thorax. The lassitude continues general for several days; sleep becomes heavy and troubled, and there are severe cramps in the legs and toes. These effects are continued for months; the person appears sad and dejected; any noise produces a nervous shock like an electric discharge.

The antidote used by M. Chenot is gum or marsh-mallow water; bathing gives much relief. These remedies alleviate but do not remedy the effects of the poison. For several weeks, now, since M. Chenot inhaled this gas, through the breaking of a manometer, he has suffered from an insurmountable feebleness and loathing; and the least touch, even his own, produces on him severe irritation.

Blowpipe with a continued Blast.—The blowpipe here figured has the merit of enabling the operator to keep up a continued blast, without the



practise required for the ordinary blowpipe, and without fatigue. M. de Luca observes that it is only necessary to blow as in an ordinary tube. The peculiarity of the blowpipe consists in the addition of a bag of vulcanised india rubber, G, having within a valve A (fig. 3), which opens from without inward, and closes from within outward, and is placed at the extremity of the mouth-tube. The valve admits the air, but prevents it from returning. The india rubber bag, by its elasticity, throws a stream out regularly by the aperture; and it serves also in place of the ordinary condenser. Figure 1 is the blowpipe with the india rubber bag; in figure 2, the cylindrical recipient is removed.

M. de Luca is a refugee Neapolitan, and one of the directors of the *Ateneo Italico.* The application which he has here made of the vulcanised india rubber bag appears to be copied from the "Cornemuse," a picturesque musical instrument much used among the Neapolitan herdsmen.

Use of Oxygen in Asphyxia.—Two physicians, MM. Faize and Genetti, who are experimenting on the effects of gaseous bodies on the animal economy, have observed that oxygen restores life to animals in which it has been nearly extinguished, as it restores brilliancy to a burning body which is nearly out. They have operated in various cases, such as asphyxia by chloroform, carbonic acid gas, and even strangulation. When life appeared to be extinct, so that the beating of the pulse and movement of the chest were hardly sensible, they have injected oxygen into the lungs and almost immediately the effect was apparent and there was a restoration to life. In some comparative experiments, made under like conditions, they have shown that the atmospheric air was almost always without effect, and in no case can its action be compared to that of oxygen which is all-powerful and instantaneous.

Local Anesthesis.—The anesthetic experiments spoken of in my last communication, although they have not led to a successful result, still have suggested an application which appears to resolve the problem in a different manner. It is now attempted to produce local anesthesis by *cold.* The mode of operation is very simple. The ether is applied, drop by drop, to the part to be subjected to the operation, and evaporation is promoted by a current of air. The ether is believed to act only as a means of refrigeration; but however this be, the process employed by M. Richet at the Hotel Dieu in the removal of a tumor appears to have been attended with success, and it is reported with details in the medical journals. After cooling with the ether for four minutes, the surgeon, perceiving that the part was insensible, plunged in his knife and made an incision near 5 centimeters in length; and the patient felt no pain although awake, and was hardly aware of the operation.

Photography and Stereoscopy.—At one of the late sessions of the Academy, M. Claude exhibited some stereoscopic views representing groups of persons with remarkable effect. He had calculated the distance so well, that on the instant of applying the eyes to the glasses, the single image with three dimensions was seen, and not two images.

At another session, M. Elie de Beaumont, Perpetual Secretary, presented to the Academy, on the part of M. Frederic Martens, engraver and photographer at Paris, a number of photographs on paper, representing the Swiss glaciers and mountains, as the glaciers of Monte Rosa, Mt. Cervin, etc. This last mountain has the form of a pyramid, and, as the Swiss say, resembles a bayonet. The views represent this pyramid as obtuse and eroded, seemingly false if common observation is correct, but in fact right, the error being that of the observer, to whom, as has been often remarked, vertical objects, such as mountains and distant edifices, always appear steeper and more elevated or pointed than they really are.

Bath for bringing out Photographs.—Sulphate of iron has been often recommended for bringing out proofs on taking them from the camera.

The use of this sulphate excludes that of the hyposulphite of soda for the solution of the iodid of silver not modified by the light; in fact, the sulphate of iron should always be acidulated; and consequently it decomposes this hyposulphite producing a deposit of sulphur, which forms with the silver a sulphuret, especially injurious in the production of direct positive proofs. This effect will not take place if the proof has been carefully washed after leaving the bath of sulphate of iron; still the decomposition of the hyposulphite goes on spontaneously, and if the precaution is not taken of filtering the solution of hyposulphite, the same accidents occur.

To avoid this difficulty, M. Adolph Martin dissolves the iodid of silver that has been acted on by the light by using a solution of cyanid of silver in cyanid of potassium. Cyanid of potassium alone does not produce the same result; for in the presence of sulphate of iron, this simple cyanid gives origin to the ferro-cyanid of potassium, which affords Prussian blue with the least trace of sulphate of iron in excess, and tinges the proofs of an intense blue color. If on the contrary the cyanid of silver dissolved in the cyanid of potassium is used, there is produced indeed a little ferro-cyanid of potassium which in the presence of the salts of silver is decomposed, causing a deposit of hydrated peroxyd of iron of a yellow color; but it adheres but slightly to the proof, and if the quantity of hydrate is not large, a simple washing for a while in ordinary water will remove it.

A new red dye for dyeing wool.—On treating uric acid with nitric acid and then with ammonia, Proust obtained, at the beginning of this century a red substance which he called purpurate of ammonia. MM. Wöhler and Liebig have since studied this substance and separated a compound of a fine red color represented by the formula $C^{12}N^5H^6O^8$, which they call *murexid.* This material, which is easily prepared from alloxane and ammonia, has been applied to dyeing wool, for which it furnishes a red color richer than that of cochineal. The author of this process is M. Albert Schlumberger of Mulhouse (Haut-Rhin). His experiments have been verified by a commission from the Industrial Society of that town.

The red of murexid fixes itself on the wool without a mordant. After imbibing the alloxane, a bit of wool drying exposed then to ammoniacal vapors and afterward to the heat of a steam drum or heated iron, the red color is seen to be immediately developed. It is indispensable that the ammonia should be applied before the heat. The color is not produced if the specimen impregnated with alloxane has been heated; water removes this last, while it is wholly without action on murexid.

Although this color requires no mordant, M. Schlumberger has however found that a mordant may be useful. That which he prefers is a bath consisting of equal parts of bichlorid of tin and oxalic acid, the whole forming with water a solution marking 1° Beaumé. The mordants made with protochlorid of tin give indifferent results.

Under the action of the sun's rays, the Commission found the red of murexid to be more stable than that of cochineal, and they do not hesitate to recommend the use of it in dyeing gobelins in preference to cochineal, although the new red is just now dearer than the old, since

uric acid or alloxane are now known only in the laboratories. The price will be cheapened when it is made a branch of industry, and it is certain that guano will here find a new demand.

The red of murexid resists the action of alcohol, ether, and the acetic and oxalic acids. Muriatic, nitric and sulphuric acids destroy it; but if the destruction is not complete, the color may be restored by means of ammonia. Caustic alkalies destroy it rapidly. Reducing substances, such as protochlorid of tin, sulphate of iron, cause it to disappear; but the color may be restored by means of ammonia.

Cotton, whether with a mordant or not, whether animalized (Broquette's process) or mixed with wool, is not dyed with murexid. Impressed with the alloxane and then treated with hot iron, cotton is colored it is true of a rose tint, and the color is deepened with ammonia; but the color does not stand washing, water causing it wholly to disappear.

Silk does not take the amaranth color of murexid; it becomes yellowish rose. M. Schlumberger recognizes in this property a means of distinguishing cotton, silk, and wool.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *Thermic Researches on Hydroelectric Currents.*—Under this title Favre has published the first part of an investigation which promises to throw much light upon a very obscure part of the theory of the voltaic pile. In the present memoir the author considers the calorific effects developed in the galvanic circuit in their relations to the chemical action which gives rise to the current, and to the electro-chemical decompositions which the current may produce. The results obtained are as follows.

(1.) The chemical actions generated by the elements in activity are the exclusive source of the calorific effects produced by the battery.

(2.) All the chemical actions which take place in the voltaic couple concur simultaneously in the production of the current.

(3.) The disengagement of heat produced by the passage of voltaic electricity through metallic conductors is rigorously complementary to the heat confined in the elements of the battery, to form a sum equal to the total heat corresponding simply to the chemical actions independent of all electricity transmitted.

(4.) The chemical decompositions which the passage of the electricity through the circuit may effect, always bring into play quantities of heat the same as those which accompany chemical decompositions effected by other influences. The heat brought into play in the act of these decompositions results always from a draft made upon the total heat disengaged by chemical actions in the voltaic apparatus.—*Ann. de Chimie et de Physique*, xl, 293, *March*, 1854.

2. *On the double refraction temporarily produced in isotropic bodies, and on the relation between mechanical and optical elasticity.*—Werth-

ZIM has communicated an elaborate memoir upon these important subjects; we must however refer to the original paper for the details of the experiments and content ourselves with stating the results which, in the author's own words, are the following.

(1.) The double refraction artificially produced, either by traction or by compression, is, for the same substance, proportional to the linear changes which the mechanical force produces in the direction of the principal axes, and consequently is also proportional to the changes of volume of the body.

(2.) The temporary lengthenings and shortenings which a given weight produces, according as it acts by traction or by compression, are neither rigorously equal to each other, nor exactly proportional to these weights, so long as these are relatively small; but these differences disappear as soon as the weights become somewhat considerable, and long before those which produce the first sensible permanent alterations.

If we lay off the weights on the axis of abscissas and the corresponding lengthenings and shortenings upon the axis of ordinates, the first below and the second above this axis, we obtain two similar, if not equal, curves, the first of which is convex and the second concave toward the axis of abscissas. These curves insensibly become straight and for linear charges which are scarcely measurable by ordinary means, become undistinguishable from a straight line which represents the proportionality between the charges and their temporary effects. These facts are confirmed by direct experiments made by different observers, experiments the results of which were only too uncertain to clearly exhibit the truth of the law; this confirmation results especially from the experiments of Mr. Hodgkinson when we calculate them so as only to take the temporary effects into consideration, and when we pay attention to the causes of errors which influence all direct experiments made by compression.

(3.) The optic axes correspond with the mechanical axes in all truly isotropic bodies, whether they have been endowed with negative double refraction by pressure or with positive double refraction by traction. The double refraction or the difference of path of two rays, ordinary and extraordinary, may be determined very accurately by means of the dull complementary tints which the two images of a white ray take, when the principal sections of the polarizing Nicols prism and the doubly refracting analyzing prism make an angle of 45° with the direction of the force which is applied to the body placed between these two prisms. In these two cases the colors ascend with the changes following exactly the series of the colored rings of Newton; for measurements, however, we can scarcely make use of these colors of more than the first seven half rings: the colors of the transmitted rings are those of the ordinary image while the tints of the reflected rings correspond to the extraordinary image.

(4.) Making no account of the small differences which have just been pointed out, the temporary double refraction is independant of the height and length of the piece, proportional to the weight applied and to the doubly refracting power of the substance, and reciprocally proportional to its breadth and to its coefficient of mechanical elasticity.

(5.) The doubly refracting power of an isotropic substance which has become temporarily doubly refracting, cannot be expressed but by the difference between its ordinary and its extraordinary index; this difference changes its sign only according as we apply pressure or traction, which would not be the case if we wished to express the doubly refracting power by a function of the two indices other than the difference of their first powers.

(6.) The dispersion of double refraction is insensible for substances which have been submitted to experiment.

(7.) Glasses which had been submitted to the operation of compression while in a pasty state, ceased to be optically homogeneous bodies, and this alteration, entirely distinct from what is called tempering, did not always disappear by annealing.

(8.) The doubly refracting power is not the same for different isotropic substances; no connection can be established between this power and the ordinary index of refraction or even the density.

(9.) By analogy with the ordinary or mechanical coefficient of elasticity E, we call coefficient of optical elasticity C the ratio between the charge applied to the unit of surface and the double refraction which it produces; we have then the simple equation,

$$I_o - I_e = \frac{E}{C}$$

which serves to determine the doubly refracting power

$$p = \pm (I_o - I_e).$$

(10.) The value of the doubly refracting power being once known for a substance, we may use the phenomena of double refraction to determine any one of the quantities which enter into the equation

$$\pm P (I_o - I_e) = d \, . \, E \, . \, La,$$

where P is the charge, I_o and I_e are the indices for the ordinary and extraordinary ray, d is the difference of path, and La the breadth of piece employed.

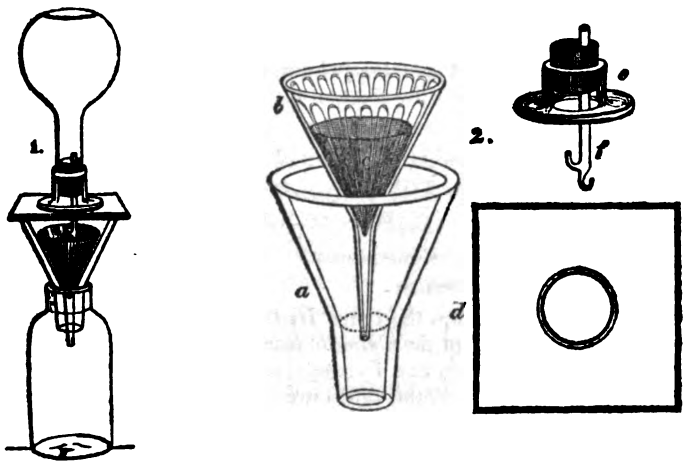
(11.) The most important of these applications consists in the determination of the force P, whatever be its magnitude and mode of action. The chromatic dynamometer gives immediately, and without the employment of any coefficient of correction, the effective pressure which is exercised by a screw press, fly-wheel, hydraulic press, lever, &c. it will serve to make known for all these machines the ratio between the useful and the theoretical effect, to graduate ordinary manometers accurately, and even to measure living forces.

(12.) The same formula would serve to determine the coefficient of mechanical elasticity if we had a direct method of finding the extraordinary index Ie; but in the mean time it has permitted me to establish the optical coefficient of the diamond, and to fix certain limits between which its mechanical coefficient is comprised.

(13.) The difference of path d being independent of the length of the undulation λ, if the ratio $\frac{d}{\lambda}$ is to remain the same for the different values of λ, P must vary proportionally to λ; which furnishes an easy method of determining the lengths of undulations, and of deciding whether a given ray is homogeneous, or what are the different simple rays of which it is composed.

(14.) The phenomena of magnetic rotation disappear in all bodies in proportion as they cease to be mechanically homogeneous and optically isotropic; it is to be remarked that among bodies which are naturally doubly refracting or which are rendered so by the employment of a mechanical force, those which have the most energetic powers of rotation are those which are at the same time endowed with the feeblest doubly refracting powers.—*Ann. de Chimie et de Physique*, xl, 156, *February*, 1854. W. G.

3. *On a new Filtering Apparatus;* by Prof. J. P. Cooke.—It is frequently important in chemical analysis to conduct the process of filtration either in vacuo or in a neutral gas, especially in an atmosphere free from carbonic acid. In order to overcome certain difficulties, I have been led to contrive an apparatus for this purpose, which, as I believe, is superior to all others now in use, both on account of its simplicity and its cheapness. The complete apparatus is represented in Fig. 1, and some of its parts enlarged appear in Fig. 2. It consists of a wide-mouthed



glass bottle, into the neck of which is firmly ground with emery a funnel (*a*, fig. 2) having a short but large spout. These are made sufficiently thick to resist the atmospheric pressure, and the rim of the funnel is ground so that the apparatus may be closed air-tight by means of a g ass plate. Within the outer funnel the common filtering funnel is placed resting loosely against its side so as to allow a free passage of air.

In order to wash a precipitate, or to produce a vacuum in the interior of the apparatus, a glass plate with a hole an inch or more in diameter drilled through its centre (*d*, fig. 2) is substituted for the covering plate. Through this passes the tube of a washing bottle (*f*, fig. 2) The washing bottle is made in the ordinary way except that it is fitted with a cork, which projects about an inch above the neck. This upper end of the cork fits the neck of a glass plate ground on the under side as is represented at *e*, fig. 2. The plate is about three inches in diameter, and when resting on the plate *d*, (see fig. 1) covers the opening completely and permits sufficient lateral motion to bring the stream of water on different parts of the precipitate. A vacuum is readily ob-

tained by connecting the apparatus with an air pump by means of a flexible tube having a brass plate at the end sufficiently large to cover the opening in the plate *d*, and by an easy manipulation the interior may afterwards be filled with hydrogen, or any other gas.

Numerous processes in which this apparatus may be advantageously applied will suggest themselves to the chemist. I have found it very useful in washing precipitates with volatile liquids, (as in the separation of fats) in preparing many metallic protoxyds, in separating the alkaline earths from alumina and sesquioxyd of iron, &c. Moreover it will be found useful in protecting both precipitate and filtrate from the dust and fumes of a laboratory in many cases even when exclusion of air is not essential. The apparatus is manufactured by the New England Glass Co., and sold by Mr. J. M. Wightman, of Boston, at a price not exceeding that of a filtering stand with brass rings.

II. Mineralogy.

Chemical Contributions to Mineralogy; by James D. Dana.

Chlorite Section of Hydrous Silicates.—Among *Anhydrous* Silicates, there is a group of species (which the writer has called the Andalusite Section), in which the oxygen of the bases exceeds that of the silica. It includes

1. Staurotide $\bar{Al}\,\dddot{Si}^{\frac{1}{3}}$, Andalusite $\bar{Al}\,\dddot{Si}^{\frac{2}{3}}$, Topaz $\bar{Al}\,\dddot{Si}^{\frac{3}{5}}$;–Trimetric and homœomorphous.
2. Kyanite $\bar{Al}\,\dddot{Si}^{\frac{2}{3}}$, Sillimanite $\bar{Al}\,\dddot{Si}^{\frac{2}{3}}$; also $\bar{Al}\,\dddot{Si}^{\frac{3}{4}}$, $\bar{Al}\,\dddot{Si}^{\frac{5}{6}}$, $\bar{Al}\,\dddot{Si}^{\frac{1}{2}}$—Triclinic.
3. Euclase $\bar{R}\,\dddot{Si}^{\frac{3}{4}}$, Sphene $(Ti+\dot{C}a)\,\dddot{Si}^{\frac{2}{3}}=(RO^2+RO)\,\dddot{Si}^{\frac{2}{3}}=\bar{R}\,\dddot{Si}^{\frac{2}{3}}$;—Monoclinic.
4. Tourmaline $(\dot{R}^3, \bar{R}, \bar{B})\,\dddot{Si}^{\frac{3}{4}}$;—Rhombohedral.
5. Gehlenite $(\dot{R}^3, \bar{R})\,\dddot{Si}^{\frac{2}{3}}$;—Dimetric.

Parallel with this Group, there are *Hydrous* Silicates, and they constitute what may be called the *Chlorite Section.* In a few among them, as Euphyllite, Hisingerite, and Pyrosclerite (in part), there is the ratio 1 : 1; but in all the others, the ratios are either 1 : $\frac{3}{4}$, 1 : $\frac{2}{3}$ or 1 : $\frac{1}{2}$.

The oxygen ratios of the species, for the protoxyds, peroxyds, silica, and water, according to the most recent analyses, together with the accepted formulas, are as follows:

	$\dot{R}$	$\bar{R}$	$\dddot{Si}$	$\dot{H}$	
Hisingerite (A), - - -	1	2	3	2 (or 3)	$\dot{F}e^3\,\dddot{Si}+2\bar{F}e\,\dddot{Si}+6(\text{or } 9)\dot{H}$
" (B), - - -	3	1	4	3	$3\dot{F}e^3\,\dddot{Si}+\bar{F}e\,\dddot{Si}+9\dot{H}$
Thuringite, - - - - - -	2	3	3	2	$2\dot{R}^3\,\dddot{Si}+\bar{R}^3\,\dddot{Si}+6\dot{H}$
Pyrosclerite (A), - - -	4	2	6	4	$2\dot{R}^3\,\dddot{Si}+\bar{R}\,\dddot{Si}+6\dot{H}$
" (B), - - -	6	4	7½	5	
Euphyllite, - - - - - -	1	8	9	2	
Margarite, - - - - - -	1	6	4	1	$\dot{R}^3\,\dddot{Si}+8\bar{Al}^2\,\dddot{Si}+8\dot{H}$
Clinochlore, Chlorite, - - - - -	5	3	6	4	$\dot{R}^3\,\dddot{Si}+\bar{R}\,\dddot{Si}+2\dot{M}g\,\dot{H}^2$
Delessite, - - - - - -	2	2	3	2	
Ripidolite, - - - - - -	3	3	4	3	$3\dot{R}^3\,\dddot{Si}+\bar{R}^2\,\dddot{Si}+9\dot{H}$
Aphrosiderite, - - - - -	3	3	4	2	
Chloritoid (A), - - - - -	1	2	2	1	$\dot{R}^3\,\dddot{Si}+\bar{Al}^3\,\dddot{Si}+3\dot{H}$
" (B), - - - - -	1	3	2	1	
Cronstedite, - - - - - -	1	1	1	1	
Sideroschisolite, - - - -	0	2	1	1	
Clintonite,* - - - - - -	3	5	2	1	

* The analyses of this species by Meitzendorff give the oxygen ratio for $\dot{R}$, $\bar{R}$, $\dddot{Si}$, $\dot{H}$, 12·22 : 20·52 : 8·64 : 3·85, which equals quite closely 3 : 5 : 2 : 1.

It will be observed that in several of the above formulas the two silicates of the formula differ widely in oxygen ratio; thus, in Ripidolite, the protoxyd silicate has the ratio 1 : 1; while the peroxyd has the ratio $3:1=\bar{R}\,\bar{Si}^{\frac{1}{3}}$, a very doubtful possibility under any circumstances.

Viewing these same ratios in a different manner, that is, taking the ratio between the sum of the oxygen of the protoxyds and peroxyds and that of the silica, more simple results are obtained, and the parallelism with the Andalusite Section of Silicates is brought out. We thus make out four groups, as follows:

1. General Formula $(\dot{R}^3, \bar{R})\,\bar{Si} + Aq.$
2. " " $(\dot{R}^3, \bar{R})\,\bar{Si}^{\frac{3}{4}} + Aq.$
3. " " $(\dot{R}^3, \bar{R})\,\bar{Si}^{\frac{2}{3}} + Aq.$
4. " " $(\dot{R}^3, \bar{R})\,\bar{Si}^{\frac{1}{2}} + Aq.$

In *Clintonite*, whose ratio corresponds to $3\dot{R}^3, 5\bar{Al}, 2\bar{Si}, 3\dot{H}$, if $2\bar{Al}$ replace silica, the species has the general formula $(\dot{R}^3, \bar{R})\,\bar{Si}^{\frac{2}{3}} + Aq$, like Chloritoid (A).

Again, in *Thuringite*, if one third of the alumina replace silica, the formula may be $(\dot{R}^3, \bar{R})(\bar{Si}, \bar{Al})$ analogous to that of Hisingerite.

Writing out the proportion of $\dot{R}^3$ to $\bar{R}$, these formulas become:

Group I. *General Formula* $(\dot{R}^3, \bar{R})\,\bar{Si} + Aq.$

Hisingerite (A),	$\dot{Fe}^3\,\bar{Si} + 2\bar{Fe}\,\bar{Si} + 9\dot{H}$	$=(\frac{1}{3}\dot{Fe}^3 + \frac{2}{3}\bar{Fe})\,\bar{Si} + 3\dot{H}$
" (B),	$3\dot{Fe}^3\,\bar{Si} + \bar{Fe}\,\bar{Si} + 9\dot{H}$	$=(\frac{3}{4}\dot{Fe}^3 + \frac{1}{4}\bar{Fe})\,\bar{Si} + 2\frac{1}{4}\dot{H}$
Thuringite,	$\dot{Fe}^3\,\bar{Si} + \bar{R}(\bar{Si}, \bar{Al}) + 3\dot{H}$	$=(\frac{1}{2}\dot{Fe}^3 + \frac{1}{2}\bar{R})(\bar{Si}, \bar{Al}) + 1\frac{1}{2}\dot{H}$
Pyrosclerite (A),*	$2\dot{R}^3\,\bar{Si} + \bar{Al}\,\bar{Si} + 6\dot{H}$	$=(\frac{2}{3}\dot{R}^3 + \frac{1}{3}\bar{Al})\,\bar{Si} + 2\dot{H}$
Euphyllite,	$\dot{R}^3\,\bar{Si} + 8\bar{Al}\,\bar{Si} + 6\dot{H}$	$=(\frac{1}{9}\dot{R}^3 + \frac{8}{9}\bar{Al})\,\bar{Si} + \frac{2}{3}\dot{H}$

II. *General Formula* $(\dot{R}^3, \bar{R})\,\bar{Si}^{\frac{3}{4}} + Aq.$

Pyrosclerite (B),	$3\dot{R}^3\,\bar{Si}^{\frac{3}{4}} + 2\bar{R}\,\bar{Si}^{\frac{3}{4}} + 7\frac{1}{2}\dot{H}$	$=(\frac{3}{5}\dot{R}^3 + \frac{2}{5}\bar{R})\,\bar{Si}^{\frac{3}{4}} + 1\frac{1}{2}\dot{H}$
Clinochlore,† Chlorite,	$5\dot{R}^3\,\bar{Si}^{\frac{3}{4}} + 3\bar{R}\,\bar{Si}^{\frac{3}{4}} + 12\dot{H}$	$=(\frac{5}{8}\dot{R}^3 + \frac{3}{8}\bar{R})\,\bar{Si}^{\frac{3}{4}} + 1\frac{1}{2}\dot{H}$
Delessite,	$\dot{R}^3\,\bar{Si}^{\frac{3}{4}} + \bar{R}\,\bar{Si}^{\frac{3}{4}} + 3\dot{H}$	$=(\frac{1}{2}\dot{R}^3 + \frac{1}{2}\bar{R})\,\bar{Si}^{\frac{3}{4}} + 1\frac{1}{2}\dot{H}$

III. *General Formula* $(\dot{R}^3, \bar{R})\,\bar{Si}^{\frac{2}{3}} + Aq.$

Ripidolite,	$\dot{R}^3\,\bar{Si}^{\frac{2}{3}} + \bar{R}\,\bar{Si}^{\frac{2}{3}} + 3\dot{H}$	$=(\frac{1}{2}\dot{R}^3 + \frac{1}{2}\bar{R})\,\bar{Si}^{\frac{2}{3}} + 1\frac{1}{2}\dot{H}$
Aphrosiderite,	$\dot{Fe}^3\,\bar{Si}^{\frac{2}{3}} + \bar{R}\,\bar{Si}^{\frac{2}{3}} + 2\dot{H}$	$=(\frac{1}{2}\dot{Fe}^3 + \frac{1}{2}\bar{R})\,\bar{Si}^{\frac{2}{3}} + \dot{H}$
Clintonite,	$\dot{R}^3(\bar{Si}, \bar{Al})^{\frac{2}{3}} + \bar{R}(\bar{Si}, \bar{Al})^{\frac{2}{3}} + \dot{H}$	$=(\frac{1}{2}\dot{R}^3 + \frac{1}{2}\bar{R})(\bar{Si}, \bar{Al})^{\frac{2}{3}} + \frac{1}{2}\dot{H}$
Chloritoid (A),	$\dot{R}^3\,\bar{Si}^{\frac{2}{3}} + 2\bar{Al}\,\bar{Si}^{\frac{2}{3}} + 3\dot{H}$	$=(\frac{1}{3}\dot{R}^3 + \frac{2}{3}\bar{Al})\,\bar{Si}^{\frac{2}{3}} + \dot{H}$

IV. *General Formula* $(\dot{R}^3, \bar{R})\,\bar{Si}^{\frac{1}{2}} + Aq.$

Chloritoid (B),	$\dot{R}^3\,\bar{Si}^{\frac{1}{2}} + 3\bar{Al}\,\bar{Si}^{\frac{1}{2}} + 3\dot{H}$	$=(\frac{1}{4}\dot{R}^3 + \frac{3}{4}\bar{Al})\,\bar{Si}^{\frac{1}{2}} + \frac{3}{4}\dot{H}$
Margarite,	$(\dot{R}^3, \dot{H}^3)\,\bar{Si}^{\frac{1}{2}} + 3\bar{Al}\,\bar{Si}^{\frac{1}{2}}$	$=(\frac{1}{4}(\dot{R}, \dot{H})^3 + \frac{3}{4}\bar{Al})\,\bar{Si}^{\frac{1}{2}}$
Cronstedite,	$\dot{R}^3\,\bar{Si}^{\frac{1}{2}} + \bar{R}\,\bar{Si}^{\frac{1}{2}} + 3\dot{H}$	$=(\frac{1}{2}\dot{R}^3 + \frac{1}{2}\bar{Al})\,\bar{Si}^{\frac{1}{2}} + 1\frac{1}{2}\dot{H}$
Sideroschisolite,		$\dot{Fe}^3\,\bar{Si}^{\frac{1}{2}} + 1\frac{1}{2}\dot{H}$

* The first of these formulas of Pyrosclerite corresponds to Silica 37·6, alumina 14·2, magnesia 33·2, water 14·9; the second, (which accords with the ratio obtained by Hermann), to Silica 32·6, alumina 19·8, magnesia 34·6, water 13·0. Other analyses, as those by Genth, and Smith and Brush, afford an intermediate ratio.

† Clinochlore has for $\dot{R}^3 : \bar{R}$ nearly the ratio 7 : 4, instead of 5 : 3.

These formulas belong to a single system or natural Group, and exhibit in a simple manner the relations of the species. Chloritoid and margarite are often associated together with corundum.

The second formula of Chloritoid corresponds to a recent analysis by Prof. von Kobell of a specimen from Bregratten, in the Tyrol, and to that by Dr. J. L. Smith of Asia Minor specimens.

The water is not regarded as a base in the above, excepting in the case of margarite. This independent relation of water is illustrated in other crystallized hydrous silicates. Thus *Pectolite*, has the crystalline form of hornblende; and *excluding the water*, its formula. So also *Laumonite* has in a similar manner the form and formula of Pyroxene, and in fact it is a hydrous Spodumene. Dioptase and Pyrosmalite, likewise, have the form and formula of Beryl and Eudialyte, as shown on page 211, vol. xvii, of this Journal; *Tritomite* the form and formula of garnet; while *Analcime* is a hydrous Leucite, and *Ittnerite* essentially a hydrous Sodalite. These are some of the examples among minerals, which show through homœomorphism, that the water in hydrous silicates is sometimes not a base.

In other cases, the water must be included among the bases: and this may be the fact with Apophyllite. The formula $(\dot{Ca}, \dot{K})\bar{Si} + 2\dot{H}$ (Rose) gives 3 per cent. too little of silica. But taking the exact ratio afforded by the analyses, as deduced by Berzelius, and making part of the water basic, we arrive at the formula $\dot{R}^3 \bar{Si}^2 + 2\dot{H}$ = Silica 52·7, lime 2·60, potash 4·4, water 16·7, in which $\dot{R}^3$ corresponds to $\dot{Ca}, \dot{K}, \dot{H}$ in the proportions 8 : 1 : 6. Datholite is a less doubtful example, giving nearly the form and formula of Sphene.

It will be observed, that in most of the species following Pyrosclerite A, if the oxygen of the water be added to that of the bases (see table, p. 128), its ratio to that of the Silica is then 2 : 1, on which ground, if the water be basic, the general formula of these species would be $(\dot{R}^3, \bar{R}, \dot{H}^3)\bar{Si}^{\frac{1}{2}}$.

Wöhlerite.—The formula of Wöhlerite deduced by Scheerer is $\bar{Zr}^3 \bar{Ni} + 5(\dot{Na}\bar{Si} + \dot{Ca}^3\bar{Si})$. It is certainly improbable that the same compound should contain three members so unlike in ratio, one with the oxygen of base and acid as 3 : 1, a second with this ratio 1 : 3, and a third, of 1 : 1. The analysis gives more exactly $4\dot{Na}$ and $14\dot{Ca}$; and thence the oxygen of the silica is to that of the other ingredients as 1 : 1, giving the general formula $(\dot{R}^3, \bar{Zr}, \bar{Ni})\bar{Si}$, in which $\bar{Ni} : \bar{Zr} : \dot{R}^3 = 1 : 3 : 6$ and affording the special formula $6\dot{R}^3\bar{Si} + 3\bar{Zr}\bar{Si} + \bar{Ni}\bar{Si}$, or $(\frac{6}{10}\dot{R}^3 + \frac{3}{10}\bar{Zr} + \frac{1}{10}\bar{Ni})\bar{Si}$ = Silica 31·2, niobic acid 14·3, zirconia 18·9, lime 27·0, soda 8·6.

Keilhauite.—The formula of this mineral, according to Erdmann, is $3\dot{Ca}^3\bar{Si}^2 + \bar{R}\bar{Si} + \dot{Y}\ddot{Ti}^3$, which contains three members with the widely unlike ratios 1 : 2, 1 : 1, 1 : 6. Erdmann's analyses afford the oxygen ratio for the bases and silica, (reckoning the $\ddot{Ti}$ with the former) of 3 : 2. Sphene has the same ratio, and I have shown that this mineral is a silicate of the form $\bar{R}\bar{Si}^{\frac{2}{3}}$, in which $\bar{R}$ or $(R^2O^3) = TiO^2 + CaO$, lime replacing part of the peroxyd. In Keilhauite the lime ($\dot{Ca}$) is nearly sufficient to make a peroxyd if combined with the $\ddot{Ti}$. Consequently Keilhauite appears to be a silicate analogous to Sphene, with the general formula $(\bar{R})\bar{Si}^{\frac{2}{3}}$ or $(\dot{R}^3, \bar{R})\bar{Si}^{\frac{2}{3}}$.

To Table I. of this volume, page 42, add :—

Æschynite, $I:I=90° 34'$, $1\bar{\imath}:1\bar{\imath}=111° 56'$, $1\breve{\imath}:1\breve{\imath}=112° 28'$. The plane I is $i\breve{2}$ (∞ P$\breve{2}$) of Rose. $a:b:c=0{\cdot}67534:1:1{\cdot}01$.

Zinc Vitriol. $I:I=90° 42'$, $1\bar{\imath}:1\bar{\imath}=120° 20'$, $1\breve{\imath}:1\breve{\imath}=120° 3'$; $a:b:c=0{\cdot}5785:1:1{\cdot}0123$. Near Epsomite.

Libethenite. $I:I=92° 20'$, $1\bar{\imath}:1\bar{\imath}=107° 40'$, $1\breve{\imath}:1\breve{\imath}=109° 52'$, $a:b:c=0{\cdot}7311:1:1{\cdot}0416$.

To Table II, page 47, add :—

Descloizite, Damour (Vanadate of lead, vol. xvii, of this Jour., p. 434)—$I:I=100° 38'$; $1\bar{\imath}:1\bar{\imath}=67° 36'$, $1\breve{\imath}:1\breve{\imath}=77° 47'$; $a:b:c=1{\cdot}494:1:1{\cdot}2052$. The striated planes $e\frac{3}{2}$ are here regarded as parallel to the vertical axis, which makes M a brachydome; and it becomes the brachydome $\frac{1}{2}\breve{\imath}$, having the summit angle 116° 25′. Taking this as $1\breve{\imath}$ instead of $\frac{1}{2}\breve{\imath}$, the vertical axis has half the above length or 0·747, the other axes being the same.

Glaserite (Anhydrous Sulphate of Potash). $I:I=104° 52'$, $1\bar{\imath}:1\bar{\imath}=59° 31'$, $1\breve{\imath}:1\breve{\imath}=73° 14'$; $a:b:c=1{\cdot}749:1:1{\cdot}3$.

Thenardite. $I:I=103° 26'$, $1\bar{\imath}:1\bar{\imath}=61° 12'$, $1\breve{\imath}:1\breve{\imath}=73° 42'$; $a:b:c=1{\cdot}691:1:1{\cdot}267$.

Struvite. $I:I=101° 42'$, $1\bar{\imath}:1\bar{\imath}=85° 4'$, $1\breve{\imath}:1\breve{\imath}=96° 50'$; $a:b:c=1{\cdot}0900:1:1{\cdot}2283$. The cleavage plane is here made the base, and the prism of 96° 50′ the unit brachydome. The crystals are hemihedral (or hemimorphic) in the direction of the brachydiagonal. The species belongs near Hopeite, of Section III, Table II. If the domes $1\bar{\imath}$ and $1\breve{\imath}$ be taken as the domes $\frac{3}{2}\bar{\imath}$ and $\frac{3}{2}\breve{\imath}$, (see page 48 of this volume) then $1\bar{\imath}:1\bar{\imath}=62° 54'$, *and the dimensions are very near those of Heavy Spar;* $a:b:c=1{\cdot}6350:1:1{\cdot}2283$, a here being one half greater than above.

To Table in last volume, on pp. 432, 433, add :—

Faujasite, Dimetric, Section IV; $O:1=118° 16'$, $O:1\bar{\imath}=127° 15'$.

Edingtonite, Dimetric, Section II; $O:1=136° 20\frac{1}{2}'$.

Gismondine, Dimetric, Section VI; $O:1=138° 45'$, equals $O:\frac{1}{4}$ in Scheelite.

Alunite, Rhombohedral, Section IV; $O:R=124° 40'$, $R:R=89° 10'$. One of the occurring rhombohedrons ($\frac{6}{5}$R) gives the angles of the rhombohedron of Brucite; $O:\frac{6}{5}=119° 57'$, $\frac{6}{5}:\frac{6}{5}=82° 26'$.

Levyne may be added to the Calcite Group; $O:1=136° 1'$. The other species of Section II are evidently closely related to the same Group.

On p. 217, last volume, "$C=106° 8'$" and "$C=88° 46'$" should be transposed.

In the *Dimetric System* (p. 433, vol. xvii,) the two groups of species pointed out as belonging to a distinct series of angles—one the *Rutile* Group, the other the *Idocrase* Group,—differ 3 to 5 degrees in the inclination of the base on the corresponding pyramidal planes, or twice this amount in the basal angle of the pyramids. In the former the basal angle of one pyramid is mostly 4 to 7 degrees above 90°; and in the latter 1 to 6 degrees less than 90°.

III. Botany and Zoology.

1. *The Micrographic Dictionary; a Guide to the Examination and Investigation of the Structure and Nature of Microscopic Objects;* by J. W. Griffith, M.D., F.L.S., and Arthur Henfrey, F.R.S., F.L.S., &c. Part 1. May, 1854. London: Van Voorst. pp. Introd. 24, Dictionary 16, plates 4. 8vo.—The first fasciculus of a work which is likely to supply an acknowledged want, and to be extensively useful. It is intended to be completed in 12 monthly parts (2*s.* 6*d.* each), illustrated by 40 plates, and about 800 wood-engravings. The subjects to which it is devoted are: 1. Instruction in the selection and use of Microscopes and microscopic objects. 2. The characters and structure, &c., of microscopic plants and animals, and minute organs, intimate struc-

ture, &c., in both kingdoms, and also inorganic matters occurring in animal and vegetable fluids, &c. The Dictionary extends in this fasciculus only from *Acalephæ* to *Aerial Roots*. The Introduction, on the selection, use, &c., of a microscope and its accessories, which appears to be exceedingly well executed, will be completed in part 2. The plate of test objects is admirably drawn and engraved: the other plates are tolerably good. No doubt the work will be in the hands of our microscopists generally. A. G.

2. *Botany of the Voyage of the Herald;* by B. SEEMANN.—The 4th part, (London, Reeve, 1854,) continues the Flora of Panama from the *Lythrarieæ* to the *Compositæ*, and about half way through the latter order, which is remarkably well elaborated by Dr. Steetz of Hamburgh. To the *Passifloraceæ* Mr. Seemann has annexed the *Turneraceæ*, on the strength of characters furnished by a new Veraguan genus, *Erblichia*, Seemann. The reasons of the union are more fully explained in a short article published some time ago in Hooker's Journal of Botany. A. G.

3. *Dr. Hooker's Flora of New Zealand.* Part 5, (London, 1854, Reeve,) commences the account of the Cryptogamic Plants, which are so numerous in New Zealand. Dr. Hooker has himself elaborated the Ferns, with a bold and able hand; and his prefatory observations upon this beautiful order,—so perplexed of late by special Fern-systematists, who, some of them, propose new genera for every modification of any one organ, whether of vegetation or reproduction,—deserve an attentive consideration. Under *Hymenophyllum Tunbridgense*, "a scarce English Fern," there is perhaps some mistake in the statement that it is "a great favorite with cultivators;" for we had supposed that no person had yet succeeded in cultivating it. Mr. Wilson has elaborated the *Musci*, in the midst of which the present fasciculus closes: seven plates are given to their illustration, and more are apparently to come. We understand that this great work will be immediately followed by the *Flora of Tasmania;* and that the first part of the Flora of India is also ready for the press. A. G.

4. *Botany of the U. S. Exploring Expedition under Capt. Wilkes: Phanerogamia*; by A. GRAY. Vol. I. pp. 777, roy 4to.—This volume contains the *Exogenæ Polypetalæ*. Many new species, and the following new genera are established in it, viz:

Richella—in Anonaceæ, near Polyalthia of Blume, but with a singular winged seed.

Agatea and *Isodendrion*, in Violaceæ; the latter of three species.

Diclidocarpus, a remarkable Tiliaceous genus.

Draytonia, allied to Saurauja, but with the styles united into one.

Rhytidandra, an anomalous Olacineous genus.

Pelea, a Rutaceous (Zanthoxylaceous) genus, of seven species.

Amaroria; a near ally of Soulamea, the *Rex amaroris* of Rumphius.

Brackenridgea; a genus of Ochnaceæ.

Oncocarpus; a genus of Anacardiaceæ, with a remarkable fruit.

Streptodesmia; a near ally of Adesmia.

Luma; a new Myrtaceous genus, of numerous Chilian species.

Acicalyptus; a genus related to Calyptranthes and Eucalyptus, to which the more closely can only be known by the fruit, which, however, is probably baccate.

Astronidium and *Pleiochiton*, two genera of Melastomaceæ.

Haplopetalon; a new genus of Legnotidæ, to which group *Crossostylis* is also referred.

Spiræanthemum; of Saxifragaceæ, Cunonieæ, containing two species.

Reynoldsia (of two species), *Tetraplasandra*, and *Plerandra*, new Araliaceous genera; the two latter with numerous or indefinite stamens; a new feature in this family.

The folio Atlas, of 100 plates, is not yet issued. A. G.

5. *Dr. Wallich*, the distinguished East Indian Botanist (a Dane by birth) died in London, at the close of April last. A little earlier the venerable *Professor Reinwardt* died in Holland. A. G.

6. *On preserving the Balance between the Animal and Vegetable Organisms in Sea Water*; by ROBERT WARINGTON,* (Ann. Mag. Nat. Hist., 2nd ser., vol. xii, p. 319.)—In the published notices of my experiments of 1849, to maintain the balance between the animal and vegetable organisms in a confined and limited portion of water, the fact was demonstrated, that, in consequence of the natural decay of the vegetation, its subsequent decomposition and the mucus-growth to which it gave rise, this balance could be sustained only for a very short period, but if another member were introduced, which would feed upon the decaying vegetation and thus prevent the accumulation of these destructive products—a function most admirably performed by the various species of water-snail—such balance was capable of being continuously maintained without the slightest difficulty; and I may add, that the experimental proof of this has now been carried on, in a small tank in the heart of London, for the last four years and a half, without any change or disturbance of the water; the loss which takes place by evaporation being made up by rain or distilled water, so as to avoid any great increase of the mineral ingredients originally present. It follows then, as a natural deduction, from the successful demonstration of these premises, that the same balance should be capable of being established, under analogous circumstance, in sea water. And in a paper published in January, 1852,† I stated that I was, at that time, "attempting the same kind of arrangement with a confined portion of sea water, employing some of the green sea-weeds for the vegetable member of the circle, and the common periwinkle as the representative of the water-snail."

The sea water with which the experiments I am about to detail were conducted, was obtained through the medium of one of the oyster-boats at the Billingsgate fish-market, and was taken from the middle of the English Channel.

My first object was to ascertain the kind of sea-weed best fitted, under ordinary circumstances, for keeping the water clear and sweet, and in a sufficiently oxygenated state to sustain animal life. And here opinions were at variance, for one naturalist friend whom I consulted, advised me to employ the Rhodosperms; another stated that it was impossible to make the red weeds answer the purpose, as he had tried them, and strongly recommended the olive or brown-colored Algæ;

* Communicated by the Author, having been read at the Hull Meeting of the British Association.

† Gardeners' Botanical Magazine and Garden Companion, Jan., 1852.

while, again, others thought that I should be more successful with those which had in theory first suggested themselves to my own mind, namely the Chlorosperms. After making numerous unsuccessful experiments with both the brown and red varieties of Algæ, I was fully convinced that, under ordinary circumstances, the green weeds were the best adapted for the purpose.

This point having been practically ascertained, and some good pieces of the *Enteromorpha* and *Ulva latissima* in a healthy state, attached to nodules of flint or chalk, having been procured from the shore near Broadstairs, several living animal subjects were introduced, together with the periwinkle. Every thing progressed satisfactorily, and these all continued in a healthy and lively condition.

My first trials were conducted in one of the small tanks which had been used for fresh water; but as it was necessary, during the unsuccessful experiments with the brown and red sea-weeds, to agitate and aërate the water, which had been rendered foul from the quantity of mucus or gelatinous matter generated during the decay of their fronds, until the whole had become oxydized, and the water rendered clear and fitted for another experiment, it was, therefore, for greater convenience, removed into a shallow earthen pan and covered with a large glass shade to protect the surface of the water, as much as possible, from the dust and soot of the London atmosphere, and at the same time impede the evaporation. In this vessel then I had succeeded perfectly in keeping a large number of beautiful living specimens in a healthy condition up to the close of 1852. I therefore gave instructions for the making of a small tank as a more permanent reservoir, and one more adapted for carrying on my observations and investigations on the economy and habits of the inhabitants.

From the experience I had obtained in my experiments with the freshwater tank, I was induced to modify slightly the construction of this vessel; thus, at the back, or part towards the light; the framing was filled with slate in the same way as the ends and bottom; for I had found that the glass, originally employed, very soon became covered with a confervoid growth which had an unpleasing appearance to the eye, and in consequence of which I had been obliged to paint the glass on the exterior, to prevent this growth from increasing to too great an extent. It was also an unnatural mode of illumination, as all the light should pass through the surface of the water. The front towards the room and the observer was constructed of plate-glass, the whole being set in a stout framework of zinc, and cemented with what is known under the name of Scott's cement, and which I have found to answer the purpose most admirably. Within this tank were arranged several large pieces of rock-work, thrown into an arched form, and other fragments were cemented in places against the slate at the back and ends, and at parts along the water line, so that the creatures could hide themselves at pleasure; a short beach of pebbles was also constructed in order that shallow water could be resorted to if desired. The whole tank was covered with a light glass shade to keep out the dust and retard evaporation.

With the sea water obtained in January, 1852, I have been working without cessation up to the present time, agitating and aërating when it

became foul during the unsuccessful experiments on the sea-weeds, but since then it has been rarely ever disturbed; the loss which takes place from evaporation being made up, as before stated, with rain or distilled water.

For a considerable period, after commencing these experiments, I was much troubled to obtain living subjects in a healthy condition, but having alluded to this, and the success of my investigations, in a short notice appended to a paper published in the "Annals of Natural History" for October, 1852, my friend, Mr. P. H. Gosse, who was then sojourning at Ilfracombe for his health, offered in the kindest manner to supply me with materials, and from that period he has always most heartily responded to my wants. It must not be imagined for a moment that the beautiful creatures I have thus received have been all preserved alive or always quite healthy. In experimental investigations this would be unreasonable to expect, as the very fact of experimenting implies a disturbance of the then state of things. Besides which, from want of a sufficient knowledge of natural history, from want of forethought and experience and other causes, I have lost many very fine specimens; and as the detail of these losses may prevent the occurrence of the like annoyances to others, I shall venture to occupy your time for a short period with their history.

My greatest loss arose from too great anxiety to transfer the collection I had preseved in a healthy condition to the end of December, 1852, into the new tank. As soon as it arrived from the maker's I lost no time in introducing my numerous family to their new abode, and dearly I paid for my precipitancy, for on the next morning I found many of my most beautiful specimens dead; thus I lost two fine Holothurias *(H. Pentactes)*, a small freckled Goby *(Gobius minutus)*, a beautiful little Pipe-fish *(Syngnathus lumbriciformis)*, and several others, and on opening the door of the case the cause of this mortality was at once evident,—an iridescent film of oily matter was floating on the surface of the water, arising from the paint with which the angular joints and edges of the small tank had been colored not having become sufficiently hardened.

Another source of loss arises from the several creatures attacking and devouring each other, and it therefore becomes a point of great importance—and highly necessary to be carefully observed, where their preservation is an object—to ascertain what varieties may be safely associated in the same tank; as, for instance, I have found that the Shrimps, and Prawns attack, and very soon devour, all the larger varieties of Corallines and Polyps, Sabellæ, Serpulæ, Rock-borers, Cirrhipeds, some of the Annelids, many of Bivalve and Univalve Mollusks that are unprotected by an operculum, or have no power of closing their valves. The instances which have come under my own immediate observation have been the destruction of the *Pholas dactylus*, *Saxicava rugosa*, *Cypræa Europæa*, and several specimens of Sabellæ, Serpulæ, *Coryne sessilis* and many others.

The common Crab *(Cancer Mænas)* is likewise a most destructive agent; and the tribe of rock-fish, the Blennies, Gobies, &c. are also most voracious, devouring all the varieties of Cirrhipeds, Corallines, Polyps, Annelids, &c.; they will also attack the shrimps and prawns,

and even seize upon the horns of the periwinkle, which they bite. If the mollusks do not keep a very firm hold of the rock or tank sides, they are rapidly turned over by these fish on their backs and lie helplessly exposed to their attacks.* It is doubtless their seeking food of this kind which causes these little fish to be so generally found in the shallow rock-pools of the coast. In consequence of these ravenous propensities I have been obliged to establish several small tanks and imitation rock-pools, so as to separate these various depredators from each other: thus in one I have varieties of *Actinia*, Shrimps, Nudibranchs, Holothurias, and some Annelids; in a second the rock-fish, as the Blennies, Gobies, Cottus, with Crabs and *Actiniæ;* in a third Corallines, Annelids, Polyps, Rock-borers, Sabellæ, Serpulæ, Holothurias, and *Actiniæ.*

Another curious instance of loss I may detail which has quite recently occurred, and which may prove interesting; it was in a small rock-pool containing Blennies, Gobies, Crabs, &c. I had procured two live oysters for the purpose of feeding my numerous small fry in these Vivaria, and one of these having proved ample for the purpose of one meal, the other was placed on the sandy bottom; on the second day after this, the oyster was observed to have opened the valves of his shell to a great extent, which were afterwards seen closed, but a small *Gobius niger*, inhabiting the pool, could nowhere be seen. The day after this the oyster was opened for the general feeding, when lo! within the shell was found the unfortunate *Gobius*, quite dead. Whether this little gentleman had been attracted within the trap by curiosity or the ciliary motion of the oyster, it is impossible with certainty to say; but that he must have seized on some sensitive part of the oyster is more than probable, so as to have caused such a rapid closing of the valves of the shell as could entrap so active a burglar.

Another important point is the gravity of the sea water; this should be very carefully regulated, for it must be borne in mind that many of the marine creatures are supplied by a permeation of water through their tissues or over their delicate and beautiful organs. The specific gravity should not rise above 1026 at 60° Fahr., and a small hydrometer should be introduced at short periods to ascertain that this point is not exceeded, particularly during the hot months of summer. The reduction to this gravity can be readily effected by the addition of rain or distilled water. Many of the creatures will of themselves afford indications of this increase of density; some of the *Actiniæ* will remain closed and become coated with a white slimy covering within which they remain for a length of time, and if the specific gravity of the water be lowered this is very soon ruptured by their expansion, thrown off, and the tentacula become soon extended.

* Since the reading of this paper at Hull I have received a Blenny of larger size, being about 8½ inches in length, and although it has become so tame that it will allow itself to be touched by the hand and takes its food from the fingers, yet its destructive propensities are so great, that it very soon killed four small Crabs; and to save three others, of rather a larger size, I have been obliged to remove the Blenny to a rock-pool in association with his own species and a few *Actiniæ.* The only refuge the poor Crabs had was to bury themselves in the sand, and whenever they attempted to move out of their refuge they were immediately pounced upon and only escaped by burrowing rapidly again.

All putrescent matter or excess of food or rejecta of the *Actiniæ* should be carefully removed from the water, as the noxious gaseous compounds generated by the decay of such matters appear to diffuse themselves rapidly through the water, act as a virulent poison, and speedily destroy the vitality of the occupants. Thus many beautiful subjects were lost in a few hours from the introduction, into a small glass jar, of a large Pecten shell, encrusted with corallines, which had become loaded with putrescent matter by partial submersion in a foul muddy bottom.

Great care should also be taken in moving the *Actiniæ*, that the foot or sucking disc with which it attaches itself to the rocks, stones, or mud, be not injured, as, when this occurs, they rarely survive, but roll about without attaching themselves, and gradually waste away and die.

With these exceptions then, every thing has gone on very satisfactorily, care being always taken not to overload the water with too large a proportion of animal life for the vegetation to balance, as whenever this has been inadvertently attempted, the water has soon become foul, and the whole contents of the tank, both animal and vegetable, have rapidly suffered, and it has required some time before the water could be restored to its former healthy condition.

In one of the numbers of the "Zoologist" of last year, I stated that besides the *Ulvæ*, *Enteromorphæ* and *Cladophoræ*, I had found the *Zostera marina* a very useful plant for oxygenating the sea water; but this observation has reference only to the case of a tank supplied with a ground where its roots will find a sufficiency of food for its growth, as in a clear shingle or sand it soon decays; and it should be associated with such animals as delight in a ground of this nature, as many of the Annelids, Crabs, burrowing Shrimps, &c. There are several interesting observations which have been made from time to time connected with this subject, which I hope to lay before the natural-history world as soon as I can find leisure time for the purpose.

Apothecaries' Hall, Sept. 10, 1853.

IV. Astronomy.

1. *New Planets:—Bellona* (28), (Comptes Rend., xxxviii, 455, 561.)—On the first of March, 1854, Mr. Luther, Director of the Observatory at Bilk, discovered a new planet which has received from Mr. Encke the name *Bellona:* it is of the tenth magnitude. Its position, March $6^d\ 10^h\ 27^m\ 30^s$, M. T. Hamburg was R. A. $180° 35' 38''$ and Dec. $+7° 47' 34''$. Mean daily motion in R. A. $10' 7''$ decreasing, in Dec. $9' 26''$, increasing.

Amphitrite (29), (Compt. Rend., xxxviii, 428, 645.)—Mr. Albert Marth, at the Regent's Park Observatory in London, discovered another planet near *Spica Virginis*, on the morning of the second of March. It appears as a star of the tenth magnitude. Mr. Bishop has proposed for it the name *Amphitrite*. The following elements of its orbit were calculated by M. Yvon Villarceau, according to the method given in the *Connaissance des Temps* for 1852, from 16 observations made at Paris during the month of March.

Epoch 1854, March 0·00, M. T. Paris.

Mean anomaly,	114° 36′ 54″·58	
Long. perihelion,	64 50 22 ·81	Mn. Eqnx.
" asc. node,	356 20 34 ·94	Mar. 0·0, 1854.
Inclination,	6 6 19 ·69	
Angle of excentricity,	4 34 47 ·04	
Mean daily motion,	864″·3666	
Semi-axis major,	2·5637300	
Period of revolution,	4yrs·104962	

This planet was discovered independently by M. Chacornac, assistant observer at the Observatory of Paris, on the third of March. He also on the fourth of February, at Marseilles, noted a star of the tenth magnitude which is now wanting in that place, and which is shown to have been the body first recognized as a planet by Mr. Marth.

2. *New Comet, I. of* 1854, (Comptes Rend., xxxviii, 648.)—The comet which was visible to the naked eye on the twenty-ninth of March last and the few following days, was seen on the same day in Paris. The following elements were computed by Mr. James Ferguson (Astron. Journ., No. 71,) from the Washington observations of April 3, 7 and 11.

Perihelion passage, 1854, March 24·0581, M. T. Berlin.

Long. perihelion,	214° 52′ 52″·0	Mn. Eqnx.
" asc. node,	316 19 58 ·2	Apr. 7·0, 1854.
Inclination,	83 30 33 ·4	
Log. perihelion dist.,	9·441070	
Motion,	Retrograde.	

This comet was seen in the east on the morning of the twenty-third of March by Mr. Alfred de Menciaux near Damazan in France.

3. *Annular Solar Eclipse of May* 26, 1854.—From a communication in the Boston Evening Traveler, May 29, 1854, we gather some of the following particulars respecting the solar eclipse of May 26, 1854. On account of cloudy weather, the eclipse was but partially observed throughout New England, and in many places wholly lost. At Cambridge, Mass., the formation of the ring was observed and it occurred within seven seconds of the time predicted by R. T. Paine, Esq. in his communication to the American Academy,—which is a remarkably close coincidence. The formation and rupture of the ring and the two other contacts were concealed by clouds. "At Middlebury, Vt. where the eclipse, by calculation was central, the beginning of the eclipse could not be seen, but the formation and breaking of the ring and the end of the eclipse were finely observed, and the spectacle was magnificent." At the Coast Survey station on the summit of Mount Agamenticus, nearly in the central line, the sun was invisible throughout the day.

At New York City, Ogdensburg, and Washington, the sky was very favorable, and good observations of the phenomena were secured, including numerous heliotypic pictures with the times of each.

Effect on the Deviation of the Magnetic Needle.—Mr. J. G. Ellery sends us an account of observations made by him at Morgantown, Burke Co., N. C., on the deviation of the magnetic needle. The instrument with which his observations were made was a miner's compass,

with a needle between two and three inches in length, possessing the delicacy of movement of the best surveyor's compass, but without levels or vernier. The weather was warm and sultry, with indications of rain from some of the large clouds in different parts of the heavens; but with hardly air enough stirring to move a leaf. No motion of the needle was observed between 1 P. M. and 4 P. M. Soon after it was observed to shift its position toward the west, and at $5\frac{1}{4}$ it had attained its greatest variation, pointing then more than half a degree to the west of north. The variation decreased from that time till a quarter past six, when the needle was again precisely on the meridian. In the circumstances of the observation it seems quite uncertain whether the movement of the needle was not due to other causes than the interception of solar influence.

Lieut. MAURY (Ast. Journ., No. 72) communicates the observations made at the Washington Observatory:—

Beginning by Mr. Ferguson, - -	4^h	2^m	$37^s{\cdot}57$
End, " - - -	6	27	26·46
" by Prof. Keith, - - -	6	27	29·64

Mr. Ferguson observed with the large equatorial, and Prof. Keith with the west transit instrument, lifted from its Ys and mounted on the reversing apparatus. The computed times for beginning and end were $4^h\ 2^m\ 41^s{\cdot}4$ and $6^h\ 27^m\ 29^s{\cdot}4$.

Prof. JAMES CURLEY (Astr. Journ., No. 72) furnishes the observations made at the Georgetown Observatory. The last contact was observed by Prof. Sestini with the equatorial, using a power of 25 or 30, in order to have the whole sun in the field of view. The time was determined with the meridian circle from the transit of the sun on the 26th, and after applying the necessary corrections was determined to be for the last contact $6^h\ 27^m\ 22^s{\cdot}67$, Georgetown Mean Time. Lat. 38° 54′ 26″ N., Long. $5^h\ 8^m\ 18^s{\cdot}2$ west of Greenwich.

Effect on the Magnetic Intensity; by LEWIS R. GIBBES.—The condition of the atmosphere throughout the day was unfavorable to nice observations, and we have but few positive results to record. Though the face of the sun during the eclipse was not observed, yet a thin hazy cloud overspreading its disc, and the western part of the sky, and the accompanying unsteadiness of the atmosphere in refractive power, kept the limbs of both sun and moon in a state of constant agitation. This tremulons condition of the limbs prevented the study of those remarkable phenomena sometimes seen at the contact of the limbs of the two bodies.

The beginning of the eclipse was observed at $3^h\ 58^m\ 31{\cdot}4^s$ mean time at Charleston Observatory, the end at $6^h\ 19^m\ 08{\cdot}8^s$. Place of observation, the College. Magnifying power used, 55. Screen glass, green and light red combined. No unusual phenomena observed; limbs tremulous at both contacts. No spots, either dark or bright, to be seen on the sarface of the sun. No prolongation of the moon's limb beyond the sun's disc was observed, nor any alteration of the form of the cusps; nor was the moon seen before the contact of limbs, through either the white or colored screens.

The temperature and transparency, or rather transcalescency of the atmosphere was so constantly varying, that no results of interest could be obtained with reference to the eclipse.

Within the last two or three years, M. Lion, Professor of Physics at Beaune, in France, has maintained that the intensity of the magnetic force of the earth is increased during a solar eclipse. To test this, observations were made during the late eclipse with the following apparatus: A magnetic bar, 4½ inches long and ¼ of an inch square, weighing 400 grains, was placed in a stirrup of thin sheet brass—the two together weighing 410 grains—and, by a hook of fine wire, was suspended by 5 fibres of untwisted silk, 10 inches in length; the whole was protected by a glass receiver from agitation by currents of air, or other causes of disturbance. This apparatus was mounted on a firm stand, and placed on the stone floor of a room, in the basement story of the College, used for storing the coal for the laboratory, empty packing boxes, &c., though by no means to be regarded as a coal hole; yet may not experiments be carried on in a coal hole, if no better place can be had? A few feet north of it was placed a table for the record book, chronometer, and a small telescope, whose axis coincided with that of the magnetic bar when at rest. The time required by the magnet to accomplish 100 oscillations or double vibrations were observed at intervals during the day; the end of one oscillation, and the beginning of the next, being taken at the instant that the axis of the magnet coincided with that of the telescope, the north end of the magnet swinging from west to east. The results are contained in the following table. The approximate interval for 100 oscillations was 17¼ minutes. The first column of the table gives the time of the phases of the eclipse and of the middle of each interval occupied by 100 oscillations; the second column, the precise length of interval of each 100 oscillations.

Date. h. m.	Interval of 100 oscillations. min. sec.
26th—10 08, A. M.	17 15·93
12 25, P. M.	General eclipse begins.
12 51, "	17 16·20
1 08, "	17 15·58
1 25, "	17 14·80
3 16, "	17 15·64
3 35, "	General eclipse middle.
3 59, "	Eclipse begins at Charleston.
4 31, "	17 15·98
4 48, "	17 16·10
5 09, "	Eclipse middle at Charleston.
6 01, "	17 15·56
6 18, "	17 15·94
6 19, "	Eclipse ends at Charleston.
6 20, "	General eclipse ends.
29th—12 30, "	17 15·12
12 47, "	17 15·15

The observations were begun on the 26th, two hours before the beginning of the general eclipse on the earth, and continued at intervals until it ended, and two more were taken on the 29th. The results give no indication of any sudden or unusual change in the magnetic intensity during the eclipse; the intervals are not all precisely equal, but their deviations from coincidence are not greater than what are due to una-

voidable errors of observation, to imperfection in the apparatus used, and possibly to ordinary causes of disturbance. The second, third, and fourth results, which appear to be regularly diminishing, were obtained in close succession, the instant of ending the first 100 oscillations being the beginning of the second 100, and the end of the second 100 being the beginning of the third 100, as will be seen by the differences of the times, being 17 minutes. Their diminution is due to the gradually diminishing arc of vibration, the initial arc being somewhat large —8 or 10 degrees—in order to allow of 300 continuous vibrations. A more delicate suspension would have been desirable. The initial arc in the other cases was about 5 degrees, and the observations continued to 200 oscillations, except in the first and fifth results. We may from these results draw the conclusion that there were no disturbances of the magnetic intensity due to the occurrence of the eclipse, although the strictly logical deduction is, that if such disturbances existed, they were too minute to be detected by the apparatus used.

M. Lion has also maintained that this disturbing influence of a solar eclipse may be perceived at places where the eclipse is not visible. This point we examined with the same apparatus above described, at the occurrence of the solar eclipse of 6th June, 1853, which was not visible at Charleston, the northern edge of the penumbra passing just south of this city. The results are contained in the following table with the same arrangement as the preceding.

Date.	June, 1853. h. m.	Interval of 100 oscillations. min. sec.
5th—	8 09, A. M.	17 17·09
	7 00, P. M.	17 16 88
6th—	8 08, A. M.	17 18·45
	8 25, "	17 18·00
	11 47, "	General eclipse begins.
	12 05, P. M.	17 16·88
	12 22, "	17 17·23
	2 38, "	General eclipse, middle.
	3 00, "	17 18·02
	4 00, "	17 18·18
	5 47, "	General eclipse ends.
	6 35, "	17 18·83
	10 00, "	17 18 66
8th—	8 00, A. M.	17 17·82
	10 00, "	17 17·36

These results show no disturbance proceeding from the occurrence of the Solar Eclipse, and we cannot avoid the conclusion that the Professor at Beaune has in some way mystified himself in his observations, especially as his instrument was, if we mistake not, an ordinary magnetic compass, and therefore far less fit for such observations than the above described apparatus.

Each result in the above tables is the mean of *five* sets taken in the mode usually adopted in such observations, except the first in the first table, which is a mean of three. More, therefore, than 5000 oscillations, or more than 10,000 vibrations have contributed the results in the first table, and more than 12,000 vibrations the results of the second table.

College of Charleston, 1st June, 1854.

4. *Eclipse of the Sun, May* 26, 1854, *at Yale College*, (Lat. 41° 18′ 23″, Lon. 4h. 51m. 47s.)—The previous day was rainy, but it cleared off in the night, and the morning afforded so pure and clear a sky as to inspire high hopes of a most favorable time for observing the eclipse. But by noon a strong northwesterly wind began to bring up ridges of cumulus clouds which, as the time of the eclipse drew near, left only here and there a patch of clear sky and prevented our obtaining the first contact, although Mr. Francis Bradley who observed separately with his reflector, had a momentary glimpse of the sun at 4h. 19m. 47s. M. T. when the eclipse had advanced a few seconds. Messrs. Lyman and Newton used the Clark Telescope with a power of 110. No spots were visible, but the mottled appearance of the sun was very distinct, and the definition good. When the sun was first seen emerging from a cloud, the eclipse had advanced perhaps 15 or 20 seconds. Clouds and clear sky alternately obscured and revealed the sun until five o'clock, after which the view was unobstructed.

The *end* was closely observed by Mr. Bradley with a power of 80 and by Mr. Lyman with 110, but the limb, being now near the horizon, was so broken and wavy that the instant of last contact could not be determined within several seconds. The time fixed upon was 6h. 42m. 19s. This is probably within from 5 to 8 seconds of the truth.

Mr. Lyman remarks, that with the Clark Telescope (aperture 5 inches) the moon's limb during the early part of the eclipse, was well defined, the lunar mountains being projected with great distinctness on the sun's disk. Two or three times near the middle of the eclipse, the extreme points of the cusps were observed to be momentarily cut off by projecting mountains. No other important physical phenomena were noticed by him, though the cusps and limbs of the sun and moon were carefully watched at frequent intervals. Mr. Newton could detect no signs of polarization with a double image prism.

Professor Olmsted occupied himself with the meteorological instruments, and the general aspects of the earth and sky. He was favorably situated on the top of a high tower where an unobstructed view was enjoyed on all sides. As the eclipse advanced to the maximum (nearly 11 digits) the shadows became less deep, the color of the grass and trees, now at the height of their summer verdure, wore an increasing olive hue, and a sublime and strange appearance invested all things. Having, however, witnessed the total eclipse of June, 1806, and retained its appearances very fresh in memory, he remarked that there was an immense difference between a total eclipse of the sun, and one only *almost* total, the former being vastly more sublime and impressive.

The meteorological instruments, including the barometer, two thermometers, (one in the sun the other in the shade,) and a delicate azimuth compass, were noted at short intervals, but none of the observations suggested any thing important, except that the thermometer exposed to the direct action of the sun rapidly fell as the eclipse advanced, and at the maximum, differed only two degrees from that in the shade, the former being 69½° and the latter 67½°. At 3h. 16m. the solar thermometer had stood at 92¼°, and just after the beginning of the eclipse when the sun came out of a cloud, it stood at 82°. The sensations indicated a perceptible decline of temperature, but nothing of that chill was experienced which characterized the total eclipse of 1806. O.

V. Miscellaneous Intelligence.

1. *American Association for the Advancement of Science.*—This Association held its annual meeting at Washington, in the rooms of the Smithsonian Institution, during the week commencing with Wednesday the 26th of April, 1854. James D. Dana was the President of the meeting. Prof. J. Lovering, of Cambridge, Permanent Secretary, Prof. J. Lawrence Smith, General Secretary. Prof. John Torrey was elected President for the next meeting, which was appointed to be held in Providence, R. I., on the 3d Wednesday of August, 1855.

The following is a list of the papers read at the recent session:

(1.) *Physics, Astronomy, Geodesy, &c.*

Comparison of the diurnal inequalities of the tides at San Diego, San Francisco, and Astoria, on the Pacific coast of the United States, from observations in connection with the Coast Survey. By *A. D. Bache*, Superintendent.

On the resistance experienced by bodies falling through the Atmosphere. By Prof. *Elias Loomis*, University of New York.

Electric properties of whalebone rubber. By Mr. *John M. Batchelder*, Superintendent of the Crystal Palace (communicated by Prof. Peirce).

Preliminary determination of cotidal lines on the Atlantic coast of the United States, from the Coast Survey tidal observations. By *A. D. Bache*, Superintendent.

Earthquakes of Chili. By Lieut. *Gilliss*.

On the physical constitution of the sun and cometary bodies. By Prof. *W. A. Norton*.

On the periodic and occasional perturbations of the directive force of the declination of the magnetic needle. By Prof. *W. A. Norton*.

On the magnetic forces observed along the line of the boundary between the United States and Mexico. By Maj. *W. H. Emory*.

Results of some investigations respecting the double comet of Biela. By Prof. *J. S. Hubbard*, of the National Observatory.

Illustration of cycloidal curvature as involved in the supposititious travelling whirlwinds, and some new demonstrations of the impossibility of storms being whirlwinds, unless on a limited scale, as the consequence of in-blowing winds. By Prof. *Robert Hare*.

Suggestion relative to the observation of the annular eclipse of the sun of May 26, 1854. By *Stephen Alexander*.

On the distribution of temperature in and near the Gulf Stream, off the coast of the United States, from observations made in the Coast Survey. By *A. D. Bache*, Superintendent.

On the Gulf Stream. By Lieut. *M. F. Maury*.

On the Basin of the Atlantic. By Lieut. *M. F. Maury*.

Astronomical determination of the sun's diurnal and annual intensity. By *L. W. Meech*.

On the transparency of the ocean. By Commander *Glynn*, U. S. N.

On the nature of Forces. By Lieut. *E. B. Hunt*, Corps of Engineers, U. S. A.

Description of the U. S. Coast Survey Apparatus for measuring base lines. By Lieut. *E. B. Hunt*, U. S. A., and Assist. U. S. C. S.

Note on a new electro-chronometric method. By Dr. *Wolcott Gibbs*.

On a new instrument for facilitating the projection of great circle routes in charts, and finding by inspection the course and distance. By Prof. *W. Chauvenet*.

Comparison of the British Association Catalogue of Stars with the Greenwich Twelve Year Catalogue. By Prof. *Elias Loomis*, University of New York.

Remarkable lunar phenomenon observed at Auburn, N. Y., Feb. 16, 1843; with diagram. By *Blanchard Fosgate*, M.D.

On the arrangement of Lecture rooms, with reference to sound and sight. By Prof. *Joseph Henry*, Secretary Smithsonian Institution, Washington.

A constructive method of projecting solar eclipses. By *Chauncey Wright*, Cambridge, Mass. Presented by Lt. C. H. Davis, U. S. N.

On the superior facilities for the computation of the Lunar Ephemeris afforded by the new system of arguments introduced by Prof. Peirce into his "*Tables of the Moon.*" By *J. D. Runkle.*

On Irradiation. By Prof. *W. B. Rogers.*

On the Satellites of Uranus. By Prof. *Elias Loomis*, University of New York.

On the inverted microscope; with some remarks on the illumination of microscopical objects. By Prof. *J. Lawrence Smith*, of Louisville, Ky.

The astronomical expedition to Chili. By Lieut. *J. M. Gilliss*, U. S. N.

Abstract of a paper on the tidal currents of Long Island and approaches, from observations in connection with the United States Coast Survey. By *Charles A. Schott.*

On the longitude of Frontera, El Paso, and San Eleazario, resulting in the determination of a cardinal point of the Boundary Survey. By Maj. *W. H. Emory.*

On the relative value of the different astronomical methods of determining the longitude. By Lieut. *C. H. Davis*, U. S. N.

On the determination of the longitude of the Observatory at Cambridge from the chronometric expeditions of the Coast Survey. By *G. P. Bond.*

Difference of longitude by Moon and Star culminations. By Prof. *G. W. Coakley.*

The longitude of America; determined by moon culminations. By Prof. *B. Peirce*, of Harvard College.

Method of observing at sea for the determination of the latitude, longitude, and variations of the compass. By *O. C. Badger*, U. S. N.

Cloverden Observatory and the Shelby College (Kentucky) Equatorial. By *B. A. Gould*, Jr., and *Joseph Winlock.*

(2.) *Meteorology.*

The Brandon tornado, Ohio, January 20, 1854. By Prof. *O. N. Stoddard*, Miami University.

Cape Verde and Hatteras hurricane and other storms. By *W. C. Redfield*, of New York.

On the probable increase of hail storms in Cuba, especially from 1844 to 1854. By *Andres Poey*, of Havanna.

An account of a storm that passed over Connecticut, August 1, 1851. By Prof. *John Brocklesby.*

On the meteorological phenomena observed at various points on the Boundary Survey. By *Marine T. W. Chandler* (read by Maj. W. H. Emory).

On the barometer off Cape Horn. By Lieut. *M. F. Maury.*

A Theory of Storms. By *T. Basnett.*

On the permanence of the principal conditions of climate. By *Lorin Blodget*, of Washington.

The climate of Chili. By Lieut. *J. M. Gilliss.*

On the swelling of springs and the re-appearance of storms before rain. By Prof. *John Brocklesby.*

The Meteorograph: a self-registering instrument for meteorological observations. By Prof. *N. B. Webster*, of the Virginia Collegiate Institute, Portsmouth, Va.

On the law of variations of atmospheric pressure through successive months of the year; and its practical application to barometric measurements of heights in the interior of Continents. By *Lorin Blodget.*

(3.) *Geology and Mineralogy.*

On the cleavage and other effects caused by trap dykes in the middle secondary rocks of Virginia. By Prof. *W. B. Rogers.*

Notice of a peculiar variety of coal from Breckenridge county, Ky. By Prof. *B. Silliman*, Jr.

On a number of mineral species. By *T. S. Hunt*, of the geological survey of Canada.

Notice of some imprints which have recently been observed in the sandstone strata at the quarries in Portland, Ct., with some remarks on this formation on the Atlantic coast of the United States. By *John Johnston*, Prof. Nat. Science Wesleyan University, Middletown, Conn.

Brief outline or general description of a remarkable fossil, not known to be described, and by some supposed to be an Ichthyodorulite. By Prof. *Wm. Hopkins*, of Genesee College, Lima, N. Y.

On the absence of the evidence of remains of fishes in all the Silurian rocks of the United States. By *James Hall.*

Is anthracite the coke of bituminous coal? By Prof. *B. Silliman*, Jr.

On phosphatic organic remains in the Palæozoic rocks. By *T. S. Hunt*, of the Geological Survey of Canada.

On the Crystalline Limestone of North America. By *T. S. Hunt*, of the Geological Survey of Canada.

Remarks upon the geological formation of the country along the line of the boundary survey, based upon the examination of Dr. Parry, made under the order of Major Emory. By *James Hall.*

On the western limits of the Cretaceous formation on the northern continent of America, as evidenced by the various collections that have been made by exploring expeditions under the direction of the government of the United States. By *James Hall.*

Geology of the lead mines of Wisconsin. By *Edward Daniels*, Geologist to the State of Wisconsin.

On the age of the so-called new red sandstone of the United States. By Prof. *W. B. Rogers.*

Red sandstone of the Connecticut River Valley, and the proofs of its Oolitic or Liassic age. By *James Hall.*

A description of a post-diluvial deposit in Campbell Co., Ky., with a catalogue of the fossil remains. By *W. H. B. Thomas*, of Cincinnati, Ohio.

Sketch of the general geological structure of the region of country in connection with the United States and Mexican boundary line. By *C. C. Parry.*

On the chemical composition and metamorphoses of some sedimentary rocks. By *T. S. Hunt*, of the Geol. Survey of Canada.

Some comparative observations on the carboniferous strata of North America. By Prof. *H. D. Rogers.*

On some phenomena of cleavage structure, and metamorphism in coal and other strata. By Prof. *H. D. Rogers.*

On the geology of the Lower Rio Bravo. By *Arthur Schott* (read by Maj. W. H. Emory).

The Silurian and Devonian systems; and the nature of the evidence for drawing a line of separation between the two systems in the United States. By *James Hall.*

Observations upon the geology of the Mauvaises Terres, Nebraska, with notices of the geographical and geological range of some of the fossils of that region. By *James Hall.*

Remarks upon a collection of Cretaceous fossils from Nebraska, and the absence of species known in the southern extension of the same formation. By *James Hall.*

Remarks upon the results of extensive and continued collections of fossil species from a portion of the Silurian rocks of New York, showing the number of species and individuals of each species obtained from a limited locality during a period of ten years. By *James Hall.*

On the reproduction of similar types or representative species in successive geological formations. Illustrated by a collection of species of the Brachiopoda from the Niagara and Lower Helderberg groups of the Palæozoic rocks of the United States. By *James Hall.*

(4.) *Chemistry.*

On the use of hydrogen gas to displace sulphuretted hydrogen in the analysis of mineral waters. By Profs. *W. B. Rogers* and *R. E. Rogers.*

Illustrations of chemical homologies. By *T. S. Hunt*, of the geological survey of Canada.

Decomposition of water at the ordinary temperature, by an alloy of zinc and antimony; with a description of a new process for procuring hydrogen. By *Josiah P. Cooke*, Jr.

A new filtering apparatus. By *Josiah P. Cooke*, Jr.

On a new form of electrical machine. By *G. C. Schaeffer.*

Researches on arseniuretted and antimoniuretted hydrogen, and their relations to Toxicology. By Prof. *Raphael Napoli*, of Naples.

On the chemical relations of odors, and their employment as tests. By *Geo. C. Schaeffer.*

On meteoric stones; with an account of some recently discovered. By Prof. *J. Lawrence Smith,* of Louisville, Ky.

On two new general methods of chemical analysis. By Dr. *Wolcott Gibbs.*

On the volumetric determination of Nitric, Arsenic, Antimonic and Stannic acids, and on the separation of Manganese, Cobalt and Nickel. By Dr. *Wolcott Gibbs.*

The American Patent System, and its relations to science—especially to chemical science. By Dr. *L. D. Gale,* of the Patent Office, Washington.

(5.) *Zoology.*

Life in its physical aspects. By *Charles Girard.*

On the Delphinus, or Phocæna Orca, the Whale Killer or Thresher, mentioned by John Tradescant in the journal of his voyage to Russia in 1618. Communicated by Dr. *Hamel,* of the Imperial Academy of Sciences at St. Petersburgh.

Experimental observations on the sense of smell and taste. By Dr. *H. C. Hilgard.*

On the development of monocotyledonous stems. By *J. Darby,* of Georgia.

On the Whale. By Lieut. *Maury,* U. S. N.

Appendix.—On Personal Equation, &c. By *J. Winlock.*

Some reasons for suspecting the Mobilian language to have been spoken along the southeastern shores of North America in the first half of the 16th century. By *Buckingham Smith.*

2. *Abstract of a Meteorological Journal kept at Beloit College, Beloit, Wis., for the year* 1853, (communicated for this Journal.—Lat. 42° 30′ 23″ N.; Long. 12° 03′ 20″ W. from Washington; elevation above Lake Michigan, 172 feet—above the Ocean, 750 feet; by S. P. Lathrop, M.D., Professor of Chemistry and Natural History.

Months.	Barometer. Max.	Min.	Mean.	Thermometer. Max.	Min.	Mean	Cloudiness.	Prevailing winds.	Inches rain & melted snow.
January,	29·65	28·44	29·635	48	− 5	27·04	3·70	N., S. & S.W.	1·25
February,	29·61	28·66	29·193	50	− 9	23·00	4·55	N.W. & N.	2·85
March,	29·64	28·69	29·198	60	0	33·57	3·48	N.W., N. & S.	2·00
April,	29·56	28·58	29·147	70	27	40·12	4·82	N.E. & N.	5·68
May,	29·59	28·80	29·295	84	38	57·44	4·54	N.E., N.W. & S.	4·37
June,	29·53	28·92	29·377	91	54	73·16	3·04	S.W. & S.	4·85
July,	29·52	29·04	29·343	83	54	68·90	2·78	S.W & N.E.	8·10
August,	29·55	29·08	29·238	91	52	71·37	3·08	S.W. & N.W.	1·68
September,	29·53	28·78	29·234	91	41	64·02	3·97	S.E. & N.W.	6·48
October,	29·58	28·78	29·431	73	11	47·00	2·65	S., S.W. & N.	·50
November,	29·77	28·91	29·347	61	10	41·30	6·02	S.W. & N.	6·85
December,	29·59	28·53	29·156	53	− 5	26·39	3·83	S.W. & N.W.	1·10
Year.	29·77	28·44	29·308	91	− 9	47·75	3·87	S.W., N.W. & N.	45·71

The mean temperature of the past year is 47°·75, being nearly the mean of the three previous years, which is 47°·505.

The mean temperature for the winter months of 1852–53 is 24°·863, which is something over a degree less than the mean of the two previous years, and 2°·567 lower than for the year 1850–51. The mean temperature for the spring months is 43°·71, being nearly as low as the temperature of the same months in 1850, which was 43°·08, and 2°·713 lower than the mean of 1851 and 1852. The mean temperature of the summer months is 71°·143, being nearly the same as in the year 1850, and 1°·24 higher than the mean of the years 1850–52. The mean temperature of the autumnal months is 50°·773 being about the same as the year 1851, and 1°·38 above the mean of the years 1850–52.

The average density of the atmosphere as indicated by the barometer, is 29·308 inches, being ·031 inch below the year 1851, and nearly ·04 higher than either of the years 1850 and 1852.

The amount of rain and melted snow for the year is 45·91, which is 5·91 more than the previous year, and 7·66 less than the mean of the two years 1850–51. This was not so equally distributed through the several months of the year as usual, the month of July having 8·10 inches, while the month of October had only 0·50 inch. September and November have nearly the same amount.

The amount of snow which fell in the winter of 1852–53 is 30 inches, being the same amount as last year, and more than either of the two previous years, and it was quite equally distributed through the winter months.

The last year was never surpassed by more abundant crops. They were *uniformly* good. None of them were injured to any amount, by any unfavorable condition of weather, or superabundance of insects injurious to vegetation, or blight or mildew. Fruit trees of every kind and description were loaded to their utmost with delicious fruit. We fear that we shall seldom see the like of the past year in the quantity and quality of grains, and the richness and abundance of fruit.

The prevailing winds were Southwest and Northwest, instead of N. and N. W., as in the three previous years.

CALENDAR.—*February* 8th, Coldest day of the year, average of thermometer –3°; 28th, Dwarf Peony and Star of Bethlehem up.

March 7th, Auroral arch; 15th, Remarkable Parhelia at 3 P. M.; 17th, Tulips, first birds heard to sing, wild geese seen; 19th, Bluebirds, Blackbirds and Woodcock seen; 22d, Narcissus up, Robins seen; 24th, Larkspur up; 26th, Triton seen.

April 4th, Pulsatilla potens and Ranunculus fascicularis in flower, striped snake seen; 14th, Gooseberries in leaf; 16th, Missouri and black Currant in leaf, Cucullaria in flower; 17th, commenced gardening; 25th, Jonquil in blossom; 26th, Dwarf Iris in bloom, Turtles, Frogs and Toads seen.

May 2d, Missouri currant in flower; 4th, Plum in blossom; 5th, Cherry in blossom; 11th, Puccoon and Liverwort in flower; 12th, Baltimore Oriole and Turtle Dove seen; 13th, Uvalaria perfoliata in flower; 16th, Silphium trifoliatum, Greek Valerian, Blue Phlox, American Cowslip and Tulip in flower, burr and black Oak in leaf; 17th, Robinia hispida and Carya alba in leaf; 19th, Lilac and Cowslip in flower, corn planting; 21st, Trillium and Stramonium in flower; 22d, Blue eyed grass and Hypoxis erecta in flower; 25th, first really growing weather, cold hitherto; 28th, Mountain ash, Baptisia tinctoria, Creeping vetch and Potentilla in flower; 30th, Zizia and Aquilegia in flower.

June 2d, Vibernum opulus, Cinnamon rose, Arenaria laterifolia, and Pentalophus longiflorus in flower; 6th, Capsella bursapastoris, Cypripedium spectabile, Ranunculus aquatilis, Philadelphus coronarius and Hydrophyllum virginicum in flower; 9th, Philadelphus flava and Iris versicolor in flower; 18th, Campanulas, Lobelias and Rudbeckias in flower.

July 13th, Auroral arch and streamers.

August 11th, Hottest day, average of thermometer 85°·33; 23d, Comet first seen in the west at 8¾ P. M.

September 1st, Auroral arch.

October 25th, First snow.

3. *The Climate of San Francisco—Review of the Weather for the Year* 1853; by Dr. Henry Gibbons.—The first part of January was cloudy and rainy, but after the 11th, the weather was mostly clear and charming, only one rain occurring in the last two weeks. The lowest temperature was 41°, and the highest 62°. The mean at sunrise was 47½° and at noon 56½°. The prevailing winds were very light, from north and northwest. There were nine days entirely clear, and four days entirely cloudy. January 1852, was colder, having five mornings below 41°; January 1851, was much colder, having 13 mornings below that point. Both these months were dry, scarcely any rain falling. But the first two weeks of January 1852, were rainy; the remainder of the month dry. Sacramento City was drowned on the 1st of the month. In January 1851, there was ¾ inch of rain; in 1852, ½ inch; and in 1853, 4 inches.

February; for the first three weeks, the weather was fine. Up to the 21st there were no less than seventeen days entirely clear. In the last week there were four rainy days, but in the whole month only one day was entirely cloudy. The temperature was delightful, the means at sunrise and noon being 48° and 60°. The coldest morning 42°, and the warmest noon 67°. The prevailing winds were from north, northwest and west, and mostly light. The hills were covered with flowers. In February 1852, there were four mornings colder than in this month, and in 1851, thirteen colder mornings. February appears to be always a dry month. In 1851, there was ⅓ inch of rain; in 1852, ½ inch; in 1853, 1 inch.

March was mostly a pleasant month, with several moderate rains towards the middle, and three days of heavy rain in the last week. The prevailing winds were from West, Northwest and North, with an increasing tendency to West, and increasing force. The minimum temperature was 41°, and the maximum 77°; mean at sunrise 49½°, and at noon 62°. The first week of the month was very warm. On the 15th, Mount Diabolo was covered with snow, as mostly happens towards the end of March. There is commonly considerable rain in this month. In the dry winter of 1851, there were 2 inches; in 1852, 6½ inches; in 1853, 5 inches.

April was a pleasant month, with winds generally from West and Northwest, and frequent light sea breezes. Temperature agreeable, varying from 46° to 56° at sunrise, and from 59° to 75° at noon; means at sunrise and noon 52° and 65°. The heaviest rain for several years fell on the night of the 16th, viz: upwards of three inches in twelve hours. The only thunder of the season occurred during this rain. April mostly gives us some days of rainy weather. In 1851, an inch of rain fell; in 1852, only ½ inch; in 1853, 5 inches. The coldest morning was 46°. In 1851, there were five colder mornings, and in 1852, eighteen. Dry and cold weather go together in our winters.

May was generally warm and pleasant, the coldest morning being 47° and the warmest 62°, while the coldest noon was 61° and the

warmest 81°. The means at sunrise and noon were 53½° and 68°. The wind settled in the western quarter, and increased in force, though not offensively high. There were several slight rains, with a large portion of cloudy and broken weather. The clouds always give their parting blessing in May. In 1851, there fell ¾ inch of rain; in 1852, ⅛ inch; and in 1853 ⅛ inch.

June was uncommonly warm, the mercury ranging from 49° to 60° at sunrise, and from 60° to 84° at noon. The sea winds were constant, but not often fraught with mist. The sky was unusually clear for summer.

The weather of July was uniform, varying in temperature at sunrise from 50° to 55°, and at noon from 63° to 78°. The means at sunrise and noon were 52½° and 68°. Cloudy and misty weather prevailed and there were but four days of clear sky from sunrise to sunset.

August was a cloudy and misty month, but less so than July. Its temperature also was very uniform, ranging at sunrise from 51° to 56°, and at noon from 63° to 76°. The means at sunrise and noon were 53° and 67°. The sea winds, though constant, were not often high.

In the three summer months of 1851, there were four misty mornings, and 33 misty evenings; in 1852, 7 mornings and 27 evenings, and in 1853, 15 mornings and 36 evenings, misty.

September was rather pleasant, affording one or two days really hot. The morning extremes were 50° and 60°, and the noon extremes 63° and 88°. The sea winds continued their daily visits with diminished force, and there was much cloudy and broken weather, with two small rains near the middle of the month. The means at sunrise and noon were 55° and 70°. September usually brings a day or two of light rain. One inch fell in 1851, a few drops only in 1852, and an eighth of an inch in 1853.

October was as usual, warmer than several of the previous months. The coldest morning was 49°, and the warmest 64°; the coldest noon, 60°, and the warmest 85°. The means at sunrise and noon were 54¼° and 71°. During this month, the sea winds began to give out. The sky was was generally fair, and one slight rain fell, amounting to $\frac{1}{10}$ inch. In October, 1851, there was $\frac{2}{10}$ inch, and in 1852, ¾ inch.

November placed the usual embargo on the sea winds. The temperature was moderate, a few slight frosts occurring. The coldest morning was 44°, and the warmest 59°; the coldest noon 55°, and the warmest 73°. The means at sunrise and noon were 51° and 63°. There was much cloudy weather, with occasional moderate rains. The prevailing winds were from west and south. The first southeasterly storm, in 1851, was on the 8th; in 1852, on the 13th; and in 1853, on the 16th. Quantity of rain in the three years respectively, 2 inches, 5½ inches, and 1½ inches.

December was more pleasant than common. The coldest morning was 40°, and the warmest 54°; the coldest noon 50°, and the warmest 69°. The means at sunrise and noon were 46½° and 57½. Hoar frosts were frequent, but the cold was not sufficient to injure vegetation. There was much fair weather. A copious rain fell on the 10th, and several light rains at other times. Prevailing winds from north, northwest, northeast and south. Thunder was heard on the 10th, for the

second time in the year. In December 1850, there fell 1 inch of rain; in 1851, 7 inches; in 1852, 12 inches—the greatest quantity in any one month for three years and more; in 1853, 2 inches.

The summing up for the year 1853 exhibits a mean temperature of 51⅛° at sunrise, and 65° at noon, which is warmer by two degrees than either 1851 or 1852. The lowest mark reached by the mercury was 40°, or eight degrees above the freezing point. The extreme of heat was 88°. In 1852, the extremes were 35° and 98°; in 1851, 30° and 84°; and in December 1850, the thermometer fell as low as 28°. The amount of rain in each month of 1853, was, in round numbers, as follows: January, on eight days, 4 inches; February, four days, 1 inch; March, six days, 5 inches; April, eight days, 5 inches; May, three days, ⅛ inch; June, July and August, none; September, two days, ⅛ inch; October, one day, $\frac{1}{10}$ inch; November, eight days, 1½ inches; December, six days, 2 inches; making in the year, forty-four days on which rain fell, to the depth of 19 inches. In 1851, there was rain on fifty-three days, quantity 15 inches; in 1852, on sixty days, quantity 25½ inches. From the 1st of January, 1853, to the dry season, the quantity was 16½ inches; and from the dry season to the end of the year, 3½ inches. The last rain of the Spring was May 24th, and the first of the Autumn was September 15th. The hills began to look green in the last week of November, and at the close of the year at least thirty species of plants were in bloom around the city, some of them the lingering flowers of Summer, and a few the products of a new growth. There were two small specimens of thunder during the year, none of the aurora borealis, and a considerable sprinkling of meteors in the second week of August, and also in the fourth week of November.

4. *Mammoth Trees of California.*—An article in the Sonora Herald of August 27, 1853, contains the following statements respecting the Mammoth Trees of California, one of which was the subject of Prof. Gray's remarks in the last volume of this Journal. The tree that has been cut down was 95 feet in circumference at the ground, and 300 feet high. Another tree is lying near by, now dead. It is decayed within, and contains a cavity which for 250 feet of its length averages 10 or 12 feet in height; so that a man may enter it on horseback and ride the whole distance. From its diameter near its base, its circumference was estimated at 110 feet, and it was judged to have been near 400 feet high. Another tree still standing has a circumference near the ground of 97 feet, its height 350 feet. Not far distant there is a trio of trees, the united circumference 92 feet, and height 300, the middle one rising 200 feet without a branch. In the same neighborhood there is a twin tree, with a circumference of 90 feet; the trunks of the two parts are joined for 10 feet; the height is 325 feet. A single tree of perfect symmetry, is 92 feet in circumference and 350 feet high. There are 85 of these mammoth trees scattered over an area of 50 acres. The soil of this Mammoth Grove is moist and rich.

On the route from Sonora to the Grove, about 10 miles northward of Sonora, there are the "Natural Bridges" over the Cayote Creek, four miles below Vallecita. The rock is limestone. The entrance of the arch under the upper bridge is 25 feet wide and 30 feet high, and the

rock is variously eroded into Gothic forms and hung with stalactites. The lower bridge is of equal extent, though differing in imitating more the rounded Grecian style, than the Gothic, in its arches.

Fifteen miles north of Vallecita is Murphy's camp, and 15 miles be yond, the traveller arrives at the Mammoth Grove. Four miles before reaching it, he has the Sierra Nevada in view, bounding the horizon on the north and east, high hills between the Sugar-pine and Tuolumne on the southeast, Bald Mountain on the south, and the Bear Mountain range on the west; and the country is a mass of precipitous hills and deep recesses or shaded ravines. Nine miles north of Murphy's there is an extensive cave with many apartments.

5. *Recent Earthquake Shocks in California*, (from letters of W. P. Blake, dated San Francisco, and addressed to one of the editors.)—A slight shock of an earthquake was felt in this city by many persons on the morning of March 2d. I was sleeping with my head towards the east, and my couch seemed to have received a violent push from that direction; the windows and doors rattled at the same time. Although I had never before experienced the sensation of an earthquake, I was so strongly convinced that I had been awakened by one, that I arose and carefully noted the time (4^h 40^m A. M.). A friend who occupies a room near mine, and was lying north and south, felt the movement, and describes it as transverse to the direction of his bed. The motion was evidently very rapid, but not sufficiently extensive or violent to leave any traces of its passage.

A similar shock was felt here in January last, at 3 o'clock on the morning of the 9th. And on the third of the same month, two successive shocks were reported at Mariposa. The last steamer that arrived from Rejou, brought the intelligence that clouds of smoke and ashes were constantly rising from the crater of Mount St. Helens. It is said that the smoke issues in sudden puffs.

Earthquakes are so common all along this coast, that it becomes important to have at different points suitable instruments for recording the direction and intensity of the waves.

Another earthquake was felt in this city on the 10th of April, at 10^h 38^m A. M. It was very generally felt throughout the city. It is also stated that it was so violent at Point Lobos that glass in some of the windows was broken by the jar. The motion of the earth appeared to be in a vertical rather than a horizontal direction, producing a sensation of rising and sinking, as if a heavy body had fallen upon an elastic floor. The vibrations were short and sharp, apparently as if formed by an explosion. Two shocks were noticed, with an interval of five or six seconds between them.

6. *Memorial to the United States Congress by a Special Committee of the American Association for the Advancement of Science, on establishing a Geographical Department of the Library of Congress.*

At the meeting of the American Association for the Advancement of Science, held at Cleveland during August, 1853, a resolution was submitted providing for a special committee to prepare and present a Memorial urging on Congress "the advantages of establishing a complete, thoroughly organized, and liberally sustained Geographical Department of the Congress Library, and presenting therein such a project or plan

of organizing this Department as shall seem to the Committee best adapted to promote its final usefulness and success in relation both to the Government and to the country at large." The undersigned members of this Committee, in execution of the duty thus imposed, would now respectfully offer such views on this subject as seem most appropriate.

I. There is not in the United States, or on this continent a single collection of geographical materials which is even tolerably complete. The Harvard collection, the collection of the State Department, the Hydrographic Office, the Topographical and Engineer Bureaus, the Coast Survey, the Smithsonian Institution, and those of Libraries, Colleges, Societies and scholars generally, throughout our country, have been formed for some special and limited purpose, and hence, all are practical and imperfect. None rises to the rank of a true Geographical Library, in which should be found the means of investigating all geographical questions, both of sea and land, at home and abroad. Year by year partial localities assume the highest temporary importance; as for instance, quite recently, Hungary, the Black Sea, Japan, China, Australia, the Pacific Islands, Central America, our entire western coast, the Amazon, the La Plata, and especially our unexplored Western Territories. None can say how great the value of local information on any country, either old or new, may become in a single year. Yet, there is not on this continent any single place where we can resort with confidence for the materials requisite for precise and complete investigation on such questions, as they arise. Publications have actually been made which amply illustrate most great questions of Commerce, policy, international relations, military operations, and Science, so far as they depend on Geography; but these publications have only been systematically collected in some of the great libraries of the European capitals. Hence, we are forced to seek across the Atlantic, the means of thorough investigation into many of the important questions influenced by geographical considerations, or unquestioningly to accept the information vouchsafed by foreign investigators, by whatever prejudices or designs their views may be affected. For instance, would we follow critically the military operations between Turkey and Russia: there are not to be found in this country the geographical aids requisite for a strategic insight into the present and coming campaigns. In the French *Dépôt de la Guerre*, on the contrary, we would be able to trace each step and probability, because the French Government has been systematically collecting, through many years, all possible information on this, as on all other supposable theatres of war or policy. It is a singular and striking fact that the most extensive collection of maps now in America, even in the American department was collected by Prof. Ebeling of Hamburg, and that this collection, purchased by Mr. Thorndike of Boston, and by him presented to the Harvard Library, has been laid under serious contribution for some of our most important negotiations, especially that on the Northeastern Boundary question. Not only is there among us a signal deficiency of collections on foreign geography, but there is at best only a most imperfect and fragmentary centralization of materials illustrating the past and present geography of the states, counties, towns, cities and historical localities of the Uni-

ted States. In brief, while there are sundry partial and special collections of great value owned by scholars, societies, and the General and State Governments, there is nowhere a real and general library of Geography in the United States. Hence we consider the formation of a systematic, comprehensive and complete geographical library, as a most genuine and unequivocal *American desideratum.*

II. The materials which an American National Library of Geography should embrace may be distributed under the following classes:—1st, United States Maps, including general, state, county, town, city and village maps, and plans of historical localities; embracing not only published maps, but manuscripts or tracings of valuable unpublished surveys. 2d, United States Charts, including DES BARRE's and other early surveys, the surveys of our coast, rivers and harbors by the Engineer and Topographical Bureaus, and the coast survey, the topographical lake survey, and all municipal and private charts of importance along our entire seaboard, as BLUNT's, for instance. 3d, Foreign Maps, including the detailed Topographical Maps prepared by the several European States, such as those made by the Ordnance, French, Swiss, German, Prussian, Italian, Austrian, Russian, Swedish, Hindostan, Canadian and Cuban Surveys. Also all maps of the West Indies, of Central and South America, of the British Provinces, (including Hudson's Bay Company Maps,) the Russian Possessions, the Pacific Islands, and, indeed, *all* valuable maps of foreign domains, whether of public or private origin. 4th, Foreign and Ocean Charts, including the 2,000 British Admiralty Charts, the 1,400 charts of the French *Dépôt de la Marine*, the Spanish, Prussian, and Swedish charts, the great variety of private and Expedition charts, MAURY's wind and current charts, &c. 5th, The publications of Exploring Expeditions—especially the works of the British, French, Russian, Austrian, Spanish, and American Governmental Expeditions; as also reports of private voyages and travels of reputation. Peculiar importance should be attached to publications illustrating the early discoveries and explorations of the American continent; to subsequent explorations of our coast and interior, and the Polar, Central and South American voyages and travels. 6th, Geographical Society publications and periodicals, such as those of the Royal Geographical Society, of London; the Bombay Geographical Society; the Asiatic Researches; the Paris *Société de Géographie;* and the Russian Geographical Society; with the *Nautical Magazine*, and other similar periodicals. Also, all pamphlets on geograpical subjects should be collected with especial industry and care. 7th, Books illustrating local geography—especially within the United States; topographical descriptions; hand-books, town and city annals, and whatever can illustrate our geography in relation to history, should be collected; and the same to some extent for foreign countries. 8th, Books and maps on Physical Geography. 9th, Special atlases of cities and states—such as those of VAN DER MAELEN, JOHNSON, the Society for the Diffusion of Useful Knowledge, COLTON, &c. BAUERKELLER's series of maps in relief should be added. 10th, Works on Geodesy and Navigation, and descriptions of geodetic surveys. 11th, Works on Geographical Bibliography. 12th, A pair of first-class terrestrial and celestial globes.

It is only by great industry and skill in collecting these materials that a geographical library of the first order can be formed at all, and even then completeness can be obtained only after some years of vigorous and liberally sustained effort. The Library of the *Dépôt de la Marine* for instance, includes over 4,000 distinct works of geography, many of which extend to several volumes; besides which there is a vast collection of separate maps and charts. Such a repository can be formed only by collecting through a long series of years; though that portion which is of the greatest immediate importance, can fortunately be accumulated in a comparatively short period.

III. * * *

A Geographical Library of Congress would possess an important advantage in that prestige of name and position which would enable it to enlist, if well administered, the friendly cooperation of foreign representatives, and through them of the proper departments of their respective governments. As many of the most important geographical works have been official or governmental publications, this cooperation is actually essential to the completion of such a collection as that proposed.

A geographical library of Congress would furnish invaluable aid to the coordinate executive branches of Government, and would be a judicious and well-requited enterprise, were it only for the services it would render to the State Department, the Engineer and Topographical Bureaus, the Coast Survey, and the various Naval Bureaus. Its value in relation to river and harbor improvements, to local and general American history, to map compilations for Government and common use, to the intelligent discussion by the periodical press of important pending questions; its agency in fostering and giving a right direction to contemplated explorations, in making known the researches of foreign explorers, and in indicating what geographical researches are most essential or most worthy of patronage; these and many other prospective uses might be urged as reasons why the general government should execute this project for the benefit and in behalf of this nation.

IV. A geographical library can be formed and duly administered only by being placed under the special direction of one versed in geography as a science. In this respect it differs much from any other section of a general library. The materials to be accumulated must be procured from sources so diverse and special, that a general geographer cannot be well informed thereon. Many valuable maps and charts exist only in manuscript, and tracings should be procured and verified. This demands an acquaintance with drawing. An active and laborious correspondence would be necessary to bring together the vast number of local maps which are or will be published on the subdivisions of the United States; also the best foreign maps of cities, provinces, &c. Nor can any one but a geographer of superior capacity, attain that critical knowledge of the character and reliability of different maps and charts, which is the first essential before using them. The system to be employed in storing, arranging and indexing an aggregate of many thousand maps and charts, differs entirely from that pursued in book libraries. The charge of compiling new maps and of tracing copies for the library and for individuals, which would be cardinal features in such a library, is totally foreign to the sphere of a general librarian.

It would be incumbent on the head of this department to maintain correspondence with Geographical Societies, with explorers, and with map publishers, all of which could be done only by a geographer. Moreover, it would be highly desirable that an annual report on the progress of geographical discovery and science should be submitted to Congress for the benefit of all who are interested in this important branch. All care should be exercised in procuring prompt information on such dawning and passing events as involve geographical elements, by corresponding with well informed authorities, by collecting and studying the publications bearing thereon, and by maintaining a complete reference index in as perfect a condition as practicable.

In view of all these facts and considerations, it is evident that the Geographical Department now proposed should have special rooms, a special head, special employés, and a special administration. As a section of the general library, it could by no means be well conducted, for no general librarian living could successfully discharge special duties of the nature proposed. A distinct and coordinate organization and administration seem therefore indispensable to success. Moreover, stability of tenure for the geographical librarian and his assistants, serving as it would to perpetuate the teachings of experience, should be secured and esteemed next only to character, mental vigor, industry, method and capacity for administration. Under the general direction and control of the Joint Library Committee, a geographical section thus organized could soon be made, it is believed, an efficient auxiliary to the great interests of legislation, of commerce and of science.

V. The appropriations which this department would require during the first two or three years, would much exceed those for subsequent years, as they would be chiefly absorbed in the purchase of those geographical publications which are at once procurable. The 2,000 Admiralty charts, the 1,400 French charts, the English, Irish, French, German, Prussian, &c. surveys, the principal atlases, the English, French, Russian, Spanish, American, and other volumes of explorations, the geodetic and nautical works, the gazetteers, encyclopedias, hand-books, &c., the globes, and many other items, should be procured almost at once. For the first year an appropriation of $30,000 appears to be no more than a judicious and efficient beginning would require. For the second and third years, a less amount would suffice, and then the requisite appropriation would but slightly exceed that needed for administration, and for the purchase of the special publications of the year. A languishing and feeble beginning is peculiarly to be deprecated, and a large part of the materials now published can better be procured and made available at once than by delaying through a long term of years. The proposed collection is needed *now*, and every year that is lost will add to the difficulty of completing the portion relating to our early history.

In concluding this memorial, we would respectfully state that the project now advocated is one which can scarcely fail to commend itself to all enlightened minds. Acting as we now do in the name and in behalf of an Association embracing a large portion of the American cultivators of science, we esteem it fortunate that the interests of this Association, of Congress, and of the entire nation, are so harmonious and

even identical on the subject of this memorial. It is, therefore, with gratification and with hopeful confidence that we now commend to the liberality of Congress a project which we believe to be truly advantageous to all and injurious to none.

Respectfully submitted on behalf of the American Association for the Advancement of Science, by A. D. BACHE, J. J. ABERT, J. G. TOTTEN, A. GUYOT, M. F. MAURY, PETER FORCE, CHARLES HENRY DAVIS, E. B. HUNT.

7. *On Gold and Platinum of Cape Blanco*; by W. P. BLAKE, (from edit. corresp.)—I have recently procured a sample of the Platinum and Gold washed out from the beach sands at and near Point Oxford (Cape Blanco) on the California coast, near lat. 43° N.

The gold is in small and very thin scales. About 20 per cent. of the sample consists of platinum, also in minute round scales not much larger than grains of common writing-sand. These scales are readily lifted by a magnet, but are not easily separated from the gold. I am informed that the miners throw out as much of the platinum as possible, as it lessens the value of the gold in the San Francisco market. The amount of platinum in the gold as washed out is estimated to vary from 10 to 30 per cent.

8. *Supposed Corallines of the Colorado Desert.*—The supposed corallines observed by Mr. Blake on the borders of the region of the Ancient Lake in the Colorado Desert, are, as he suggests in a recent letter, and as we find on examining his specimens, calcareous incrustations or tufa having singular coral-like forms, resembling some Nullipores.—D.

9. *Additions to Article on Chinese and Aztec Plumagery*; by D. J. MACGOWAN.—The two popular encyclopedias, referred to on page 59, are the Keh chi king yuen, and the Yuen kien lui yen.

A beautiful article from Shensi is sometimes met with, called *kung tsióh* [peacock] jacket pieces, composed of the skin and feathers covering the head of a species of Pavonidæ, probably the *Polyplectron Tibetanus* of Gmelin and Brisson. Each piece contains about sixteen square feet, and is consequently the covering of many peacocks' heads. Yet they can be had for ten or twelve dollars. The gorgeous garments made out of these feathered skins being less warm than fur, are mostly sought by gay and wealthy ladies, by whom they are worn within doors. The prevailing tints are green and blue, of resplendent metallic lustre; these colors are of varying intensity, mutually changing into each other, or shotted according as the light falls upon them in different directions. Were not the feather garments of the Aztecs made wholly or chiefly like these, of both skins and feathers?

10. *Prof. Edward Forbes.*—Prof. Forbes has been appointed to the vacant chair of Natural History in the University of Edinburgh.

11. *Brazil.*—The eminent Botanist, Martius, has announced his intention of publishing in his general Flora of Brazil a map showing the routes of the most distinguished travellers who have visited that country, and another map giving a general view of its geognostical features.

12. *Obituary.*—ROBERT JAMESON.—Prof. Jameson died in April last at Edinburgh, in his 81st year. He was born at Leith, in 1773. He was for two years a student under Werner at Freyburg, and continued

ever afterward an ardent advocate of the views of his master. Returning from Freyburg in 1804, he was appointed Regius Professor of Natural History in the University of Edinburgh, Lecturer on Mineralogy, and Keeper of the Museum. In 1798, he published on the Mineralogy of the Shetland Isles and the islands of Canary; in 1800, appeared his "Outlines of the Mineralogy of the Scottish Isles;" in 1808 his "System of Mineralogy." In 1819 he commenced in connection with Dr. (now Sir David) Brewster the publication of the Edinburgh Philosophical Journal, after the 10th volume of which he became the sole editor. Prof. Jameson also published various memoirs, and largely promoted the progress of Science by his labors and influence.

13. *Elementary Geology;* by Edward Hitchcock, D.D., LL.D., Pres. Amherst Coll. and Prof. Nat. Theol. and Geol. New edition, revised, enlarged and adapted to the present state of the Science. 418 pp. 12mo, with numerous illustrations.—This work is too well and favorably known to require a detailed notice in this place. It has already reached its twenty-fifth edition. In preparing for this new issue, large additions were made from the stores of facts gathered by laborers in the Science in different countries, besides more than eighty new woodcut illustrations. The chapter on the operation of aqueous agencies in effecting Geological changes has been entirely re-written, and the author has embodied the results of his own valuable researches on river Terraces and Drift through the New England states and elsewhere. The size and character of the work are especially adapted to class instruction.

14. *Archives de Physiologie de Therapeutique et d'Hygiène*, sous la direction de M. Bouchardat, Prof. d'hygiène à la Faculté de Médecine de Paris. No. 1, Janvier, 1854. Mémoire sur la Digitaline et la Digitale par E. Homolle et T. A. Quevenne. Paris, Germer Bailliere.—This first number of the Archives, extending to 376 pp. 8vo, is occupied with the elaborate memoir of MM. Homolle and Quevenne on Digitaline.

15. *Lectures on Histology*, delivered at the Royal College of Surgeons of England in the session 1851–52, by John Quekett, Resident Conservator of the Mus. Roy. Coll. Surgeons of England and Professor of Histology. Vol. II, Structure of the Skeleton of Plants and Invertebrate Animals. 413 pp. 8vo, with 264 wood-cuts. London, 1854. H. Bailliere.—These lectures treat in a popular way, and in some departments with considerable detail, of the structure of invertebrate animals. The work commences with Sponges, and passes then to the Diatomaceæ, Polythalamia, Zoophytes, Echinodermata, Echini, Mollusca, and Annulata.

16. *Illustrations of the Birds of California, Texas, Oregon, British and Russian America*, intended to contain descriptions and figures of all North American birds not given by former American authors, and a General Synopsis of North American Ornithology, by John Cassin. Philadelphia, Lippincott, Grambo & Co.—No. 3 of this valuable and beautiful work completes to the 96th page. It contains descriptions of Kirtland's Owl (Nyctale Kirtlandiæ), Blanding's Finch (Embernagra Blandingiana), the American House-Finch (Carpodacus familiaris), the Long-tailed Chickadee (Parus septentrionalis), the Red-breasted Teal

(Querquedula cyanoptera), with general observations on the Falconidæ. The plates, of which there are five, are faithfully drawn, and well colored. No. 4 has also appeared, and continues the subject of the Falconidæ. This work is to be completed in 30 parts, each part to contain five plates, and the whole to form two large octavo volumes when completed. Price $1, each part.

17. *Astronomical Observations, made under the direction of* M. F. MAURY, *Lieut. U. S. Navy, during the year* 1847, *at the National Observatory, Washington.* Vol. III. Published by authority of the Secretary of the Navy. 4to. Washington, 1853.—Besides the tables of observations made at the National Observatory, this volume contains as appendix A, Observations on Solar Spots made at the Observatory of Georgetown College from Sept. 20, to Nov. 6, 1850, by Prof. B. Sestini, S. J., illustrated by 44 plates: Appendix B. Observations on the Mississippi River at Memphis, Tenn., by R. A. Marr, U.S.N.; in which it is stated that the mean temperature of the river is 60°·95 F., while that of the atmosphere is 60°·44; that the quantity of water passing Memphis per day in March, 1850, varied between 46,138,127,330 cubic feet and 88,827,520,040, the river being 76 to 83 feet deep, and in October, from 10,708,228,080 cubic feet to 16,892,279,100, the least at the close of the month, and the depth of water 52·2 to 56 feet. The silt collected in the river at this time was sent to Ehrenberg, and the Appendix on this subject closes with a Report on the species of infusoria it contained. The whole number of forms observed was 88; and out of 44 polygastrica, no new species were detected excepting a doubtful Gloconema. The high water sediment afforded 65 species, the low water 54. Ehrenberg adds:

"According to my direct investigations, the microscopic organic living part of the river mud, amounts to—

In the Ganges	($\frac{1}{8}$—$\frac{1}{4}$),	corresponding	in each	second	to	69—	139	cub.	ft.
Nile	($\frac{1}{20}$—$\frac{1}{10}$),	"	"	"	"	"	6—	13	"	"
Mississippi	($\frac{1}{50}$—$\frac{1}{33}$),	"	"	"	"	"	2—	4	"	"

"The last is evidently too small and will probably be modified by examination of the finer mud out of the current and near the shore."

The Ganges at high water carries in one second 580,000 cubic feet of water, the Nile 176,148 cubic feet, and according to the data of Marr's tables, the Mississippi carries on an average 434,711 cubic feet.

18. *Field-Book for Railroad Engineers;* containing Formulæ for laying out curves, determining Frog Angles, Levelling, Calculating Earthwork, etc., together with Tables of Radii, Ordinates, Deflections, Long Chords, Magnetic variation, Logarithms, Logarithmic and Natural Sines, Tangents, etc.; by JOHN B. HENCK, A.M., Civil Engineer. 244 pp. 12mo. New York, 1854. D. Appleton & Co.—A valuable pocket companion for the practical engineer. It is neatly printed, and put up in pocket-book style, is of convenient size, and in its contents just what a book for the purpose should be.

19. *Annual Report of the Superintendent of the U. S. Coast Survey,* showing the progress of that work during the year 1852. 174 pp. 4to, with numerous charts. Washington, 1853.—The Annual Report of Prof. Bache is one of the most valuable documents issued annually by Congress, and we rejoice to see the volume improved in quality of pa-

per and printing. The plates, which are of great value, now appear in a form convenient for consultation and preservation.

J. Müller: Uber den allgemeinen Plan in der Entwickelung der Echinodermen. 42 pp. 4to, with 8 plates. Berlin, 1853. From the Trans. of the Königl. Akad. der Wissensch. zu Berlin.

Alexander von Humboldt: Kleinere Schriften von A. von Humboldt. Erster Band; Geognostiche und physikalische Erinnerungen, with an atlas containing a view of the volcanoes of the Cordilleras of Quito and Mexico. Stuttgart and Tubingen. 1853. T. G. Cotta.

Carl Vogt: Lehrbuch der Geologie und Petrefactenkunde. 2d edition, in 2 vols. 1st vol. just issued. Bräunschweig, 1854.—An excellent work of unusual beauty in its illustrations.

R. Ludwig: Das Wachsen der Steine oder die Krafte welche die Bildung und Entwickelung der Gebirgsarten vermitteln. 226 pp. 8vo. Darmstadt, 1853.

Adolf Kenngott: Krystallformennetze zum Aufertigen von Krystallmodellen. 2d edition. Vienna, 1854.—This very cheap work is a series of plates containing figures for the construction of models of crystals. The outline of the faces of various crystalline forms is given, and they are so arranged that it is only necessary to cut out the figures from the plates and bend the planes into their places, to make the model; and if the sheets of figures are first pasted on Bristol board, the models will have all the firmness and durability desired.

William Parker Foulke: Discourse in Commemoration of the Founding of the Academy of Natural Sciences of Philadelphia, Delivered March 20, 1854. 58 pp. 8vo. *Philadelphia*, 1854.

A. Davis (Cor. Mem. N. Y. Hist. Soc., &c.): History of New Amsterdam, or New York as it was in the days of the Dutch Governors, together with papers on Events connected with the American Revolution and on Philadelphia in the Times of William Penn. 240 pp. 16mo. *New York*, 1854. R. T. Young.

The American Medical Monthly, Edward H. Parker, M.D., Editor. Vol. I, No. 4, April, 1854. *New York*. G. P. Putnam & Co.—Issued in monthly numbers of 80 pages each. Single copies 25 cts. each.

People's Journal, Alfred E. Beach, Editor. No. 86 Nassau st., New York. Vol. I, No. 6, April, 1854. One dollar a year.—A popular and well illustrated monthly of 32 small quarto pages, treating of the popular or useful arts, with horticulture, agriculture, and whatever is of interest to the farmer, etc. etc.

The Practical Mechanics Journal, Vol. VII, No. 1, May 1854. In monthly numbers of 24 pp. 4to. 25 cts each. *London* and *Glasgow*, W. & J. H. Johnson. *New York*, Stringer & Townsend, 222 Broadway.—A work of real value to the mechanic, able and thorough in its papers, excellent and full in its illustrations. It gives new processes and inventions in chemical arts as well as in manufactures, and also discriminating notices of new publications.

The Merchant's and Banker's Almanac for 1854—published by J. Smith Homans, No. 70 Wall street, New York—contains the Calendar—List of Banks—Banking Directory—Banking Laws, &c. &c.

Proceedings of the Boston Soc. Nat. Hist.—Jan., 1854.—p. 309. On the reproduction of lost parts in Reptiles; *W. I. Burnett.*—p. 311. On Rattle-snakes; *Ib.*—p. 316. On the Cotton worm of the Southern States; *Ib.*—p. 323. On the Sedative action of the poison of the Rattle-snake; *Ib.*—p. 324. List of Birds found both in Europe and America; *T. M. Brewer.*—p. 331. On the zoological nature of Infusoria; *W. I. Burnett.*—p. 353. On a remarkable case of bisexual hermaphroditism; *Ib.*—p. 368. On the nature and character of muscular tissue; *Ib.*—p. 371. On the development of Mollusks in Holothuridæ; *Ib.*—p. 377. Note on the relations of the Fossil elephant of Europe and America; *J. Wyman.*—p. 378. On Fos-

sil footmarks; *E. Hitchcock.*—On the development of viviparous Aphides; *W. I. Burnett.*

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[SECOND SERIES.]

Art. XXI.—*On the comparative Expenditure of Heat in different forms of the Air-Engine;* by Frederick A. P. Barnard, Professor of Chemistry and Natural History in the University of Alabama.

In the year 1840, an air engine of a very simple construction was patented in England by the Rev. Robert Stirling. The effective force in this engine was derived from the fluctuations of pressure created by the sudden alternate heating and cooling of a body of confined air, which was driven from end to end of the containing vessel by the motion of a solid plunger occupying about three quarters of the entire cavity. In its passage, it passed through a regenerator constructed of parallel laminæ of copper; and while one end of the air-vessel was exposed to heat, the other was furnished with a refrigerating apparatus. To the cold end was attached the working cylinder. This engine was successfully operated for several years, at the Dundee iron foundry, and perhaps elsewhere. It was found to be largely more economical than a steam engine, of corresponding power. No advantage, it will be peceived, was taken of compression pumps, to give to the air a higher pressure than that of the reservoir; and, on the other hand, the engine had not the resistance to contend with, which such pumps occasion. The confined air, however, was originally compressed to a great density. In the smaller engines a maximum pressure of 360 lbs. to the square inch was carried—equal to 24 atmospheres—but in the larger ones the pressure rose no

higher than to·240. The maximum temperature usually employed was 650° F.

In examining, in the March No. of this Journal, Mr. Joule's project of an air engine, I assumed, what I think can easily be proved, that the plan there proposed is the most economical of all which have been suggested, and which are at the same time practicable. In Ericsson's, the compression cylinders of Joule, and the regenerators of Stirling, have been united; but I have shown that the regenerators are not essential to economy. It has however, become a matter of interest to compare with each other the several forms which it has been proposed to give to the air engine, in regard to their economical merits; and it is the object of this paper to present the means for making such a comparison.

Ten years ago, the problem here under examination would hardly have admitted of an *a priori* solution. The theoretical and experimental researches of Rankine, Joule, Thompson, Regnault, Clausius, Meyer, and others, have within a recent period, put it within the reach of an easy calculation. I shall assume that it has been established by these authorities:

1st. That when a measurable mechanical effect is produced by heat, a corresponding definite amount of heat ceases any longer to exist as such; and that the measure of this amount has been satifactorily ascertained.* This proposition is convertible.

2d. That when heat is absorbed by a solid or a liquid, it is in part expended in the internal mechanical effect of overcoming cohesion, and in part in the external effect of pressure upon surrounding resistances; while in part it remains sensible: but that, in the case of a perfect gas, there is no internal expenditure; so that all the heat absorbed may be strictly accounted for either in outward work, or in elevation of temperature, or in both.

3d. That consequently, when a gas expands against pressure, but is maintained at the same constant temperature by a just supply of heat, the entire amount of heat so supplied is converted into outward mechanical effect: but that when, during expansion, the temperature rises, more heat is supplied than is so converted; and when it falls, less.

4th. That though no gas is perhaps rigidly perfect, yet atmospheric air, and those aëriform bodies generally, which are not reducible by pressure and cold to the liquid state, are sensibly so.†

5th. That equal volumes of different gases, however different in density or specific heat, when taken at the same pressure and

* Mr. Joule's severe investigation of this point has fixed the "mechanical equivalent" of a "unit of heat"—that is, of the quantity of heat which would raise the temperature of a pound of water one degree F.—at 772 pounds lifted one foot.

† The properties of a perfect gas are expressed in the law of Mariotte, viz: that the pressure is inversely as the volume, temperature being constant; and in that of Gay Lussac, viz: that the pressure is directly as the temperature (above the absolute zero), volume being constant.

temperature and equally expanded by heat under that constant pressure, perform equal quantities of work, and therefore convert into mechanical effect equal quantities of heat.

6th. That equal volumes of different gases, taken at the same pressure and temperature, and maintained during expansion at the same unvarying temperature, convert into mechanical effect different quantities of heat, proportioned to their specific heat estimated according to volume.

7th. That different gases expanding against pressure, without any accession of heat from without, convert into mechanical effect a portion of the heat which they previously held sensibly, the temperature at the same time falling; and that this depression of temperature is the more rapid in proportion as the specific heat of the gas is less. This proposition and the last are convertible.

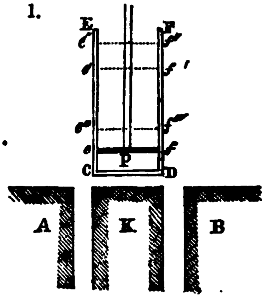
These propositions, with the aid of the specific heats of the gases furnished by Regnault, and of the formulæ of Poisson for the effect upon pressure and temperature produced by change of volume without any transfer of heat to or from the gas undergoing change, furnish all the data necessary for computing the power of any species of engine driven by the elastic force of heated gas.

It may be observed, in the first place, that the differential pressure which constitutes the driving force of an air engine may be obtained in either of two ways: first, by employing a condensing apparatus, worked by the engine itself, to compress a given mass of air, and then to sustain for a time the pressure thus secured, by application of heat; or, secondly, to subject a similar mass of air, confined in a tight vessel, simply to large alternations of temperature, without employing any auxiliary contrivance for compression. In some projects, a resort to both these expedients is aimed at; but in general one or the other will give a distinctive character to the engine.

Some projected air-engines propose to draw a fresh supply from the atmosphere at every stroke, and to discharge it when the stroke is completed; while others are designed to employ the same mass of air over and over again. In discussions of the relative advantages possessed by these different forms of construction, this difference is unimportant. We may suppose, in every case, that the mass of air is constantly unchanged; for in those in which a discharge takes place, we may suppose the discharge to be directly from the working to the supply cylinder, provided we suppose the air to be so effectually refrigerated on its way, that it shall encounter no greater resistance than that which the atmosphere itself would oppose to its free discharge. This being premised, we shall simplify the theory of the air engine, by supposing that all the fluctuations of temperature and pressure which the air undergoes, take place within the working cylinder itself. The fol-

lowing illustration is from Carnot, by whom the first attempt was made, in 1824, to present a dynamic theory of heat.*

1.



Let A be a body constantly at the temperature S°, and capable of preserving another body in contact with it at the same temperature. Let B be a second body, at the lower constant temperature T°, and capable, in like manner, of preserving another body in contact with it at the same temperature. Let CDEF be a cylinder in which moves an air-tight piston P; both cylinder and piston being impervious to heat, with the exception that the bottom, C D, of the cylinder is a perfect conductor, and without capacity for heat. K represents a stand, impervious to heat, designed, when necessary, to neutralize the conducting power of C D.

If this cylinder contain beneath the piston P, in its present position, a gas, or liquid capable of conversion into vapor, and be placed upon the body A, the piston is to be supposed to rise, while the confined gas, or liquid and vapor, is kept at the temperature S°, by the body A. If now we force down the piston to its original position, the body A being supposed to withdraw heat as it before imparted it, and always to maintain the temperature of the gas at S°, we shall have to expend precisely as much force as was developed during the expansion. But if, when the piston has reached the top of the cylinder, we remove the whole to the body B, and suppose the temperature to be instantly reduced by this body to T°, and afterward maintained at that point, then we may force down the piston with less labor than before. Thus the rising of a piston in a cylinder standing on A may force down one in another equal cylinder on B, and preserve an excess of power to be applied to other purposes.

It is practically difficult, if not impossible, by mere refrigerating contrivances, to reduce the temperature of a body of gas or vapor within such a cylinder, with sufficient rapidity or sufficient economy, to render eligible that mode of obtaining a reduction of pressure. But if we suppose the cylinder to be removed from A when the piston has reached a position short of the top, as $e'f'$, and placed on K to prevent any further supply or withdrawal of heat, and then suppose the piston to rise by the elastic force of the gas or vapor beneath, the temperature will fall by the conversion of sensible heat into mechanical effect, and artificial refrigeration will be unnecessary. Suppose that, when the piston reaches $e''f''$, the temperature is the same as that of B. The

* See Thompson's "Account of Carnot's Theory," in the Trans. of the Roy. Soc. of Edinburgh, vol. xvi, part 5, 1849.

cylinder may then be transferred to that body, and the piston pressed down, the temperature remaining at T°, until a third position, $e'''f'''$, is reached, which is such that, on again placing the cylinder on K, and completing the downward stroke, the temperature will again rise to S°. It thus appears that, of a certain definite amount of heat drawn from A, a certain less amount is given up to B, and the difference is converted into mechanical effect.*

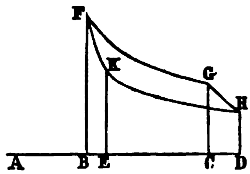
It is evident that, by reversing this whole process, that is, by first placing the cylinder on K, and allowing the piston to rise to $e'''f'''$, thereby depressing the temperature from S° to T°, then placing the cylinder on B, and allowing a further expansion to $e''f''$, the temperature being kept at T° by heat received from B, then transferring the cylinder to K again, and forcing down the piston to $e'f'$, so as to raise the temperature to S°, we may finally, by completing the downward stroke in contact with A, transfer to that body not only as much heat as was imparted by B, but also the additional amount expended in compression, above what was received during the expansion.

A gas or vapor engine constructed in conformity with this principle, is capable, therefore, if worked backward by force, of generating as much heat as is expended in its direct action, and of restoring the heat so generated to the original source. Such an engine is what Carnot defines to be a *perfect* thermo-dynamic engine. It is obvious from the nature of things, that no other description of engine, moved by heat, can convert a larger fraction of the heat drawn from the source into available power, than one of this kind: for, if there could be such an one, it would be capable of driving this one backward, and of thus constantly refunding to the source as much heat as it withdraws, while still preserving a balance of positive power—or, in other words, a balance of effect without a cause—which is impossible. We are thus furnished with a natural limit to the power obtainable by the expansion and compression of elastic fluids between given limits of temperature. Our first object then, must be to find a general expression for this effect, for the perfect engine. The illustrations which immediately follow are substantially derived from Thompson and Clausius, and would be superfluous, had the investigations of those writers been republished in this country.†

* According to Carnot's theory, which was identical with that which has hitherto been universally received, the amount of heat given up to B was *not* less than that derived from A, but exactly equal to it: hence the mechanical force exerted was presumed to be a natural concomitant of the transfer of heat from a hotter to a colder body, and not a conversion of the heat, or of any part of it, into mechanical effect.

† Clausius, on the Moving Force of Heat, &c., from Poggendorff's Annalen, vol. lxxix, republished in the Lond. and Ed. Phil. Mag., July and August, 1851. Thompson, in the paper above quoted—also Lond. Phil. Trans., part 1, 1852, and elsewhere.

2.



Suppose a column of atmospheric air, whose altitude is equal to A B, to be confined in a cylinder by a piston capable of moving without friction. Let the pressure it exerts upon the piston at the temperature S° be represented by the ordinate B F. Let it expand with unvarying temperature until the piston reaches the point C, and let the ordinate C G represent the pressure at that time. Let it now expand without receiving or imparting heat, from C to D, during which time the temperature falls to T°, and the pressure to D H. Let it now undergo compression, maintaining the invariable temperature T°, until the piston reaches E, a point such that the further compression, (without gain or loss of heat,) to the original volume, A B, shall restore the original temperature S°, and pressure, B F. It is evident that the area, F G H D B will represent the entire mechanical effect produced during the expansion, and the area, F K H D B will represent the force required to restore the air to its original state. Hence, the differential area, F G H K, will be the measure of the amount of heat converted into available force.*

Represent the original volume of the air, A B, by V', and the volume A C by V''; also A D by $V_{\prime}$ and A E by $V_{\prime\prime}$. Put τ' for the original temperature, S°, reckoned from the absolute zero (taken at 459° below 0° F.), and $\tau_{\prime}$ for the temperature T°, similarly reckoned. Then, according to Poisson's formula for temperatures as affected by expansion or compression,

$$\frac{\tau_{\prime}}{\tau'}=\left(\frac{V''}{V_{\prime}}\right)^{\gamma-1}, \text{ and also } \frac{\tau_{\prime}}{\tau'}=\left(\frac{V'}{V_{\prime\prime}}\right)^{\gamma-1}; \text{ or } \frac{V''}{V_{\prime}}=\frac{V'}{V_{\prime\prime}}$$

Now the area, G H D C, which measures the mechanical effect produced during the second expansion, may be found by integrating the expression $\int pdv$ between the limits V'' and $V_{\prime}$; substituting for p its value from Poisson's formula for pressure under these circumstances, viz.:

$$p=P''\left(\frac{V''}{v}\right)^{\gamma},$$

where P'' represents the pressure corresponding to V''. Hence the area in question is expressed by the formula,

$$\frac{P''V''}{\gamma-1}\left(1-\left(\frac{V''}{V_{\prime}}\right)^{\gamma-1}\right).$$

And, in like manner, P' representing the pressure corresponding to V', the area F K E B, which measures the force expended in the final compression, is expressed by

* Since the surrounding medium—the atmosphere, for instance—aids the compression as much as it opposes the expansion, no account need be taken of it.

$$\frac{P'V'}{\gamma-1}\left(1-\left(\frac{V'}{V_{\prime\prime}}\right)^{\gamma-1}\right).$$

But, according to Mariotte's law, $P'V'=P''V''$; and we have just seen that $\frac{V''}{V_{\prime}}=\frac{V'}{V_{\prime\prime}}$: whence the area G C D H is equal to the area F K E B; or, in other words, the second expansion is exactly balanced by the second compression:—a conclusion which we might, indeed, have independently drawn, since the same amount of heat which disappears in the expansion, reappears in the compression.

The area F G C B, according to the third general principle stated above, is the measure of the total amount of heat which the air has received from the source, converted into mechanical effect. To find its value we observe, first, that if p, v and t represent the simultaneous pressure, volume and temperature (reckoned from the absolute zero) of any gas, the expression $\frac{pv}{t}$ will be, for that gas, a constant quantity. And if we take p_0, v_0 and t_0 to express the values of these variables under ascertained circumstances, as for instance at Fahrenheit's zero, and under a given barometric pressure, then $\frac{p_0 v_0}{t_0}$ may be put $=\mathrm{R}$, and in any other condition of the gas we have $pv=\mathrm{R}t$, and $p=\frac{\mathrm{R}t}{v}$.

Now the differential of F G C B is pdv; whence, representing it by M (mechanical effect), we have,

$$M=\int\frac{\mathrm{R}t}{v}\,dv=\int\frac{\mathrm{R}\tau'}{v}\,dv=\mathrm{R}\tau'\text{ h. l. }\frac{V''}{V'},$$

between the limits V′ and V″.

In like manner, area K H D E $(=M')$ will be

$$M'=\mathrm{R}\tau_{\prime}\text{ h. l. }\frac{V_{\prime}}{V_{\prime\prime}}=\mathrm{R}\tau_{\prime}\text{ h. l. }\frac{V''}{V'}.$$

And the differential area, F G H K, which we will represent by W, will be

$$W=M-M'=\mathrm{R}(\tau'-\tau_{\prime})\text{ h. l. }\frac{V''}{V'}.$$

If H, then, be the total amount of heat received by the gas, and of which the entire mechanical effect is represented by M, the fraction converted into available work will be found thus:

$$M:W::\mathrm{R}\tau'\text{ h. l. }\frac{V''}{V'}:\mathrm{R}(\tau'-\tau_{\prime})\text{ h. l. }\frac{V''}{V'}::H:H\frac{\tau'-\tau_{\prime}}{\tau'}.$$

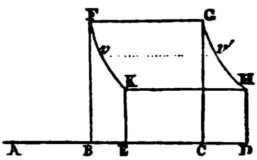
It is geometrically evident that no larger a fraction of the heat absorbed by a gas fluctuating between the temperatures τ' and $\tau_{\prime}$ can be made available, than that here represented; for the differ-

ential area cannot be enlarged unless the curve F G rise in some point above, or the curve K H descend in some point below, the logarithmic curve; which neither can do without transcending the limit τ' or $\tau_{,}$.

The practical difficulty of maintaining a body of air, while undergoing dilatation or compression, at an unvarying temperature, will render it always, probably, an impossibility to work an engine on this principle, and therefore to realize so large an advantage from the heat expended, as the extremes of temperature in furnace and refrigerator might lead us theoretically to anticipate. It is easy, however, to secure a constant *pressure*, and this presents the question of economy under a new form.

Suppose the air to expand from B to C, with rising temperature, and a constant pressure represented by F B = G C, then to expand, without receiving or imparting heat, to D, at the lower pressure H D, then to be compressed to E, with loss of heat and constant pressure E K = H D, and finally to be compressed to its original bulk, acquiring at the same time by the compression, and without receiving or imparting heat, the original temperature and pressure. The differential area, F G H K, bounded by the parallel straight lines F G, and H K, will represent the balance of positive mechanical effect. If we draw a line, as $v\,v'$, parallel to F G, it will represent the difference of volume of the air when at equal pressure in the two opposite processes of expansion and compression. Calling these volumes v and v', and the corresponding pressure p, we shall have (using the other symbols as before),

3.



$$p = P'\left(\frac{V'}{v}\right)^{\gamma} = P''\left(\frac{V''}{v'}\right)^{\gamma}$$

Or, as $P' = P''$, $\frac{V'}{v} = \frac{V''}{v'}$, and

$$V'' : V'' - V' :: v' : v' - v = \frac{v'(V'' - V')}{V''}$$

But $v' = V''\left(\frac{P''}{p}\right)^{\frac{1}{\gamma}}$; whence $v' - v = (V'' - V')\left(\frac{P''}{p}\right)^{\frac{1}{\gamma}}$.

Now area F G H K $= \int_{P_{,}}^{P''} (v' - v)dp = \int_{P_{,}}^{P''} (V'' - V')\left(\frac{P''}{p}\right)^{\frac{1}{\gamma}} dp$

Or, observing that dp is negative,

$$W = \frac{\gamma}{\gamma - 1}(V'' - V')\left(P'' - P''^{\frac{1}{\gamma}} P_{,}^{\frac{\gamma-1}{\gamma}}\right) = \frac{\gamma P' V'}{\gamma - 1}\left(\frac{V''}{V'} - 1\right)\left(1 - \left(\frac{V''}{V_{,}}\right)^{\gamma - 1}\right)$$

Where $P_{,}$ represents the pressure at D or E, and is found in terms of P'' by the formula $\frac{P_{,}}{P''} = \left(\frac{V''}{V_{,}}\right)^{\gamma}$; after which P' is put for its equal P''.

We have now four temperatures. That which we have called τ' must be raised in the same ratio as the volume is increased during the expansion at constant pressure. Put τ'' for the temperature at volume V'', and also $\tau_{,}$ and $\tau_{,,}$ for those corresponding to $V_{,}$ and $V_{,,}$.

The mechanical equivalent of the total amount of heat in the air at volume V' and pressure P', may be found thus. In expanding from V' to any other volume, v, the pressure (p) becomes

$$p=P'\left(\frac{V'}{v}\right)^{\gamma}$$

And the work done, during an infinite expansion, will be

$$\int_{V'}^{\infty} pdv = \int_{V'}^{\infty} P'\left(\frac{V'}{v}\right) dv = \frac{P'V'}{\gamma - 1}.$$

If, then, γ represent the ratio of the specific heat of air at constant pressure to that at constant volume, the mechanical equivalent of the heat absorbed, while the temperature is rising from τ' to τ'' and the pressure is constant, will be

$$\frac{\gamma P'V'}{\gamma-1}\left(\frac{\tau''-\tau'}{\tau'}\right)=M.$$

And, in the value of W above, $\frac{V''}{V'}=\frac{\tau''}{\tau'}$, and $\left(\frac{V''}{V_{,}}\right)^{\gamma-1}=\frac{\tau_{,}}{\tau''}$

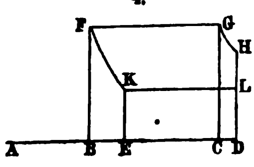
which, substituted, give us $W=\frac{\gamma P'V'}{\gamma-1}\left(\frac{\tau''-\tau'}{\tau'}\right)\left(1-\frac{\tau_{,}}{\tau''}\right)$

Whence $$\frac{W}{M}H=\frac{\tau''-\tau_{,}}{\tau''}H;$$

or the fraction of the heat absorbed which is converted into available work, is equal to the range of depression divided by the maximum temperature, as before.

In an air engine which discharges the air from the working cylinder at a pressure above that at which it is received into the supply cylinder, a less simple formula is required to express the economical ratio. In this case, if the valves are adequate to afford instant relief to the excess of pressure, the effect is the same as if the air, without being set free, were suddenly refrigerated to such an extent as to reduce the pressure to an equality with that of the supply before compression.

4.



In this case, F B C G may represent, as before, the work, done at the constant pressure P', and G C D H, that during the second expansion. D E K L will be the representative of the effect of the first compression; and F B E K that of the second.

$$\text{Then FBCG}=P'(V''-V')=P'V'\left(\frac{V''}{V'}-1\right)=P'V'\left(\frac{\tau''}{\tau'}-1\right)$$

$$\text{And GCDH}=\int_{V_{,}}^{V''}P''\left(\frac{V''}{v}\right)^{\gamma}dv=\frac{P''V''}{\gamma-1}\left(1-\left(\frac{V''}{V_{,}}\right)^{\gamma-1}\right)=\frac{P'V'}{\gamma-1}\frac{\tau''}{\tau'}\left(1-\frac{\tau_{,}}{\tau''}\right)$$

$$\text{Also KEDL}=P_{,,}(V_{,}-V_{,,})=P_{,,}V_{,,}\left(\frac{V_{,}}{V_{,,}}-1\right)=P'V'\left(\frac{V'}{V_{,,}}\right)^{\gamma-1}\left(\frac{V_{,}}{V_{,,}}-1\right)$$

$$\text{And FBEK}=\frac{P'V'}{\gamma-1}\left(1-\left(\frac{V'}{V_{,,}}\right)^{\gamma-1}\right)=\frac{P'V'}{\gamma-1}\left(1-\frac{\tau_{,,}}{\tau'}\right).$$

In the third expression above, we have $\frac{V_{,}}{V_{,,}}$, the value of which we obtain by observing that,

$$V_{,}=V''\left(\frac{\tau''}{\tau_{,}}\right)^{\frac{1}{\gamma-1}},\text{ and }V_{,,}=V'\left(\frac{\tau'}{\tau_{,,}}\right)^{\frac{1}{\gamma-1}}\therefore\frac{V_{,}}{V_{,,}}=\frac{V''}{V'}\left(\frac{\tau_{,,}\tau''}{\tau_{,}\tau'}\right)^{\frac{1}{\gamma-1}}$$

$$\text{And KEDL}=P'V'\frac{\tau_{,,}}{\tau'}\left[\frac{\tau''}{\tau'}\left(\frac{\tau_{,,}\tau''}{\tau_{,}\,\tau'}\right)^{\frac{1}{\gamma-1}}-1\right]$$

$$\text{Whence the area FGHLK}=P'V'\left[\left(\frac{\tau''}{\tau'}-1\right)-\frac{\tau_{,,}}{\tau'}\left(\frac{\tau''\left(\frac{\tau_{,,}\tau''}{\tau_{,}\,\tau'}\right)^{\frac{1}{\gamma-1}}-\tau'}{\tau'}\right)\right]$$
$$+\frac{P'V'}{\gamma-1}\left[\frac{\tau''}{\tau'}\left(1-\frac{\tau_{,}}{\tau''}\right)-\left(1-\frac{\tau_{,,}}{\tau'}\right)\right]=W.$$

The value of M is of the same form as before, since it depends only on V' and V''.

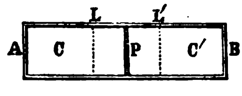
$$\text{Hence }\frac{W}{M}H=H\left[\frac{\gamma-1}{\gamma}\left(1-\frac{\tau_{,,}\tau''}{\tau'}\,\frac{\left(\frac{\tau_{,,}\tau''}{\tau_{,}\,\tau'}\right)^{\frac{1}{\gamma-1}}-\tau'}{\tau''-\tau'}\right)+\frac{1}{\gamma}\left(1-\frac{\tau_{,}-\tau_{,,}}{\tau''-\tau'}\right)\right]$$

When $\tau_{,,}:\tau'::\tau_{,}:\tau''$, this expression becomes, as before $=H\frac{\tau''-\tau_{,}}{\tau''}$

When the maximum and minimum temperatures are given, the economy of working, in an engine of this description, depends on the intermediate temperatures $\tau_{,}$ and τ'. If, when the foregoing proportion holds, we vary $\tau_{,}$ and not τ', we lose, either way. But there may be a slight increase in the economical ratio, by increasing τ' a little above the value $\frac{\tau_{,,}\tau''}{\tau_{,}}$. This fact is illustrated in my article published in the March No. of this Journal. The effect of an increase of τ', is upon the whole to elevate the *source* of the heat, since all that is absorbed is received above this temperature. It is hardly necessary to say that these two quantities may be made to vary, by varying the proportions of the cylinders of the engine, or the position of the cut-off.

In the engine of Stirling, and all others resting on the same principle, we arrive at a similar form of expression for the economical ratio, though the expression for the work done assumes a simpler shape. In order to fix our ideas, we must first examine a theoretic case, which, in practice, it is somewhat difficult to realize.

5.



Let A B be an air-tight cylinder, in which a piston P, capable of moving without friction, separates completely two equal masses of air, C and C′. Suppose that, by the alternate and instantaneous changes of temperature of these masses, the piston traverses the space L L′. When it reaches one of these limits, let the temperature of the air before it be τ', and of that behind it $\tau_{\prime}$. Then, by an instantaneous change, let the former temperature rise to τ'', the maximum, and the latter fall to $\tau_{\prime\prime}$, which is the minimum. There are but two volumes here to be considered, and we may represent the minimum by V and the maximum by V′. Put also P = pressure at minimum volume, and P′ = pressure at maximum volume, both being taken at the end of the stroke, and before the change of temperature. Then the maximum pressure will be $P\frac{\tau''}{\tau'}$; and in accordance with what we have already seen, the positive power exerted at each stroke will be

$$\frac{PV}{\gamma-1}\frac{\tau''}{\tau'}\left(1-\left(\frac{V}{V'}\right)^{\gamma-1}\right)$$

And the resistance $\frac{PV}{\gamma-1}\left(1-\left(\frac{V}{V'}\right)^{\gamma-1}\right)$

Whence $$W=\frac{PV}{\gamma-1}\left(\frac{\tau''}{\tau'}-1\right)\left(1-\left(\frac{V}{V'}\right)^{\gamma-1}\right)$$

But, in order to eliminate τ', so that the expression may contain only the maximum and minimum temperatures, which, being those of the source of heat and the refrigerator, may be supposed to be controllable, or known, we may take

$$\tau'=\tau_{\prime\prime}\left(\frac{V'}{V}\right)^{\gamma-1} \quad \text{whence}$$

$$W=\frac{PV}{\gamma-1}\left(\frac{\tau''}{\tau_{\prime\prime}}\left(\frac{V}{V'}\right)^{\gamma-1}-1\right)\left(1-\left(\frac{V}{V'}\right)^{\gamma-1}\right)$$

Also, as in the former case, the mechanical equivalent of the heat imparted is, (vol. being constant during the heating,)

$$\frac{PV}{\gamma-1}\left(\frac{\tau''}{\tau'}-1\right)=\frac{PV}{\gamma-1}\left(\frac{\tau''}{\tau_{\prime\prime}}\left(\frac{V}{V'}\right)^{\gamma-1}-1\right)=M.$$

Whence $$\frac{W}{M}H=H\left(1-\left(\frac{V}{V'}\right)^{\gamma-1}\right)=H\left(1-\frac{\tau_{\prime}}{\tau''}\right)=H\left(\frac{\tau''-\tau_{\prime}}{\tau''}\right)$$

If, in the expression for W, above, we make $V_{\prime}$ variable, we shall find that the maximum effect is obtained from a body of air, of which the minimum bulk and pressure are expressed by PV, when

$$V' = V\left(\frac{2\tau''}{\tau'' + \tau_{\prime\prime}}\right)^{\frac{1}{\gamma-1}}.$$

An expansion to a volume approaching or exceeding two-fold is therefore usually necessary to obtain the greatest effect; and the ratio rises rapidly as the maximum temperature is increased while the minimum is constant; the index $\frac{1}{\gamma-1}$ being about 2·44 for air. For a maximum temperature of 480° F., and a minimum of 60°, V′ will be 1·8 V. For 750° maximum and the same minimum, V′ will be 2·25 V.

As the expression $\left(1 - \left(\frac{V}{V'}\right)^{\gamma-1}\right)$ is the measure of the heat made available, it is evident that the economy will increase with the expansion; but there occurs here, as in the other form, a negative pressure at the end of the stroke, after passing a certain limit. P′, which has been taken for the final pressure, will be expressed thus,

$$P' = P\frac{\tau''}{\tau_{\prime\prime}}\left(\frac{V}{V'}\right)^{2\gamma-1}$$

In the two cases above supposed, P′ = ·59P for the first, and ·52P for the second.

If we would impose a condition that there shall be no negative pressure at the end of the stroke, or that the final pressure shall bear any ratio, expressed by n, to the resistance, we shall have

$$P' = nP = P\frac{\tau''}{\tau_{\prime\prime}}\left(\frac{V}{V'}\right)^{2\gamma-1}, \text{ or } n = \frac{\tau''}{\tau_{\prime\prime}}\left(\frac{V}{V'}\right)^{2\gamma-1};$$

and if we make $n = 1$, we shall have

$\frac{V'}{V} = \left(\frac{\tau''}{\tau_{\prime\prime}}\right)^{\frac{1}{2\gamma-1}}$ which limits the expansion to about once and a half the minimum volume, in the cases foregoing; V′ being equal to 1·4 V and to 1·6 V in those cases respectively.

By substituting 2V and 1·5V successively, for V′, in the expression $\left(1 - \left(\frac{V}{V'}\right)^{\gamma-1}\right)H$, we find that the fraction of heat converted into available power is ·25, in the first instance, and ·15 in the second. Engines upon this principle will not therefore compare, in point of economy, with those which use compression cylinders. This will be manifest by comparing these results with those given in the March number of this Journal. It is true that, by the use of the regenerator, there may be theoretically a large saving. All the heat absorbed, which is not expended in work-

ing, may, in theory, be taken up again by a regenerator; but it does not follow that all may be again restored to the air. In order to save all the heat, the regenerator should be capable of reducing the temperature, at the close of the stroke, to $\tau_{\prime\prime}$; but all which it absorbs between τ' and $\tau_{\prime\prime}$ will be lost. Moreover, a regenerator cannot maintain a temperature lower than τ' on its coldest side, so that it will be incapable of alone depressing the temperature sufficiently. All the heat taken in cooling between the limits just named, is invariably lost.

There is, of course, in this form of engine, as in that which employs compression cylinders, a point beyond which a regenerator would be of no avail. This contrivance is confined in its efficiency, to the limits of temperature, $\tau_{\prime}$ and τ'. If we put these temperatures equal to each other, we shall have

$$\tau_{\prime} = \tau''\left(\frac{V}{V'}\right)^{\gamma-1} = \tau' = \tau_{\prime\prime}\left(\frac{V'}{V}\right)^{\gamma-1} \quad \text{or} \quad \left(\frac{\tau''}{\tau_{\prime\prime}}\right)^{\frac{1}{2(\gamma-1)}} = \frac{V'}{V}.$$

Thus, in case that τ'' is double of $\tau_{\prime\prime}$ which will be approximately the case in practice, there will be no advantage derived from a regenerator, when the expansion exceeds, $2\frac{1}{3}$V; but as this is beyond the limit of maximum power, the limitation is of little practical importance.

The difficulty of employing a regenerator with thoroughness at all, is a more serious disadvantage. Stirling was able to pass but a portion of the air through this contrivance: and, as a general rule, where the working piston is in direct contact with the air during the heating process, a certain portion of the mass must escape heating or refrigeration, or must be very imperfectly affected. Moveable regenerators, to take the place of Stirling's plungers, have been suggested by several persons; but besides that their weight would be an objection, they would be less easily kept down in temperature on the cold side, while it does not appear that they have any decided advantage over fixed ones.

There is also great difficulty in applying furnace heat to the air in these engines. This was one of Stirling's most serious troubles; and mainly in consequence of this fact, it is probable that no new attempt will be made to construct an engine strictly on the principle now under consideration.

By a modification of the principle, however, and by employing two supply (or heating) cylinders, in aid of each working cylinder, in one of which the air is in preparation, while in the other it is expanding into the working cylinder beneath the piston, an approach may be made to a realization of the degree of power which theory indicates.

Those who have turned their attention to the planning of engines without compression cylinders, have done so chiefly for the sake of getting rid of what has appeared to them a great evil, in

the resistance of the supply cylinders. But to secure the same power from the same mass of air between the same limits of temperature, on this principle, we must employ a degree of expansion which will produce precisely the same negative pressure at the close of the stroke, which the compression cylinders create. If we work without negative pressure, we do only what can be done in the other form of engine by lengthening the cut-off. And if, in this one, we use air previously condensed, as Stirling did, we do only what Ericsson is doing now. Moreover, if we allow no negative pressure, the range of temperature through which we work, must be very limited.

It is true that the pressure, per square inch of piston surface, will, other things being equal, be in favor of the engine without compression cylinder. But it is a fallacious conclusion to infer that therefore the effective power of the engine will be increased. If we put a to represent the area of the piston, then the length of the stroke will be $\frac{V' - V}{a}$; whereas in the other form the stroke is $\frac{V'}{a}$, and this larger motion is a full compensation, other things being equal, for the less mean pressure. On these accounts, and from the much greater facility of heating and cooling the air on the Ericsson plan, the preference seems to be due to that form of the engine.

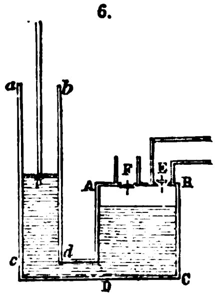
It is to be observed that, in all these comparisons, *the mass of air heated* is supposed to be the same. To compensate for the disadvantages of engines on the Stirling principle, it has been proposed by some to greatly increase the volume of air heated, to make the expansion a less fraction of the volume, and thus to carry a high pressure to the end of the stroke. This is to contradict the principle apparent in all the formulæ above obtained, that true economy requires a large range of fall in temperature in working: for though the regenerator is presumed to compensate for the absence of this, it cannot do so unless, as in Stirling's engine, it acts upon the air in the cylinder itself, or at least without allowing it to expand beyond the capacity of the cylinder—a matter of great difficulty—and at best it furnishes but an inadequate compensation. Moreover, as much advantage is gained by increasing the mass of air heated, in one form of engine, as in another. And since one of the main difficulties is to heat the air at all, it is an unwise expedient to attempt to provide against other disadvantages, by adding to this very serious, and hitherto, on the large scale at least, apparently insurmountable one.

It is perhaps worth considering, whether a more serviceable engine than has yet been invented, might not be made by combining, to a certain extent, the two principles. If two supply cylinders alternately furnish the charge for one working cylinder,

and if each supply cylinder somewhat condenses its charge and holds it exposed to heat, while the other is acting, and finally if the heated charge is not now, as in Ericsson's plan, forced into the working cylinder, but allowed to expand into it, we shall have probably a more thorough heating (or *time* for it at least) than has yet been secured, a higher mean pressure (compensated however by reduced length of stroke) and what is of principal importance, a larger working range of depression of temperature without negative pressure, than either principle alone will furnish. The number of parts would be increased by such a construction, and the machine would become less suited for locomotion than at present; but, as the supply pistons would act alternately, there would be no larger amount of friction to overcome than in Ericsson's.

One of the most serious obstacles in the way of the success of the air-engine, is the difficulty of obviating the effects of the great heat in the working cylinder. This is injurious or destructive to every thing organic, and renders close packing almost impossible. Could a liquid be found, capable of enduring a high heat, this might be made the medium of transmitting pressure, and the working cylinder might be kept absolutely cold. Let *abcd*, for instance, be a cylinder, in which moves the piston, P. Let this cylinder communicate with the larger one A B C D, closed at top, but communicating with an air heater, by the valve E. A B C D being filled with a liquid, and also *abcd*, up to the working piston, air may be admitted to the first, which will transmit its pressure through the liquid to the piston. A valve, F, may then discharge the air. By keeping the bottoms of the vessels, and the connecting channel, cold, the working cylinder will be likewise kept cool. No known liquid, however, would answer this purpose, unless it should be oil; and that would not answer at the high temperatures which have been proposed.

6.



Perhaps the idea is not entirely absurd of filling the chamber A B C D with the fusible alloy of lead, tin and bismuth, which liquefies at about 212° F. The high specific gravity would render great change of level undesirable, and hence A B C D might bear a large ratio in cross section to *abcd;* while the latter cylinder might still contain oil, and the alloy might be chiefly confined to A B C D and the communicating passage.* By this means the temperature of the working cylinder might be kept lower than that of a high pressure steam engine. The tendency of the alloy to oxydize would be an evil which would require to

* Below the range of the piston, however, *abcd* should be equal to A B C D.

be provided against in some manner—perhaps by employing air previously condensed, of which the oxygen had been converted into carbonic acid by passing thoroughly through the fire.

Since the great advantages which the air-engine holds out seem to be so nearly within our reach, and since we seem at present to be debarred from them only by obstacles such as the ingenuity of man has heretofore repeatedly surmounted, it is not only greatly to be hoped, but even to be reasonably expected, that we may soon see the invention perfected, and the important object which has hitherto in a great measure frustrated effort, successfully achieved.

University of Alabama, April 25, 1854.

Art. XXII.—*On the first Hurricane of September* 1853, *in the Atlantic; with a Chart; and Notices of other Storms:* by W. C. Redfield.

(Concluded from p. 18.)

West African Hurricanes, and Gales of the Eastern Atlantic between the Tropics.

As the great hurricane whose path we have already indicated, appears to have been of African origin, it may be well to show that the occurrence of storms in this region is not uncommon.

1. A violent hurricane swept over St. Nicholas, one of the Cape Verde Islands, lat. 16° 33′ N., lon. 24° 20′ W., on the second day of September, 1850. Its duration exceeded twenty-four hours; although the chief damage was done in three or four hours, during the morning of that day. All the crops, and nearly six hundred houses, were completely destroyed.* The marine accounts from the vicinity, date this gale on the third; doubtless in nautical time.

The ship *Sir Robert Peel*, for Bombay, after a run of about 120 miles from Bona Vista, encountered this hurricane Sept. 3d, and was completely dismasted.

The *New Margaret* was dismasted in the hurricane on the same day, in lat. 18° N., lon. 25° W.

Ship *Sir Edward Parry*, was in the hurricane Sept. 4th, off the Cape Verde Islands; St. Antonio bearing E. N. E., about 80 miles, [lat. 16° 30′ N., lon. 26° 40′ W]. It came on from eastward, increasing in violence till it blew the masts out of the vessel, while under bare poles.

H. M. S. *Portland*, encountered the gale in this vicinity.

The *Eliza Johnson* was spoken Sept. 20, in lat. 6° N., lon. 22° W., having lost mizenmast and topmasts in the gale, about two weeks before.

* London Times, Feb. 1st, 1851: p. 3.

Most of these vessels put into Rio Janeiro, where these reports were obtained by Capt. Theodore Lewis, from whom I received them in New York.

I can find no reason for doubting the continental origin of this hurricane. Its progression was evidently slow: and its subsequent course is placed under some doubt by the following report from the *Russell.*

The *Russell*, from Salem for Rio Grande, was spoken 24th Sept., lat. 4° N., lon. 20° W., by the *Richard Thornton*, arrived in the Thames from Batavia, and reported having experienced a hurricane on the 6th Sept., in lat. 28° N., lon. 32° W., in which she lost fore-topmast and main top-gallant-masts, boats, &c., also topsails, courses, jib., &c. blown away.

The position and date here given, led me first to lay down the track of this gale as having recurved on a route which passes between Teneriffe and the Azores. But the meteorological observations made by the British consuls at the Azores and Madeira, for the English Government, with other observations collected by Mr. Hunt, Consul General at St. Michaels, which were communicated by the government to Col. Reid, and by him kindly sent to me, do not render this course probable: unless the gale passed near to the Canary Islands, from whence no definite report could be obtained. The route of the gale, therefore, was probably westward; corresponding to Track xxiv. If we suppose the correct latitude to have been 18°, instead of 28°, it will place the *Russell* in a far more probable position, and one which sufficiently coincides with the foregoing reports. The nautical date, however, will then appear about one day in advance; unless the progression of the storm was at the low rate of about five miles an hour The log-book of the *Russell* might solve these doubts. On the westerly course thus indicated, the gale may have passed Bermuda about the 15th of Sept., where there were full indications of the proximity of a slow moving gale. This would show an average progression of between eight and nine miles an hour. Track xxiii.

2. Mr. Piddington has adduced the case of a cyclone passing out from the coast of Africa, to the northward of the Cape de Verdes, on a W. by N., or W. N. W. course, giving to the ship *Devonshire* as she first stood to the S. S. W., and then hove to, about 120 miles westward of St. Antonio, a severe gale from N. E. to South.*

I add here notices of three other gales, in this part of the Atlantic.

3. The *Superior*, from Harbor Grace for Barbadoes, reports as follows: Oct. 14th, 1850, in lat 24° 59′, lon. 47° 10′, experienced

* Piddington's Horn Book for the Law of Storms; 2nd edition, p. 81.

a terrific hurricane, which capsized the vessel at 5 A. M.; cut away both masts, when she righted, and all hands got safely on board again; water eighteen inches above the cabin floor; succeeded in clearing the wreck, and getting under jurymasts.

4. Ship *Damascus*, from Philadelphia for San Francisco, on the 18th of October, 1850, in lat. 25° 58′ N., lon. 41° 19′ W., encountered a severe hurricane, split foresail, main spenser and jib; also blew away main-topsail: after the stormsails were blown away the ship became unmanageable. On the night of the 18th the hurricane moderated.—See the positions on the Chart; marked XXV and XXVI.

The next case, in Sept. 1853, I find in Maury's Sailing Directions, 6th edition; received from the author.

5. The ship *John Wade*, for San Francisco, Sept. 27, lat. 17° 44′ N., lon. 35° 10′ W.; barometer 29·90; wind E., fresh breezes and clear. Sept. 28, lat. 15°, lon. 34° 50′, barometer 29·40; winds E. and E. S. E. First part, fresh breezes; middle part, strong gale. At 8 A. M. hove to under close reefed main-topsail. At 8, barometer 29·60; at 10, 29·7; at 12 M., 29·3. Sept. 29, lat. 14° 32′, lon. 34° 31′, barometer 29·60; winds W., S. S. W. Heavy gale, with violent squalls of wind and rain; middle part, sharp lightning; latter part, moderate; made sail. Capt. Little adds, "I think I was near the track of a hurricane."

The position of this gale appears to coincide nearly with the route of our hurricane of Track XXIV, which was four weeks earlier. The reported directions of wind indicate that Capt. Little crossed the center-path while within the limits of the gale. See XXXI of Chart.

6. To this series may be added a gale or hurricane encountered by Capt. Lavender, in the ship *Roman*, from Canton, Aug. 21, 1832, in lat. 12° 51′ N., lon. 39° 26′ W.: in which, according to Capt. L.'s memorandum,—split the fore-topsail, and scudded five hours under bare poles: ending with cross seas from *N. E.* and *southward.* The center-path of this gale was probably a little south of track XXIV on the Chart.

Other notices of gales in this region have met my eye, in former years; and one shipmaster stated to me that he had encountered, off the Cape Verde Islands, a severe gale of three days duration. This seems to indicate a remarkably slow rate of progression in that gale.

7. Capt. Fitzroy informs us, that on leaving Rio Janeiro for the Cape Verde Islands, early in August 1830, he first steered eastward and crossed the equator far east, which carried him into that tract of ocean between the trades which "in August and September is subject to westerly winds,—sometimes extremely

strong,—and encountered *a very heavy gale;* although so near the equator."* This is likely to have been one of the gales of August which afterwards visited the western and northern portions of the Atlantic, with great severity. Indeed, I strongly suspect this to have been the gale which passed St. Thomas on the 12th, and New York on the 17th of the month; as shown in my first paper on the character and progress of these gales.†

Capt. Fitzroy states, also, that at Port Praya, [lat. 14° 53′ N., lon. 23° 30′ W.,] no vessel should deem the bay secure during July, August, September and October,‡ because southerly gales sometimes blow with so much strength, and the rollers sent in by them are so dangerous to ships; and having experienced the force of these gales in the vicinity of the Cape Verde Islands, he confidently warns those who are inclined to be incredulous about a gale of wind being found in 15° of north latitude; beyond the [supposed] limits of the hurricane regions.§

8. If from this inter-tropical field we extend our inquiries northward to the Canary Islands, in lat. 28°, near the African coast, we may learn of other active cyclones that have crossed these Islands, in pursuing their orbital course to the shores of northern Africa and southwestern Europe. The route of one of these storms which passed near the Island of Madeira in October 1842, as shown by Col. Reid, is seen on the Chart.‖

9. I find record of another great storm, which passed over the Island of Teneriffe, on the 6th of November, 1826.

Track XVIII, seen further westward on the Chart, is the inferred route of a severe hurricane, in 1828, which was reported to me by Capt. Corning: long known as an intelligent merchant and navigator.

These several cases, together with Col. Reid's Bermuda hurricane of Sept. 1839,¶ the track of which is seen on the Chart, and that of Capt. Maclean of Sept. 1853, of which reports are annexed, are submitted as indicating the general course of progression of inter-tropical cyclones, in the eastern Atlantic; and their occasional identity, as well as systematic conformity, with those which visit the more northern portions of this oceanic basin.

* Voyage of the Adventure and Beagle, (Surveying vessels), vol. i, pp. 1 and 3.

† This Journal, First Series, vol. xx, p. 34–38.

‡ These are the months which constitute the "hurricane season" of the Windward Islands of the West Indies, where, as we have formerly shown, the hurricanes arrive from a more eastern portion of the Atlantic. We have now, more than presumptive evidence of their African origin.

§ Voyage of Adventure and Beagle, vol. i, p. 53.

‖ See Col. Reid's Progress of the Development of the law of Storms, p. 275—279: Where is found also an account of a gale in the S. E. part of the Mediterranean.

¶ For a full account of this hurricane, see Col. Reid's Attempt to Develope the law of Storms: 2nd Edition, p. 444–448.

CAPT. MACLEAN'S HURRICANE, OF SEPTEMBER 27th, 1853.—In passing over the several violent hurricanes of the past Autumn, of which I have more copious notices, I select only the present case, because its recurvation was eastward of Bermuda. A good account of this storm is given in the London Shipping Gazette of November 8th, by Capt. Maclean, who had studied the cyclones, and was thus well prepared to meet their emergencies.

His ship, the *Gilbert Munro*, left the Island of St. Lucia on the 8th of September, and lost the trade wind on the 13th, in lat. 24° 33′ N. Light winds followed, with a high barometer, till on the 26th the weather became dark and gloomy, and the wind veered to E. S. E. and S. E. At noon, in lat. 33° 10′, lon. 59° 07′, the aneroid barometer had fallen $\frac{2}{10}$ths, and the mercurial barometer began to sink also. In the night following, the wind, at S. E., increased to a fresh gale, with squalls: [Being under the right limb of the gale, then near its point of recurvation.] At 4 A. M. of 27th the wind abated; but as the morning advanced it again freshened, from S. S. E., and the bar. had fallen $\frac{4}{10}$ths; at 10 A. M. hard gale, and bar. still falling; made the necessary preparations, being certain, from the direction of the wind, that the center was to the S. W., if a rotary storm, and would soon overtake us, in its progress northeastward, and that we should then have the gale from an opposite point.

At noon of 27th heavy gale at S. S. E., and heavy sea; lat. 35° 19′, lon. 56° 36′; rain fell in torrents till 1·30 P. M., when it ceased; barometer falling rapidly. Soon after there was a lull, and in ten minutes a full calm. Being now certain of an opposite wind, had but just time to prepare for it, when it burst upon us with increased fury from N. W., veering afterwards to N. N. W. and N. N. E. At 2 P. M. it blew a perfect hurricane, with dangerous cross sea. At 2·30 P. M. the ship was blown on her beam-ends; but with great exertions was payed off before the wind, and run admirably. It continued to blow with great violence till near midnight; when the wind backed to N. N. W., the barometer rising; and at daylight of 28th had abated to a common gale. At 8 A. M. more moderate.

Capt. M. commends a knowledge of the law of storms to every shipmaster and nautical man.

The brig *Samuel and Edward*, reports having experienced the hurricane Sept. 28th, lat. 34° 40′, lon. 56° 20′, from S. to N.; lost sails; &c., and lay ten hours under bare poles.

The Schooner *Werada* took the gale in lat. 35°, lon. 59°; and while scudding under close reefed sails, was taken aback by the hurricane from N. W.

At Bermuda, lat. 32° 15′, lon. 64° 40′, heavy rains at this period, with a very strong N. E. gale [force marked 10], from about noon of 26th to evening of 27th, veering to N.; thus showing

the left side of the cyclone. Barometer at 4·30 P. M. of 26th, 29·72. At 7·30 A. M. of 27th, 29·84.* See Track XXX of the Chart.

The foregoing notices of storms of inter-tropical origin in the eastern Atlantic, may serve to show their analogies and relations to those previously traced in the western Atlantic, and in the North American states. Let us now pass westward in the same parallels, to the nearer portions of the Pacific Ocean.

Gales of the Eastern Pacific, near the Mexican Coast.

Our direct knowledge of the paths of these gales is necessarily limited; but the interests of an increasing commerce, as well as of meteorological science, claim the notices which follow.

1. The *Joseph Butler*, on or about the 24th of June, 1850, encountered a severe gale of wind, near lat. 16° N., lon. 107° W., [260 miles from the shore of Mexico,] which carried away her mainmast. I have no further accounts of this gale.

2. The barque *Como*, on the 5th of August, 1850, in lat. 14° 20′ N., lon. 117° W., encountered a severe gale, commencing at N. and veering to W. and south. Lost sails and bulwarks, and sustained much other damage. These winds denote a course of progression corresponding to that of the hurricanes in the West Indies, and that the vessel was in the left side of the storm-path.

3. NIAGARA'S HURRICANE.—The *Niagara* was dismasted in a hurricane Sept. 9th, 1850, about ninety miles south of Acapulco: [lat. 15° 16′ N., lon. 99° 50′ W.]

The *Diana*, Sept. 11th, lat. 22° N., lon. 116° W., had a severe hurricane from N. E., veering to S. W.; blew five hours; vessel hove on beam-ends.

The Diana's position was in the left side of the storm-path, but near to the axis line; the progression of the storm being still northwesterly. Its course of progression from the *Niagara* was 34° north of west; or W. N. W., nearly. Its rate of progress was nearly twenty-three miles an hour; allowing no error for the nautical dates. Part of the track falls on our Chart. See Track XXVIII.

4. The *Laura*, Sept. 26, 1850, lat. 26° N., lon. 123° W., in a severe gale was thrown on her beam-ends; lost cargo, &c. I have no further account of this gale.

5. The *Kingston*, from San Francisco for Panama, experienced a severe gale on the Mexican coast, and was thrown on beam-ends, Oct. 1, 1850, in lat. 14° N.; and reports that the gale swept the whole coast with great violence; as may be seen in the succeeding statements.

The *Belgrade*, from San Francisco for Realejo; Oct. 1, fine breeze from W. N. W., and *heavy swell from S. E.* At 10 P. M.

* From Signal Station Reports in Bermuda Gazette.

wind hauled suddenly to S. E., with increased force and squally appearances; at midnight under single reefed topsails; 1 A. M. still increasing, with vivid lightning and heavy rain; 4 A. M. split fore-topsail; 8 A. M. lost foresail; gale increasing to a hurricane; thrown on beam-ends, with loss of main and mizen-topmasts, with head of mainmast, when the ship righted a little. At 1 P. M. Oct. 2nd, hurricane still increasing, ship on her beam-ends; lost fore-topmast, with much other damage; at midnight, blowing as hard as ever; at 4 A. M. Oct. 3d, more moderate, heavy rain; Oct. 4th, lat. 18° 11′ N., lon. 104° 5′ W., made for Acapulco. It may be seen that this vessel was on the right of the axis path of the storm.

The *Galindo*, on the same route, experienced a severe hurricane on the 1st and 2nd of October; was thrown on beam-ends and dismasted; and arrived at Acapulco at the same time with the *Belgrade*.

The *Lovina*, off Cape San Lucas, the southern point of California, Oct. 5th, was thrown on beam-ends in a violent hurricane, and lay twenty-one hours.

The *Fanny*, from Mazatlan, in the gulf of California, for San Francisco, was damaged in the gale, on the 5th and 6th of October, and put back to Mazatlan.

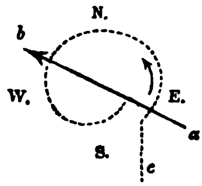
The progress of this hurricane, during four days, appears to have been N. W. by W., nearly; at a rate not exceeding eight or ten miles an hour. Part of this track falls on our Chart: Track XXIX.

6. AMAZON'S HURRICANE.—The brig *Amazon*, from New York for San Francisco, encountered a severe hurricane Oct. 3d, 1850, in lat. 13° 30′ N., lon. 116° 50′ W.; which commenced at S. W. veering successively to S. E.; E.; N.; W.; ending at S. W.; in which lost main-topsail and foresail. Capt. Watt states that the gale was equally severe as those in the West Indies. This off-shore hurricane was contemporaneous with that last noticed. The following is drawn from the account which was published by a passenger of the Amazon.

Oct. 4th, lat. 13° 40′ N., lon. 116° 30′ W.: last night the brig encountered a hurricane, preceded by squalls from S, W., with heavy rain. The squalls increased in number and intensity, until 5 P. M., when the hurricane commenced; brig under close-reefed fore-topsail and mainsail. Capt. Watts put his vessel before the blast, or "scudded" her. The tempest raged during the night, with momentarily increased fury. It veered from S. W. to due south, thence to S. E., and thence to N. E. and north, and from thence to S. W., thus making the circuit of the compass! According to our reckoning, it veered thirty-four points in the space of six hours; during which time the brig was kept before it, in which lay our only chance of escape. At 4 A. M. the

foresail was blown from the yard, and the vessel was then brought to the wind, but could not withstand the tornado, and was blown directly down on her sidé, or beam-ends. Apprehending she would founder, the order was given to put her again before the wind, but the attempt was unsuccessful. As a last resource, the main-topsail was let go, when she paid off, and dashed away like lightning before the tempest. She was kept scudding till the hurricane ceased and was then laid to in a heavy gale from S. W., which followed the hurricane.

From the above we may infer that the cousre of the vessel while scudding, was not unlike that shown in the annexed figure.



a b Course of the storm's progression.
c Course of the *Amazon* in the gale.

The short time in which the brig ran entirely round the axis of the gale, after entering its violent portion, shows that its diameter was small; and that its progression was remarkably slow. This slowness is also shown by the manner in which the brig, steering N. for San Francisco, was able to *overtake* the cyclone, and run into it, upon its southeastern side, where its wind was southwesterly. Hence, too, after clearing the vortex of the cyclone, and heaving to, the duration of the exterior portion of the gale was so much prolonged, notwithstanding the drift of the vessel was in a direction opposite to the progression of the storm. It is probable that this progression did not exceed four miles an hour; and it may have been less.

This is a slower rate of advance than I have yet found on the Atlantic; but it accords well with other cases which have occurred within the tropics, in the Indian Ocean. It appears, also, as having some relation to the slow rate of advance already noted in the cotemporaneous in-shore hurricane of the *Kingston.* Hence, we may infer, that the great current of rotation in which the cyclones are imbedded was at this period and in this region, at least, comparatively sluggish and inactive. We have noticed a similar condition in the Eastern Atlantic, in the previous month; in the case of the Cape Verde hurricane, of Track XXIII.

7. Capt. Budd's Gale, of Oct. 1851.—Capt. Budd's steamer from San Francisco, for Panama, was on the 21st of October in lat. 22° 07′, off Cape San Lucas. At daylight of 22d the wind was very high, hauling to S. E., preceded by *a heavy swell from the same quarter.* The gale blew heavy from S. E., and then commenced hauling to N. E., and blew still more heavy: barometer 29·75. He had now crossed the entrance of the Gulf of California, to within sixty miles of Cape Corientes. At 4 P. M. gale abating, and hauling to the westward, going round by the north.

The winds in this case appear to indicate that Capt. Budd fell under the right hand or northern side of the gale, as it first ap-

proached; and that the gale recurved northward, upon the contiguous portion of Mexico, before the axis of the storm had reached the position of the ship. See Chart.

8. PANAMA'S GALE, OF JULY 1852.—The *Panama*, experienced a hurricane July 16, 1852, in lat. 15° N., lon. 115° W.; which lasted ten hours: carried away top-gallant-masts, yards, sails, &c.

Extract from logbook of ship *Empire*, bound for San Francisco: July 19th, 1852, commences with heavy gales and bad sea, from the north; under double-reefed topsails and courses: [ship in front part of the gale, to the left of its axis path]. At 8 P. M. heavy gale from N. N. W.; at 10 P. M., very heavy gale; hove the ship to under triple-reefed main-topsail; midnight, gale increased to a hurricane; the mainmast went by the board, together with the mizenmast, fore-top-gallant-mast, &c., with every thing attached; blowing a complete typhoon. At 5 A. M., succeeded in clearing the wreck; at 7 A. M. gale had in some measure abated, and at 8 A. M. got the ship before the wind, then blowing from S. S. W.; at noon of 20th, only a brisk gale from S. S. E. Lat. by account, 17° 4′ N., lon. 117° 35′ W.

This could have been none other than the Panama's gale, moving on a course between 30° and 40° north of west; and, if there be no error in the Panama's date, at the rate only of about three miles and a half per hour!

This slowness of progression, in the three hurricanes of the *Panama*, *Amazon*, and *Kingston*, is of great interest for navigators in the Pacific. For it shows how perfectly the exposure and safety of their vessels, during such hurricanes, are placed in their own control; at least, in cases where sea room on all sides is afforded them. Thus, if the master of the *Amazon* had comprehended the character of his hurricane, or its law of rotation and progression, he might have run more eastward until the state of the barometer and winds would have allowed him to come up to his desired course. This would have enabled him to make a safe, rapid, and successful run, towards his port of destination, while he kept in the outskirts of the gale.

The *Empire*, when headed off by the north wind in the front of the gale, could not pursue her course for San Francisco, nor safely heave to, on either tack. But she had opportunity to run southward in the beginning of the gale, keeping the wind on the starboard quarter, until the state of the barometer and the diminished strength and westerly changes of the wind should enable her to turn eastward, around the rear of the hurricane, and thus regain her course with a fair wind.

9. A violent hurricane occurred at Cape Corientes and Ipala on the night of October 11th 1853; in which the *Eclipse*, a valuable ship, was totally lost, about five miles east of Ipala: [in lat.

20° 10′ N., lon. 105° 25′ W.] It first blew off the land, from the northward, and shifting suddenly to the westward, blew a perfect hurricane, right on shore. This may indicate its recurvation near the southern entrance of the Gulf of California at Cape Corientes. It has been shown that some hurricanes of the gulf of Mexico, commence their recurvated course to the northward and eastward in a still lower latitude. For such a case, see this Journal, vol. i, New Series, p. 153–162.

The inter-tropical gales of the North Pacific which are comprised in these few notices, are seen to have occurred in the several months from June to October, both inclusive; and I have now before me an account of another violent gale, far to the westward, in the month of May. The prevalence of storms on that coast in the other months, from October to April, has been noticed by Humboldt and other writers; and is now but too well known by the experience of navigators.

We thus establish the prevalence of violent cyclones upon the southwestern coast of North America at all seasons of the year: and find that these are sometimes of great violence. That many of these cyclones pass over the Mexican territories, some to the gulf of Mexico, under the local name of *northers*, and others to the territory of the United States, I can find no reason to doubt.

The very prominent characteristic of southeast winds, in the storms which commonly visit the Pacific coast, affords evidence of their progress along the coast in the lower latitudes, and of their direct entrance upon those shores in higher latitudes, subsequent to their northwardly recurvation. These characteristics early attracted my attention, in the gales which are noticed in the voyages of Cook, Vancouver, and others, and in the Journals of whalers, which came under my inspection.

We might infer, therefore, without reference to other and direct evidence, that the same general system of cyclonic movement prevails on the continent of North America that is found on the Atlantic. Indeed, a glance at our storm Chart might afford conviction of this fact.*

A competent knowledge of the cyclones and of the law which governs their development, has become essential to our navigators. Both merchants and insurers are beginning to discover that even the good qualities of a vessel have commonly less influence upon the safety of her voyage, than has the intelligence and skill of the commander. Hence, there are now insurers who freely select those risks which are in charge of the most compe-

* In almost every region of the Pacific, violent cyclones are known to occur: and even within five or six degrees of the equator, the ravages of a hurricane at certain islands have occasioned the destruction of a large portion of the native population.

The results of the recent inquiries have now shown, by direct observations, the prevalence of the cyclonic system of storms entirely around the globe, in both hemispheres; excepting some interior or inaccessible portions of the old continent.

tent masters; leaving other risks of whatever class, to underwriters who are willing to rely on the classification of the vessels.

American Storms of December 1836.

From the 30th day of November to 21st December, 1836, six great cyclones passed successively over the United States; having passed New York on Nov. 30th,—Dec. 5th,—10th,—14th,—17th, and 21st, respectively: under which, my barometer fell ·62,—·35,—·44,—·86,—·90, and 1·05 in., in the several cases.* The surrounding waves of exterior pressure raised by their rotation, and separating each cyclone from the other, were indicated by my barometer as follows, viz.: Nov. 28th, 30·27; Dec. 4th, 30·29;—8th, 30·35;—12th, 30·28;—16th, 30·45;—19th, 30·80; and Dec. 22nd, 30·72 inches. Each cyclone exhibited here the winds of its two right quadrants, gradually veering, from a southern quarter to the western board, as it went onward; thus showing the cyclonic centers to have passed far westward of New York, and over the Canadas, in their several routes to the northern regions of the Atlantic.

In the last of these storms, which has been examined by Prof. Loomis,† the wind at New Orleans, on the 20th, blew hard from a southern quarter, and also on our Atlantic coast, during the latter part of 20th and early part of 21st; veering westward. At Rochester, N. Y., it blew from southeast on the afternoon of 20th, with great power, and furiously at Buffalo, also veering round by the south to the west, during the night; thus showing that the axis of this gale passed northwardly at a distance much to the west of these places. This fact is confirmed, also, by the reports of winds as made to the Regents at Albany, and by those obtained from the military posts and other sources; very many of which are given by Prof. Loomis. The same fact is shown by barometric observations as published by him. For although the central nucleus of the storm, or area of greatest barometric depression, passed the western observers during the night, when the greatest and most rapid fall and rise of the barometer was not noted, yet, the depression as recorded proves to be greatest as we go towards the true center-path of the storm, as the same is approximately indicated on the Chart: marked XXVII. This is seen in the observations made at Lexington, (K.) Springfield, (O.) Marietta, Twinsburg, Rochester, Syracuse, Albany, Montreal, Hanover, and Quebec; which, even as given, show a mean fall of 1·075 in.: while those of twelve places on or near the Atlantic border, from Savannah to Newfoundland, show a mean fall of

* This series may serve to illustrate the continued succession of cyclones in the United States.

† Transactions of the American Philosophical Society, vol. vii, New Series, p. 125–164.

but ·91 in. If the true course of the storm had been from west to east, the fall in the barometer would have been much the greatest on the Atlantic border; owing to the lower level, which is not considered and allowed for in the above estimate, and to a less obstructed rotation of the storm, on reaching the Atlantic.*

Moreover, the barometric minimum was observed at Quebec about as early, on the 21st, as at New York and its vicinity; although 420 miles further to the north, and nearly on the same meridian. This more rapid advance of the central portion of the storm, which has been seen in other cases, proves that the true course of progression was on the general route which I have indicated. The rate of the storm's progression, from noon of 20th to noon of 21st, I estimate at about 33 miles an hour.

It is true, however, that Prof. Loomis has traced this storm *eastward*, from the Mississippi to the Atlantic; and has stated, also, that in this case "there was no whirlwind." But, not rejecting his claims as a cyclonologist, I may state that he had almost no observations other than from the right side of the storm's center-path. Now, in like limited manner, and *with like directions and changes of the wind*, the great hurricane of September last, which we have just considered, may be traced eastward from its center-path, for a greater distance, and in the same latitudes, as already shown. The like is also true of the great Cuba hurricane of 1844, which was examined in this Journal;† as also, of Col. Reid's hurricane which crossed Bermuda and Newfoundland, in Sept. 1839, (see Chart,) on perhaps the most northerly course that has been traced in any storm.

Yet, who that duly examines these cases, will doubt that these storms, in their essential character, were great whirlwinds, moving northward and eastward? Indeed, the same or like phenomena may be traced in every great cyclone that passes over these latitudes. This eastward extension appears due, in part, to the enlargement of the cyclone; and while affecting its external form, and that of the lines of equal pressure, it does not essentially change the rotative movement; as may be seen by the continued development of the cyclonic winds, and their influence on the barometer.

It is well known that other and similar tracings from west to east have been made of the progress of various storms in the

* The extreme range of the barometer in a period of seven years at Hudson, O., near to Twinsburg, and about 1100 feet above tide, as given by Prof. Loomis, is 1·719 in.: while the range observed in New York during the same period, was 2·25 in. Difference, ·531. The mean of the annual ranges at Hudson during the same period appear to have been 1·402 in: while the mean of the annual ranges at New York was 1·874 in. Difference, ·472 in. It appears, therefore, that near half an inch should be added to the depression of the barometer in some of the western observations of this storm, in order to a fair comparison of the barometric indications with those on the Atlantic border in the same storm.

† This Journal, vols. i, and ii, New Series, 1846.

United States. It is believed, however, that the clew to these cases is already afforded; and that many or most of these storms were true cyclones; with orbital courses really analogous to those which are seen on the Chart.

What are Cyclones?—The term Cyclone was first proposed by Mr. Piddington, to designate any considerable extent or area of wind which exhibits *a turning or revolving motion;* without regard to its varying velocity, or to the different names which are often applied to such winds. If used in this sense it may prevent the confusion which often results from other names, more variable or indeterminate in their signification. Thus, all hurricanes or violent storms may perhaps be considered as cyclones or revolving winds. But it by no means follows that all cyclones are either hurricanes, gales, or storms. For the word is not designed to express the degree of activity or force, which may be manifested in the moving disk or stratum of rotating atmosphere to which it is applied. It often designates light and feeble winds, as well as those which are strong and violent.*

It follows that the local directions and changes of the wind in any cyclone, and their effect on the barometer, are much like those exhibited in the gales and storms of the same region, except in the *degree* of their effect; which is doubtless proportioned to the general activity of the rotation, integrally considered.

The cyclones are often productive of rain in a portion of the cyclonic area; but vary in this respect, in different regions, and at different seasons of the year.

Universality of Cyclones.—As early as 1833 my inquiries led me to announce the conclusion that the ordinary routine of the winds and weather in these latitudes often corresponds to the phases which are exhibited in the revolving storms, already de-

* As regards the temperate latitudes of the northern hemisphere, the true normal wind is commonly from the southwestern quarter of the horizon; and the accession of a cyclone, except on its right margin, is usually marked by a change of the wind from the western board to some point on the eastern side of the meridian, accompanied and often preceded by a fall of the barometer. On the right margin of its path the cyclone may commence from near the southwest, in perfect continuance with the normal wind with which it here coincides. As the cyclone advances, the wind on the right of its axis-path *veers* "with the sun," or from the east towards the south and west; while on the left side of this path or line, the same cyclonic wind changes from the east to the *north*. On and near the axis-path the earlier winds of the cyclone blow *across* the line of progress; from the southeastern quarter to the northwestern, with a falling barometer; and when the axis of the cyclone has passed, its later winds are found crossing the line of progress in the opposite direction, from the northwestern quarter to the southeastern, with a rising barometer.

The true cyclonic wind may not always be found at the earth's surface, in every portion of the path of the cyclone, if its action be feeble, or subject to interruption, or to the interposition of bordering winds or cyclones. Even in stormy cyclones, irregularities of direction are often noticed at the surface; but in these cases it commonly happens that the *storm-scud*, at the elevation of a few hundred feet, exhibits locally the true direction of the cyclonic wind. But the changes of direction successively observed in the storm-scud, are commonly *in advance* of those in the lowest wind.

scribed, and that a correct opinion, founded upon this resemblance, can often be formed of the approaching changes: and that the variations of the barometer resulting from the mechanical action of circuitous winds and the larger atmospheric eddies, pertain not only to the storms, but to a large portion of the winds in these and the higher latitudes. *Vide* this Journal for October, 1833, (vol. xxv,) pages 120 and 129.

The more inert and passive cyclones which seldom gain attention, but which constantly occupy in their transit the greater portion of the earth's surface, appear to move in orbits or courses corresponding with those of the more active class which have been traced on the storm-charts; a result that will not be doubted by those who have given careful attention to this branch of inquiry. In a broad view of the case, the constant occurrence and progression of the cyclones, in various degrees of activity, constitutes the normal condition of the inferior or wind-stratum of the atmosphere, at least in the regions exterior to the trade winds of the globe; to say nothing of their prevalence in the intermediate region, where their presence is shown on some occasions by the most indubitable evidence.

At the late meeting of the American Association for the Advancement of Science, held at Cleveland, an ably elaborated paper was presented by Prof. James H. Coffin, of Easton, Pa., on the relations which exist between the direction of the wind and the rise and fall of the barometer. By a careful analysis of these effects during all seasons of the year, as observed at various places in the north temperate zone, Prof. Coffin establishes the northeastwardly progression and leftwise rotation ↺ of a continued series of cyclones, in which are developed the same local relations between the rotary action of the various winds and the movements of the barometer that are found in the several rotary storms and hurricanes which have been subject to investigation; and such in kind, though not in degree, as we have seen in the principal case already considered. Thus, if I rightly appreciate the labors of Prof. Coffin, the cyclonic character of the variable winds in the temperate latitudes, which had been inferred from special observations and an extended range of geographical inquiry, is now established by a different and wholly independent method of investigation.

The storm-paths and routes of the cyclones clearly indicate, also, the true course of the principal circulation in the lower atmosphere, on both sides of the equator. An enlarged view of these physical truths and conditions may serve to convince meteorologists and others of the necessity for a thorough revision and correction of the received views of dynamical meteorology. Such revision, I apprehend, is now imperatively required. For the constant recession from the equator of a great portion of the

lowest currents of atmosphere, as seen in the orbital courses of storms in all latitudes, and to which I have already alluded, together with the mean direction of the observed winds in the northern temperate zone, even neglecting other world-wide phenomena, may suffice to show, that the current theory or hypothesis for explaining the general winds of the globe, is essentially erroneous and defective in its application, and greatly obstructs the path of scientific inquiry.

New York, March 18, 1854.

ART. XXIII.—*Researches upon Arseniuretted and Antimoniuretted Hydrogen, and their relations to Toxicology;* by RAPHAEL NAPOLI, Royal Professor of Chemistry at Naples.

(Read before the American Association for the Advancement of Science, at Washington, May, 1854, by T. S. Hunt, for the author.)

AFTER Lassaigne had observed that nitrate of silver decomposes arseniuretted hydrogen with the formation of arsenious acid, and the separation of metallic silver, Jacquelain proposed the chlorid of gold for the same object, and Berzelius in his *Traité de Chimie* says of this gas, "It precipitates the precious metals, as gold and silver, from their solutions, and is itself dissolved by the oxydation of its elements." Such a decomposition really takes place with arseniuretted hydrogen and the ter-chlorid of gold, and also with the ferric and platinic chlorids; still no chemist, so far as I know, has explained the reaction.

The explanation which I now propose was suggested to me by M. Nicolo Prestandrea of Messina, who wrote to me as follows; —"When arseniuretted hydrogen is passed through a solution of chlorid of gold, we see that the gold is really reduced, and the arsenic dissolved as arsenious or arsenic acid. But as these bodies contain no oxygen, whence comes this element to oxydize the arsenic, unless from the decomposition of the water of the solution, whose hydrogen at the same time forms hydrochloric acid, with the chlorine of the gold salt? This acid might be formed from the union of this chlorine with the hydrogen of the gaseous arseniuret, without any decomposition of water, in which case both gold and arsenic, should be separated in the metallic state, according to the following equation,

$$AuCl_3 + AsH_3 = 3HCl + Au + As.$$

So that the theory of this reaction is not yet made clear. In order to explain the facts just mentioned we must suppose that arsenic, in its nascent state at least, can be dissolved by hydrochloric acid; such being the case, it would be easy to understand the formation of the acids of arsenic, the precipitation of the gold,

and the production of hydrochloric acid. The gold having been reduced, according to the formula just given, there remains 3HCl, and As, which would yield $AsCl_3$, and three equivalents of free hydrogen. The chlorid of arsenic when diluted with a large quantity of water, is decomposed into arsenious and hydrochloric acids."

On consulting Berzelius and other works of authority, I found it stated on the one hand, that arseniuretted hydrogen is not altered by hydrochloric acid, and on the other, that arsenic is not affected by hydrochloric acid. These statements seemed to render the proposed explanation inadmissible, but I have found by experiment that they are incorrect, and have arrived at the following conclusions, first, arseniuretted hydrogen is almost totally decomposed by pure concentrated hydrochloric acid, and secondly, arsenic itself is soluble in this acid.

I passed the arseniuretted hydrogen gas generated in Marsh's apparatus, through concentrated hydrochloric acid in a Liebig's bulb tube, and after continuing the process for an hour, chlorid of arsenic was found dissolved in the acid, thus proving the decomposition of the arseniuretted hydrogen.* To prove the second point I took some crystallized metallic arsenic, washed it repeatedly with cold and pure hydrochloric acid, and when its surface was perfectly free from oxyd, attacked it with boiling hydrochloric acid in a small retort, placing a little water in the receiver. On collecting the portion which distilled over, I found it to contain chlorid of arsenic, and the liquid residuum in the retort, contained a notable portion of the same chlorid; thus showing the solubility of arsenic in hydrochloric acid.

These facts, being well established by a series of repeated experiments, gave me an explanation of the reactions of arseniuretted hydrogen on the perchlorids of gold and iron, and the bichlorid of platinum; the formulæ are as follows:

$$AuCl_3+AsH_3=Au+AsCl_3+H_3.$$
$$3Fe_2Cl_3+AsH_3=6FeCl+AsCl_3+H_3.$$
$$3PtCl_2+2AsH_3=Pt_3+2AsCl_3+H_6.$$

While studying the above reactions, I had occasion to repeat some observations which were made long since by Stromeyer, and appear to have been forgotten by chemists, but which serve to render more accurate the examinations for arsenic and antimony by Marsh's apparatus. The facts are these: arseniuretted and antimoniuretted hydrogen are both decomposed by pure and

* The decomposition of arseniuretted hydrogen with hydrochloric acid, is represented by $AsH_3+3HCl=AsCl_3+3H_2$, and is analogous to that of hydrid of copper with the same acid; in each case a metallic chlorid is formed, and the hydrogen of both compounds is set free. See Brodie's remarks on the latter reaction, in the Chemical Gazette for 1853, p. 300.—(T. S. H.)

highly concentrated nitric acid, the former yielding water, nitrous vapor, and soluble arsenic acid, while the latter gives insoluble antimonic acid, causing a turbidness in the liquid, which is increased by concentration. If then we pass a mixture of these two gases into nitric acid, and afterwards boil the turbid solution, a yellowish-white precipitate of antimonic acid separates, and by adding water and continuing the ebullition, the antimony is entirely thrown down, while the arsenic acid remains dissolved.

The two gases are also decomposed by aqua-regia, with the formation of chlorid of arsenic, and perchlorid of antimony. By a careful distillation of the mixture, the arsenical chlorid passes over first, while the chlorid of antimony remains in the retort, as has been shown by Malaguti and Sarzan. An analogous reaction is produced with strong hydrochloric acid, but with this difference that the arseniuretted hydrogen is almost entirely decomposed, while the antimonial gas undergoes a less complete decomposition.

The applicability of these reactions to the concentration and separation of the arsenic and antimony, in the gas obtained by Marsh's apparatus, will now be apparent. Instead of burning or decomposing by heat, the evolved gas, it is passed through a U tube to dry it, and then by means of a caoutchouc connector into a Liebig's bulb apparatus, containing concentrated, fuming nitric acid, which is to be heated by immersion in a bath of water or oil. Having assured ourselves of the purity of the gas evolved by the zinc and sulphuric acid, we add the suspected matter, and then pass the gas into the hot nitric acid, which completely decomposes in the manner just described, any compounds of arsenic or antimony which may be evolved. If the nitric acid remains clear, we are almost sure of the absence of antimony. When the operation is finished, the contents of the bulbs are to be transferred to a small flask, the apparatus washed out with a little nitric acid, and the whole carefully evaporated to one-half. If there is no precipitate, the absence of antimony is certain; we then evaporate still further to remove the excess of acid, dilute the residue with water and examine it like a pure solution of arsenic acid. Should the nitric acid appear turbid either before or after evaporation, antimony is present, and perhaps arsenic; in this case, after evaporating as before to a small bulk, we add water and filter, the antimony remains behind in an insoluble condition, while the arsenic, if any were present, is held in solution, and both can be examined by the ordinary tests.

In the case of a mixture of arsenic and antimony we may proceed in a different manner for their separation. Having prepared a small tubulated retort, to which can be adapted a small receiver, we introduce through the tubulure, a tube reaching nearly to the bottom of the retort, in which is placed a small quantity

of aqua-regia composed of two parts of hydrochloric and one of nitric acid. The retort being gently heated, the gases from Marsh's apparatus are passed in a slow current into the aqua-regia. In the reaction which takes place, any arseniuretted or antimoniuretted hydrogen will be decomposed, and chlorid of arsenic or antimony formed. This operation finished, the tube is removed, the tubulure closed, and the receiver, partly filled with water, being attached, the acid liquid in the retort is gently distilled to one-half its volume. We then examine the water of the recipient, and if any arsenic is present, it will be found in the distilled liquid; if this metal be absent, we find nothing in the water, or at most, some traces of antimony, in case the operation has not been well conducted. If the gas contained any antimony it will all be found in the retort, in the state of perchlorid.

I have not deemed it necessary to present the numerical results, which in repeated experiments have shown the great accuracy of these methods, but believing that chemists will at once recognize the value of the proposed processes, it is sufficient for me to have called their attention to the following facts, in part already known, and in part new.

1st. The power of hydrochloric acid to dissolve and decompose arseniuretted hydrogen.

2d. The solubility of metallic arsenic in the same acid, when concentrated.

3d. The explanation of the reactions of arseniuretted hydrogen, with the perchlorids of gold and iron, and with the bichlorid of platinum.

4th. The decomposition of arseniuretted and antimoniuretted hydrogen by nitric acid, and by aqua-regia.

5th. The application of these reactions to toxicological analysis, for the detection and separation of arsenic and antimony.

Art. XXIV.—*On some of the Crystalline Limestones of North America;* by T. S. Hunt, of the Geological Commission of Canada.

(An Abstract of a paper read before the American Association for the Advancement of Science, at Washington, April, 1854.)

The crystalline limestones of Canada, with those of New York and the New England States, may be divided into four classes, belonging to as many different geological periods. The first and most ancient occur in that system of rocks, named by Mr. Logan the Laurentian series, which extending from Labrador to Lake Huron, forms the northern boundary of the Silurian system of Canada and the United States. The lowest beds of the Silurian

repose horizontally upon the disturbed strata of this oldest American system, a southern prolongation of which crosses the Otaway near Bytown, and the St. Lawrence at the Thousand Isles, and spreading out, forms the mountainous region of northern New York. This series consists in large part of a gneiss, which is often garnetiferous; but beds of mica slate, quartz and garnet rock, hornblende slate and hornblendic gneiss are also met with, besides large masses of a coarsely crystalline, often porphyritic rock, consisting chiefly of a lime and soda feldspar, which is sometimes labradorite, and at others andesine, or some related species, and is generally associated with hypersthene. It often holds beds or masses of titaniferous iron ore, and from its extent, occupies a conspicuous place in the series. It is the *hypersthene rock* of McCulloch and Emmons.

With these, the limestones are interstratified, but their relations to the formation have not yet been fully made out. All of these rocks bear evidences in their structure, that they are of sedimentary origin, and are really stratified deposits, but their investigation is rendered difficult by the greatly disturbed state of the whole formation. Among these stratified rocks, there are however dykes, veins, and masses of trap, granite and syenite, often of considerable extent, which are undoubtedly intrusive. There are abundant evidences that the agencies which have given to the strata, their present crystalline condition, have been such as to render the limestone almost liquid, and to subject it at the same time to great pressure, so that in many cases it has flowed around and among the broken and often distorted fragments of the accompanying silicious strata, as if it had been an injected hypogene rock.

The limestone strata are from two or three feet to several hundred feet in thickness, and often present a succession of thin beds, divided by feldspathic or silicious layers, the latter being sometimes a conglomerate of quartz pebbles and silicious sand; in one instance, similar pebbles are contained in a base of dolomite. Beds frequently occur in which the carbonate of lime has been mixed with silicious sand, in some cases yielding an arenaceous limestone, while in others, a chemical union has produced beds of tabular spar, often passing into pyroxene from an admixture of magnesia. These minerals sometimes form beds, in a nearly pure state, but in other cases they are intermixed with quartz, carbonate of lime, orthoclase, scapolite, sphene and other species.

The limestones are sometimes coarsely crystalline, at others finely granular or almost compact; their color is white passing into reddish, bluish, and grayish tints, which are often arranged in bands coincident with the stratification. Some of the dark grey bands, harder than the adjacent white limestone, were found by Mr. Murray to owe their color to very finely disseminated

plumbago, and their hardness to intermingled grains of rounded silicious sand. The limestone is often magnesian, and the manner in which the beds of dolomite are interstratified with the pure limestone, is such as to lead us to suppose that some of the original sedimentary deposits contained the two carbonates, and that the dolomite is not the result of any subsequent process.

The principal mineral species found in these limestones are apatite, serpentine, phlogopite, scapolite, orthoclase, pyroxene, wollastonite, idocrase, garnet, brown tourmaline, chondrodite, spinel, corundum, zircon, sphene and graphite. All of these appear to belong to the stratification, and the chondrodite and graphite especially, are seen running in bands parallel to the bedding. Magnetic iron ore is sometimes found in beds interstratified with the limestone. The apatite which is in general sparingly distributed, is occasionally very abundant in imperfect crystals and irregular crystalline masses, giving to small beds of the limestone the aspect of a conglomerate. Some of the coarsely crystalline varieties of this limestone give a very fetid odor when bruised.

In some parts of this formation, in the rear of the bay of Quinté, the rocks are less altered than in most other places, and here the limestones, although more or less crystalline in texture, afford none of the fine crystallized species elsewhere met with. The foreign ingredients seem to be mechanically intermixed, giving an earthy appearance to the weathered surface of the rock, or are separated in the form of small grains of pyroxene, showing an imperfect metamorphism. For further descriptions of the rocks of this series, see the Reports of the Geological Survey of Canada, particularly that of Mr. Logan for 1846, and Mr. Murray's for 1853; also Dr. Emmons's Report on the Northern District of New York. In position and in lithological characters, the Laurentian series appears to correspond to the old gneiss formation of Lapland, Finland and Scandinavia.

In the second class we include the crystalline limestones of western New England, and their continuation in southeastern New York, and the adjacent parts of New Jersey and Pennsylvania. The limestones of the Champlain division of the Lower Silurian rocks which are found on the Yamaska River, enter Vermont near Misisquoi Bay, where they show a commencement of alteration. Farther south, they become the white granular marbles of western Vermont, and of Berkshire, Massachusetts, which according to Hall, still exhibit upon their weathered surfaces, the fossils of the Trenton limestone; thence passing southwest, they cross the Hudson near West Point, and appear in Orange and Rockland counties, New York, and in Sussex county, New Jersey, in a highly altered condition, closely resembling the crystalline limestones of the Laurentian series, and containing in great abundance the same imbedded minerals. These limestones are some-

times dolomitic, and Hitchcock observes that in the granular marbles of Berkshire, pure and magnesian limestones occasionally form different layers in the same bed. (Geology of Massachusetts, p. 84.)

In Orange county, according to Mather, it is easy to trace the transition from the unaltered blue and gray fossiliferous limestones of the Champlain division, (including the Calciferous sandrock and the Trenton,) to the highly crystalline white limestone with its characteristic minerals. (See his Report on the Geology of the first district of New York, pp. 465 and 486.) This view is fully sustained by H. D. Rogers in his description of the limestones of Sussex Co., given in his final report on New Jersey, (cited by Mather as above, p. 468 et seq.) Mather farther concludes very justly that all the limestones of western Vermont, Massachusetts and Connecticut, and those between the latter state and the Hudson River, are in like manner altered Lower Silurian strata. (p. 464.) From the similarity of mineral characters, he moreover supposes that the crystalline limestones about Lake George are of the same age, and he extends this view to those of St. Lawrence County. Both of these however belong to the Laurentian series, and are distinguished by their want of conformity with the Champlain division, and by their association with labradorite and hypersthene rocks which seem to be wanting in the altered Silurian strata. The slates of this division in Eastern Canada, generally contain some magnesia, with very little lime; and four or five per cent. of alkalies, chiefly potash;* hence the feldspar which has resulted from their metamorphosis is generally orthoclase, and they have yielded gneiss, and mica slate, which with quartz rock, and chloritic and talcose slates, make up the Green Mountains.

In the upper part of the Champlain division, there are found some beds of a limestone, often conglomerate, which is generally magnesian and ferruginous, and often contains a great deal of silicious sand; and associated with it are beds of carbonate of magnesia without a trace of lime, though sometimes very silicious. These beds are interstratified with slates and sandstones, and in the metamorphic region are replaced by the serpentines, which are often intermixed or associated with limestones and dolomites, and, with their accompanying talcose slates, may be traced one hundred and thirty-five miles in Canada, and thence by Vermont, Massachusetts and Connecticut, through New York, New Jersey, Pennsylvania and Maryland, southward. These rocks are everywhere marked by the occurrence of chromic iron ore, in masses running with the stratification, or in disseminated grains, in the serpentine, and sometimes in the dolomite; they are also the

* See my remarks On the Composition and Metamorphoses of some Sedimentary Rocks, L. E. and D. Philos. Magazine for April, 1854, p. 233.

auriferous rocks of the great Appalachian chain. Gold, associated with talcose slates, serpentine, chromic and titaniferous iron ores, is traceable along their outcrop from Canada to Georgia. Gold-bearing veins have also been found in the slates which in Eastern Canada, form the base of the Upper Silurian. I remark that in a somewhat chloritic and very silicious magnesian limestone, which is associated at Granby with red and green slates and sandstones, a portion of oxyd of chromium was detected by analysis. I have also found titanium in some of the very ferruginous slates, which by their alteration become chloritic schists holding magnetic and specular iron, ilmenite and rutile.

Serpentine is found as an imbedded mineral in the Laurentian limestones, but the extensive deposits of serpentine rock, with its associated talcose slates and chromic iron, appear to be confined to the upper part of the altered Champlain division. The examinations of C. U. Shepard, and those subsequent of J. Lawrence Smith and G. J. Brush, have shown that many at least of the so-called serpentine rocks of northern New York, are hydrous silicates of alumina, iron, and potash, containing very little lime or magnesia; they are the dysyntribite of Shepard.

As the northwestern limit of the metamorphic belt in Eastern Canada runs southwesterly into Vermont, the undulations of the strata, which are nearly N. and S., escape from it to the northward. Proceeding E. S. E. however, from the unaltered Trenton limestones of the Yamaska, we cross the overlying slates, sandstones and dolomites, and entering the metamorphic region find the serpentines, talcose, chloritic and micaceous schist, with gneiss and quartzite, very much disturbed, and repeated by undulations. On reaching the valley of Lake Memphramagog, we come upon the third class of crystalline limestones, which are Upper Silurian. This limestone formation has a continuous outcrop from the Connecticut valley, by the lake just mentioned and the upper part of the St. Francis river, to the Chaudière, and is thence traceable by intervals as far as Gaspé, where it is clearly unconformable with the Lower Silurian. It holds the characteristic fossils of the Niagara group, but for some distance from the line of Vermont, is so much altered as to be white and crystalline, and to contain abundance of brownish mica, the fossils being often obliterated. At Dudswell on the St. Francis, the beds of white granular marble show upon their weathered surfaces or in polished sections, the forms of encrinal discs and corals, among which the characteristic *Favosites gothlandica*, and various species of *Porites* and *Cyathophyllum*, have been identified. These fossils in a similar condition are also found at Georgeville on Lake Memphramagog. Following the section in a S. E. direction, to Canaan on the Connecticut river, we meet with calcareo-micaceous schists, which are gradually replaced by mica slates with quartzose beds.

Some of the fine dark-colored mica-slates exhibit crystals of chiastolite, and others near Canaan, abound with black hornblende and small garnets. (For the details of this section see Mr. Logan's Report for 1847–48.)*

These Upper Silurian strata constitute the micaceo-calcareous rocks of Vermont, which Prof. Adams traced through the state, to Halifax on the border of Massachusetts, and they are continued in what Hitchcock has called the micaceous limestones of this state, which according to him pass by insensible degrees into mica slate. The limestones of Coleraine, Ashfield, Deerfield and Whately, Mass., belong to this formation, and perhaps also the crystalline limestone which is found at Bernardston, with magnetic iron and quartz rock, and shows imperfect fossils upon its weathered surfaces. (Hitchcock's Geol. of Mass. p. 560.) The condition of these limestones resembles that of the granular marbles on the other side of the Green Mountains, and they nowhere exhibit that degree of alteration which distinguishes the latter farther south. The same calcareo-micaceous rocks are conspicuous in western Connecticut; but in the towns of Salisbury, Sharon and Canaan the crystalline limestones, and in Litchfield and Winchester, the serpentines, of the Lower Silurian are met with, and these rocks appear again in the southwestern part of the state.

In the fourth class we include the crystalline limestone of eastern Massachusetts, which occurs in a great number of places in the towns of Bolton, Boxborough, Chelmsford, Carlisle, Littleton, Acton, Natick and Sherburne. It appears according to Hitchcock, in interrupted lenticular masses, lying in the gneissoid formation, or in the hornblendic slates, and occasionally presenting distinct marks of stratification. Still farther east at Stoneham and Newbury, we find crystalline limestone, sometimes magnesian, in irregular masses, lying in a rock intermediate between syenite and hornblende slate. Serpentine is found with that of Newbury; and at Lynnfield, a band of serpentine has been traced two or three miles N. E. and S. W. Dr. Hitchcock, to whose report on the Geology of Massachusetts we are indebted for the present details, says of this serpentine, "I am satisfied that it is embraced in the great gneiss formation, whose strata run from N. E. to S. W. across the state." p. 159. He further remarks of the syenite of Newbury and Stoneham, which includes the crystalline limestones, "I have every reason to believe that it is only a portion of a gneiss formation which has undergone fusion to a great degree; for portions of the rock still retain a slaty or stratified structure," and he conceives it probable that all the crystalline limestones of Massachusetts are of sedimentary origin; p. 586. It may be remarked that the irregular shape of these interstratified

* See also on the Geology of Canada, this Journal [2] vol. ix, p. 12, and xiv, p. 224.

masses, is analogous to the interrupted stratification and lenticular beds, frequently met with in fossiliferous limestones.

The limestones of Bolton, Chelmsford and the adjoining towns, are in general highly crystalline, and are remarkable for the variety of fine crystallized minerals which they contain. Among these are apatite, serpentine, amianthus, talc, scapolite, pyroxene, petalite, chondrodite, spinel, cinnamon-stone, sphene and allanite, which include the species characterizing the Laurentian and Lower Silurian metamorphic limestones. The limestone of these quarries evolves a very fetid odor when bruised. Chromic iron ore has never, so far as I am aware, been observed with the serpentines of this region.

We have now to inquire as to the geological age of this great mass of crystalline rocks which is so conspicuous in Eastern New England. Mr. Logan has shown that the rocks of the Devonian System in Gaspé, assuming the Oriskany sandstone as its base, attain a thickness of more than 7000 feet, and as they are still 2500 feet thick in New York, and do not die away before reaching the Mississippi, it is to be expected "that they would follow the Upper Silurian zone in its southwestern course from the eastern extremity of Gaspé, and display a conspicuous figure either in a metamorphic or unaltered condition, between it and the carboniferous areas of Eastern America; to one of which New Brunswick belongs, while another is met with in the state of Rhode Island, and in a metamorphic condition in Massachusetts." (Report for 1848, p. 58.) The lower part of the Devonian, farther west embraces beds of limestone, but in Gaspé the formation consists almost entirely of siliceous and argillaceous beds; in Mr. Logan's section of the whole 7000 feet on the Gulf of the St. Lawrence, he observed only one small bed of limestone, and a few thin bands of limestone conglomerate. When we consider the geographical position of the Upper Silurian rocks in the Connecticut valley on the one hand, and the coal field of southeastern Massachusetts on the other, we can scarcely doubt that the intermediate gneissoid, and hornblendic rocks, with their accompanying limestones, are the Devonian strata in an altered condition. Prof. Agassiz, from his own examination of the region, was led to a similar conclusion as to the age of the so-called syenites, and in August, 1850, presented to the American Association for the Advancement of Science at New Haven, a paper on the Age of the Metamorphic rocks of Eastern Massachusetts, which has never I believe been published. The less altered limestones which, according to Dr. Hitchcock are found interstratified with red slates at Attleborough and Walpole, may correspond to those which with similar slates and sandstone, are met with at the base of the carboniferous formation in Canada on the Bay de Chaleurs, and in New Brunswick.

We have then distinguished four classes of crystalline limestones: first, those of the Laurentian series with their accompanying garnetiferous gneiss, labradorite and hypersthene rocks; secondly, those of the Lower Silurian formation, with their attendant auriferous rocks, talcose slates and chromiferous serpentines; thirdly, those of the Upper Silurian age, with their associated calcareo-micaceous schists; and fourthly, those which belong to the gneissoid rocks of eastern Massachusetts, and are probably of the Devonian period.

I have endeavored in this paper to bring together the facts known with regard to the different crystalline limestones, and their associated strata in this portion of the continent, and to show how far these may serve as a guide in the geological investigation of the metamorphic rocks. While the result confirms the observations of European Geologists, that similar crystallized minerals may occur in the metamorphic limestones of very different geological epochs; it also shows, that within certain limits, the mineral characters of the altered silicious strata, may serve as important guides to our investigations.

Art. XXV.—*Documentary Publications and Science in the Coast Survey Report for* 1853.*

Congressional printing is no longer beneath criticism. It had reached such a depth of degradation in respect to paper, type, proof-reading, press-work, binding and unpunctual delivery, that not even the long suffering of Congress could further endure its vexations and malpractices, however serviceable these might be thought to a party or pet contractor. From Faustus to Little & Brown's last imprint, typography could scarcely show worse specimens than some of the Congressional contract documents, from the Mexican war dispatches to the reorganization of the public printing, about two years since. The chief provisions of the law of reorganization are, one for the election of the same or separate public printers by the Senate and House, the rates and style being carefully defined in the law; one, instituting a responsible superintendent of public printing, appointed by the President, whose business it is to make sure of the proper execution of all printing ordered, in respect to manner, time and quality; and finally, one, directing that a general contract be made for the supply of all the document and bill paper used in public printing, this contract being made by the superintendent, who is also re-

* Report of the Superintendent of the Coast Survey, showing the progress of the Survey during the year 1853. Washington, D.C. Robert Armstrong Public Printer. 1854. Quarto Report, 88 pp. Appendix, 180 pp. Total pages 276 and 54 sketches.

sponsible for its proper fulfillment. The whole efficiency of this system is dependent on the capacity and integrity of the superintendent of public printing, who should be not only an honest man, but a man of administrative capacity and of technical acquaintance with the details of printing, a judge of the quality of paper, a critic of engraving and of the varieties of engraved prints, and a thorough proficient in the printing usages of Congress. This appointment has thus far, we believe, fallen into good hands, and to this, with the praiseworthy seconding of the public printer, we are bound to ascribe most of that conspicuous improvement in all typographical elements so observable in the Congressional documents of the last two sessions. The plan of furnishing inspected contract paper to the printers instead of permitting them to impose on Congress whatever trash they might choose, has proved a capital hit, and will work admirably so long as the contracts are rightly awarded and the contractors held rigidly to the bond. To the recipients of Congressional documents it is so great a blessing to have them decently executed, as certainly they now are in respect to paper and printing, that the recent renovation seems worthy of distinct critical acknowledgment and public congratulation. Unfortunately, such congratulation must stop short of documentary binding, which is still the victim of a wretched system, poorly administered. Indeed, nothing less than a national bindery, a superintendent of binding or the bestowal of the functions of such an office, with a binding assistant, on the printing superintendent, would seem adequate to the cure of existing abuses.

To men of science, Congressional documents are rapidly growing in interest. Much important scientific matter now sees the light in this and only this form. A large portion of the researches, investigations and explorations of the country, are in some wise so related to the general government, as to find their fitting place in the immense series of Executive documents and reports of committees. If to these be added the scientific publications of the State governments, it is really quite surprising to observe how large a portion of the labors of our scientific men are published through these channels. This is doubtless a natural result of the great preponderance of descriptive research and science in a country so unexplored as ours, and in which for that reason, natural history, botany, mineralogy, descriptive geology, geography and meteorology, rightly occupy leading places, and specially enlist governmental patronage. For a time, general and abstract researches will, and legitimately may, give place to the labors of the literal historiographer of nature, though this discrimination ought on no account to survive the occasion for it.

Most of the descriptive science published by Congress has been in connection with the various expedition reports by the

government officers employed from time to time in exploring our western territory, and those from foreign shores, important to our commerce though too little known. Investigations into the botany, natural history, geology, meteorology, topography and agricultural capacities of the various sections explored, have formed integral and essential parts of these explorations, and of course their results have been duly incorporated into the several reports. From Lewis and Clark, Long and Nicollet, down to the present time, these expedition reports have been growing in number, interest and value. The explorations of Wilkes, Fremont, Abeel, Pope, Peck, Cook, Whiting, Miehler, Simpson, Cross, Sitgreaves, Stansbury and Gunnison, Marcy and McClellan, Emory, Whipple, Williamson, Evans, Stevens and McClellan, have been or soon will be formally reported to Congress, and together they constitute a large part of the reliable information now published on our immense western and southwestern territory. In addition to these have been or soon will be published on foreign countries, the Wilkes Exploring Expedition narrative, maps, and scientific descriptive volumes, Lynch's Dead Sea, De Haven's Arctic exploration report, Herndon's and Gibbon's Amazon reports, the reports and results of Gillis's Astronomical expedition to Chili, the reports of the Japan expedition, Ringgold's North Pacific expedition, Page's La Plata exploration, an African exploration, &c. Add to these Foster and Whitney's Reports on mineral lands, Owen's Geological Report, Schoolcraft's Indian publications, the Census Reports, the Patent Office Reports, the Coast Survey Reports, the Smithsonian Reports, and the multitude of less pretending reports on scientific subjects (such as the Capitol extension, building stone experiments, Espy's reports, boiler explosion reports, on anæsthetic agents, &c.) embraced in the file of Executive documents and reports of committees: the resulting aggregate of matter possessing scientific value thus published by Congress, far exceeds our natural anticipation both as to amount and importance.

Unfortunately, the scientific value of materials published in the documentary series, whether of Congress or of State legislatures, is very much impaired by the unsystematic and injudicious plan *of distribution* actually pursued. Men of science to whom particular reports would be of direct practical use, are often entirely unable to procure copies of them, while many men of more political importance, but who will never even look into them, have these same reports profusely lavished upon them. Valuable documents which are reported to applicants as all exhausted, do wholesale duty as wrapping paper for Washington grocers and market men, at a standard price of four cents a pound, maps and plates included. This subject of documentary distribution deserves the serious attention of Congress, and it would not seem

a vain hope that some system could be devised which would be indefinitely superior to that now prevailing, as well in respect to securing rigid responsibility for documents as property, and in promoting the economy, order and convenience of their practical distribution, as in the more important point of securing something like fitness in sending special documents to their appropriate recipients. Distributing Owen's Geological Report to a dry goods importer and the Treasury report on commerce to a geologist, would seem too great an absurdity to exist if we did not know that hundreds of truly valuable volumes are annually thus wasted. This place is not the fitting one for a full discussion of this subject, but it does seem specially appropriate here to state, that a general wish certainly prevails among our scientific men, for the speedy adoption of some system whereby each actual investigator can be regularly and certainly furnished with the exact documents he needs. To purchase these works at regular publishers prices, would be on the whole better for them, despite their notorious brevity of purse, than the present system of lottery distribution; but save in a few exceptional cases, regular purchase is impracticable. The British system of publishing parliamentary documents at moderate fixed prices, would undoubtedly be more acceptable to cultivators of science than the existing chaotic practice which sends away the larger portion of Congressional and State reports on scientific matters. In half the instances, a report when needed is not now obtained at all, either by application or purchase, and when purchased, it is almost always at an exorbitant price. This whole subject deserves consideration and reforming action.

We believe that the printing orders of Congress will enable the Superintendent of the Coast Survey to send copies of his Report for 1853 to the active cultivators of science and such other persons as would find it of real utility, application being duly made to him in Washington, with the name, address, occupation and special scientific or practical pursuits of the applicant. From this Report we will now abstract in a few pages the points of chief scientific interest embraced. Having been favored with the sheets in advance of binding, we are enabled to make this abstract in anticipation of the actual distribution of copies, which probably will not begin until sometime subsequent to the appearance of this article.

The Coast Survey has now reached a very regular rate of annual progress, and its operations during 1853 extended into each of the eleven Coast Survey sections constituting the entire United States coast. The progress of reconnoissance, triangulation, topography and hydrography during the year, has been very satisfactory, being much the same as during the previous year: on

this, there is no occasion for present remarks. In proceeding to give an abstract of science in the Report of 1853, we may advantageously make use of the following heads. 1. *Gulf Stream explorations;* 2. *Tides and tide gauges;* 3. *Longitude operations;* 4. *Geographical positions;* 5. *Map projection tables and notes;* 6. *Publishing records and observations;* 7. *Miscellaneous.* The remaining subject matter of the report lacks purely scientific interest, and could scarcely be abstracted. The details of field and office operations, the examinations of light house sites, the lists of parties, &c. are given with the customary fullness, constituting a thoroughly digested record of the year's operations.

Gulf Stream exploration.—This great and singular peculiarity, embracing in its mighty sweep our entire Atlantic off-shore vicinage, is so important to navigation and so essential a feature of our coast hydrography, both in its practical and scientific character, that its thorough exploration ought certainly to form an integral part of the Coast Survey, whence our off-shore charts are all to be derived. A specific and complete delineation and theory of this unique oceanic movement can only be reached as a result of elaborate and continued observations on all its physical and phenominal elements. This giant problem is thrown down as a gage at our national door, and the honor code of philosophic chivalry bids us accept the challenge. With a clear perception of the requirements of this great research, Prof. Bache in 1845 organized and began the execution of a plan of operations, which provided for running a system of perpendicular sections across the axis of the stream from selected points of the coast and observing at frequent stations along these sections, the several elements required. Between 1845 and 1848, sections were run from Montauk Point, Sandy Hook, Cape Henlopen, Cape Henry and Cape Hatteras; when from accidents and other hindrances, the work was intermitted until in 1853, when sections were run from Cape Hatteras, Cape Fear, Charleston, St. Simons, St. Augustine and Cape Canaveral. The results for 1853 are given in a sketch of detailed sections, and a general delineation of the Gulf Stream in its several component bands or threads, as thus far determined, will be found among the sketches. Over six pages of the Report are devoted to a full exposition of the results already reached.

The element of temperature, superficial and at various depths, has been chiefly observed, up to this time; the instruments used being Six's registering thermometer for moderate depths and Saxton's metallic deep-sea thermometer for the greater depths, a temperature sounding of 2160 fathoms having been made. One general result of the investigation is that "there are alternations

of temperature across the Gulf Stream, cold water intruding and dividing the warm, making thus alternate streaks or streams of warm and cold water. In fact, the Gulf Stream is merely one of a number of bands of warm water separated by cold water." A "cold wall" limiting the Gulf Stream on the shore side, is clearly made out, as also its slight shoreward slope from the warm water overlying the cold. A distinct current of underlying cold water from the northern regions is found alike in the northern and southern sections. "It can hardly be doubted that this cold water off our southern coast may be rendered practically useful by the ingenuity of our countrymen. The bottom of the sea fourteen miles E. N. E. from Cape Florida, 450 fathoms in depth was in June, 1853, at the temperature of 49° Farenheit, while the air was 81° Farenheit. A temperature of 38° (only six degrees above the freezing point of fresh water) was found at 1050 fathoms in depth about 80 miles east of Cape Canaveral. The mean temperature of the air at St. Augustine is 69°·9 Farenheit, and for the three 57°·5. The importance of the facts above stated in reference to the natural history of the ocean in these regions, is very great, but, of course, requires to be studied in connection with other physical data. It has also a bearing upon the important problems of the tides of the coast. This exploration of the Gulf Stream will be steadily prosecuted to its close, the different problems being taken up in turn or in connexion as may be found practicable."

The most remarkable fact brought to light in relation to the Gulf Stream is probably that of the existence of two submarine ranges of hills near its origin, which produced most marked effects on the distribution of its parts. "The form of the Charleston and Canaveral sections," as shown in the diagram, shoals "gradually from the shore to 53 and 36 miles respectively, then suddenly falling off to below the depth of 600 fathoms. On the Charleston section, 96 miles from the coast is a range of hills steep on the land side and having a height of 1800 feet and a base of about eleven miles on the seaward side; a second range 136 miles from the coast, 1500 feet high, with a base of about seventeen miles, on the outer side. Beyond this there is a more gradual rise. On the Canaveral section the inner range is 68 miles from the coast." The effect of this form of the bottom in forcing up the deep cold water stratum is very marked, so that the deep isothermals of section, exhibited a general conformity to the bottom curve. It is undoubtedly due in a considerable degree to these submarine hill-ranges, and to their uplifting of the cold water, that the Gulf Stream is divided into several superficial bands, though to what exact extent and how far subject to variations remain to be studied. Horizontally, the conformity of the Gulf Stream to the coast line configuration is verified even in detail,

and its modifications by the variation of steepness in the off-shore bottom slope, are strongly marked. With these results, the names of Lieuts. Davis, George M. Bache, Richard Bache, S. P. Lee, Maffitt and Craven are conspicuously associated; George M. Bache being distinguished as a martyr to his zeal, in the very glow of talent, hope and success.

The results of the microscopic examinations of seventeen Gulf Stream bottoms made by Assist. L. F. Pourtales (Appendix No. 30), are of great interest. From these and many other investigations of bottoms, he has derived the generalization that the per-centage of shells, chiefly Foraminiferæ, progressively increases with the depth, and he remarks of a bottom from the depth of 1050 fathoms that it "is no longer sand containing Foraminiferæ, but foraminiferæ containing little or no sand. The grains of sand have to be searched for carefully under the microscope, to be noticed at all." It will be seen that this result coincides with Prof. Bailey's recent announcement, thus closely linking the Gulf Stream bottoms with those of the remoter parts of the Atlantic. Mr. Pourtales also somewhat examines the question whether these minute animals lived where they were found, or have been gradually washed down from the reefs. Though not decisive the evidence inclines him to the opinion that they lived where found. This is indicated by the fact that most of the individuals are found perfect, notwithstanding the extreme delicacy of the shells, and again by the delicate pink color of the Globigerinæ, which could scarcely survive transportation. The fact of the occurrence of the same species off the New Jersey coast and off Cuba and other West India islands under very dissimilar circumstances of light and temperature is also indicative that they are actually drawn from their true habitat in these Gulf Stream soundings. Mr. Pourtales well remarks on the importance of "a knowledge of the habitation and distribution of the Foraminiferæ" to geologists, "since of all classes of the animal kingdom, none has contributed so large a share to the formation of rocks, at least in the cretaceous and tertiary formations."

Tides and tide gauges.—It is an indispensable step in the survey of each harbor, river, bay, &c., of the coast, to make special observations on the tides; at least so far as to establish the place of reference to which the soundings shall be reduced and to have adequate tide records for effecting this reduction of each sounding. A tide table with the corrected establishment and notes descriptive of the tidal movements are parts of the engraved matter required to go on each finished chart of the Coast Survey. In the regular prosecution of this work, there thus results a great accumulation of tidal observations which require reduction and discussion before the charts can be completed. Also several permanent tide stations are established along the coast, to furnish by

their minute and continuous records the elements of wider and more critical investigations into tidal phenomena. All these observations are now regularly reduced by a special "tidal party" under the particular direction of Prof. Bache.

The Report of 1853 (Appx. No, 26) contains a very valuable table, embodying the principal reduced results at 64 important tide stations on the Atlantic, Gulf and Pacific coasts. Appendix Nos. 27, 28 and 29, contain elaborate discussions by Prof. Bache, of the tides at Key West, and Rincon Point, San Francisco, in which they are reduced and resolved into results of the physical tidal theory. The curves of the phenomena of the theoretical components are presented in three plates. Prof. Bache thus sums up the tidal peculiarities of our entire coast. (p. 7, Report.)

"It is an interesting fact that the tides of our Atlantic coast, of parts of the Gulf of Mexico, and of the Western coast, are of three different types. Those of the Atlantic coast are of the ordinary type of tides—twice in twenty-four hours—having, however, a distinct though small difference in the height and time between the morning and afternoon tides, known as the diurnal inequality. The Gulf tides are single-day tides, and, until the Coast Survey developments established the contrary, were believed to depend upon the winds which have the character of trade-winds, and, therefore, considerable regularity along that coast. The tides of our Pacific coast ebb and flow twice in twenty-four hours, but with so large a diurnal irregularity in height that the plane of reference of mean low water, commonly used on the charts, would if employed be a snare to navigators. A rock in San Francisco bay, which at one low water of the day might be covered to the depth of three and a half feet, might at the next be awash. The observation of the tides on the Atlantic coast having been made in close connection with the other parts of the hydrography, the stations still wanting will be filled up as we advance. A few stations are still required on the Gulf of Mexico to complete the general determination of its tides from Cape Florida to the Rio Grande. We have already found nearly the dividing position, Cape St. George, Apalachicola, where the tides resemble on the one side, eastward, those of Cedar Keys, Key West and Tampa Bay, ebbing and flowing twice each day, with a large diurnal inequality, and on the other, westward, resemble the tides at Mobile entrance, the Delta of the Mississippi, Galveston and the Rio Grande entrance, ebbing and flowing, as a general rule, but once in twenty-four hours."

The Report contains a detailed description of Saxton's self-registering tide gauge (Appendix No 38. Sketch No. 54); a report of operations in establishing a tide gauge with a pipe leading seaward on a difficult open coast near Nantucket (Appendix No. 13); and finally a report of operations in obtaining off-shore or

open ocean tidal observations, on a shoal a mile and a half from land. (Appendix No. 15.) The excellent results from Saxton's gauge lead to high expectations from the records now regularly received from three permanent and three movable Saxton gauges, operating on our Western coast. The importance of separating the true tide wave from the heaping up of water along shore, leads us to watch with peculiar interest the off-shore observations and to hope for their success at much greater distance from land.

Longitude operations.—It is now esteemed essential where practicable, in conducting the survey, to refer at least one principal station in each section, to the central longitude point (Seaton station, Washington), by a telegraphic determination of longitude differences. During the year 1853, operations were conducted for thus connecting Charleston with Seaton station, the longitude difference already found by Mr. Walker in 1850, being only a preliminary determination. Such was the imperfect condition of insulation of the telegraph wires, as found by repeated trials, that it became indispensable to establish an intermediate station and Raleigh was thus occupied. Dr. B. A. Gould's report of these operations is given in Appendix No. 33. Some observations were also made on the velocity of the galvanic wave, and the personal equations of the observers were duly compared. Charleston will soon be in turn similarly connected with New Orleans.

Prof. B. Peirce reports (Appendix, No. 31) the results of his investigations and of some observations made under his charge, for the purpose of ascertaining a method of determining, "the longitude from observed transits of the moon, which shall not be involved in the great and singular errors of the lunar ephemeris." After stating the faults of the present methods, in which standard corresponding observations of moon culminations are interpolated, he concludes that as the existing lunar theory will not stand the test of observation, a correct ephemeris is not now practicable. He then, from three hundred and sixty-seven special comparisons, determines the standard probable error of an observation of a lunar transit as one-tenth of a second of time. An attempt to determine an annual empirical correction for the lunar elements failed, and gave place to the determination of a constant error of epoch and a periodical error running through a half lunation. For Greenwich observations, 1847, the probable error of ephemeris longitudes thus corrected, when compared with observation, came out very nearly the standard probable error of a transit observation. This investigation has been since continued.

Prof. W. C. Bond reports (Appendix, No. 32) the results of some operations for testing the accuracy of the spring governor records. Numerous star transits of Spica (110 are given) over the wires of the Cambridge equatorial were simultaneously recorded by two spring governors, differing one-tenth of a second

in their pendulum-vibration times: one governor was at Cambridge and the other at Haverford, Penn. The result of scrupulous comparisons of their records, indicates no discrepancy exceeding three hundredths of a second ascribable to the imperfect equalizing action of the spring attachment.

Mr. G. P. Bond has reported in considerable detail, (Appendix No. 34,) on the computations of the chronometric expeditions of 1849, '50 and '51, for determining the difference of longitude between Cambridge, Mass., and Liverpool. He details the precautions taken to ensure accurate reductions of the transit observations for local time and for evolving errors of observation. He discusses the micrometric and level division values, the azimuth and collimation errors, lateral refraction, personal equations, clock errors, the position of the midwire of the transit, the pivot figures, the errors of comparing the chronometers with the standard clock, and the irregularities of chronometer and clock rates. The general results of the computations have since been submitted to the Amèrican Association at the Washington meeting, when Prof. Peirce announced additional discussions of moon culmination longitude methods, in reference to the longitude of America. We are now near the final fixation of the standard longitude difference between our system of connected stations and that of Europe: which difference once authoritatively established, will doubtless be liable to no future change, unless by submarine telegraphic determinations.

Geographical Positions.—In the Coast Survey Report for 1851, is a list of 3240 stations, to which an addition of 600 is made in the Report of 1853, (Appendix No. 7.) For each of these 3840 stations, a latitude and longitude is given. Also for each of the lines connecting these 3840 stations, as shown in the section triangulation sketches, the length of line is given in metres, yards and miles, and its azimuth in both directions is introduced. This extensive series of geographical positions and of triangulation elements is the product of an immense labor of observation and computation, being indeed the great trigonometrical consummation of the survey up to the present time. It will prove of wide and permanent use to have easily accessible so extensive a series not only of latitudes and longitudes of stations but of accurate distances and bearings between so many intervisible points, along our entire seaboard. In numerous instances, surveyors can conveniently test their compass variations by observing on one of these lines of given azimuths. The enduring value of this list for plotting surveys and maps is self-evident and will not be impaired by the slight corrections to which some of the positions and distances will in future be liable.

The development of *station errors*, or distinct discrepancies between the geodetic and astronomical latitudes and longitudes

of particular stations, is a constantly recurring result of the survey. They are caused by local irregularities in figure and density of the earth and amount in several instances to about three seconds, while at a station of the Ordnance Survey, the station error is nine seconds. The attraction of mountains is not the usual cause of these errors, though a displacement of the vertical to a much greater amount has in some cases been traced to this origin. When it is known that even now, before the mutual verification of sections by connecting their base lines, the tabular distances given in this list are generally considered as liable only to an average error of about one foot in six miles, it will scarcely seem wonderful that the station errors are found to be as distinctly indicated by a comparison of the azimuth and back azimuth observations, as by those for latitudes and longitudes; so that the two results even verify each other quantitatively. The notes introducing the list give a clear insight into its mode of construction and arrangement.

Map projection tables and notes.—The most voluminous adjunct to this report is Appendix No. 39, giving tables for projecting maps with notes on map projections. It is much to be hoped that this valuable accession to the means of accurate map construction will accomplish something towards effecting a reform in the very imperfect chartographic practice, now too widely prevalent.

The notes present in a condensed form a classified synopsis of the various projections which have been used. The four classes into which these are distributed are based on their peculiar modes of mathematical genesis. The distinctive features of eighteen species of projections, are briefly and systematically presented. Bonne's projection, being that chiefly used in Europe for topographical surveys of considerable areas, is discussed in greater detail. Still more space is given to the *polyconic* projection, which is that used in the Coast Survey office. This name is new, and the two varieties, called *rectangular* and *equidistant* are both in use and require the same tables. Fortunately these methods can now be employed by any intelligent draftsman, furnished with this report, in constructing any local, county, state or general map, within the United States. Full instructions are given under a special head for the graphic construction of the rectangular and equidistant polyconic projections. The formulæ used for computing the tables, also the constants employed and their logarithms are given, though without the detailed derivation of the formulæ.

The Tables are six in number. Table I. gives the relation between the units of length used in different countries—Table II. has for its object to facilitate the conversion into each other of metres, yards and statute miles, and will be found highly con-

venient in many computations—Table III. gives the length, in statute and nautical miles, of a degree of the meridian for each 5° between latitudes 20° and 50°—Table IV. gives the length of a longitude degree for each degree parallel, between Latitudes 17° and 50°, expressed in nautical and statute miles and meters —Table V. gives the lengths of the parallel and meridian arcs and coördinates for projecting large maps in the United States and can be used for a map embracing the area between Latitudes 17° and 50° and extending 70° in Longitude, which limits include considerably more than the entire United States. Table VI. gives the lengths of the arcs of parallels to seconds for each minute of Latitude between 24° and 50°; it also gives the meridian arcs and coördinates with corresponding accuracy. This table is available for constructing any local map projection on a large scale, anywhere within the latitude specified. For state or general maps, Table V. should be used, and for town and country maps, &c., Table VI. is required. It will be seen that these tables suffice for all the geographer's needs within our national limits, while a little study and practice will enable any one to use them correctly and rapidly. The superiority of the projection on which these tables are based, should induce its general use for all the purposes indicated.

Publishing Coast Survey records and observations.—We observe with satisfaction that the estimate for the ensuing year, embraces an item of $20,000 for the long desired and oft recommended publication of records and observations, made in the progress of the survey. It is earnestly to be hoped that the experience of the British Ordnance Survey in relation to this class of publications will not be lost on us. Why should we wait until their day of greatest-usefulness is past, until all freshness of interest has departed, before doing what must be done at last, and what ought for all positive reasons to be done now? Delay is most thriftless policy in this case, and especially as the Treasury is now plethoric with surplus revenue. The whole matter is thus clearly set forth by the Superintendent. "The history of such works shows that the observations accumulated during their progress and which must be published for permanent reference and to give them authenticity, are brought out very slowly. Those who have taken part in them are dispersed, and questions arise which require their aid to answer. However perfectly in theory a work is organized, such questions will arise. The interest in the results is lost with the responsibility for their accuracy. The present time, when the organization is complete, and the observers are still connected with the work, is the proper time, on every account to publish the observations. The economy of present publication would be very considerable. I am sustained in these views by the judgment of the scientific men of

the country generally." We certainly hope to see this work soon commenced.

Miscellaneous.—Among the operations in Maine are reported some measurements of heights by nearly all practicable modes. From these and other operations there, we see evidently looming forth "important data for the coefficient of refraction under different circumstances, and in relation to the relative advantages in accuracy, time and other particulars of the different modes of measuring heights."—At East Base near Galveston, an elaborate set of latitude and magnetic observations is reported.—The list of Coast Survey nautical discoveries and developments for the year embraces nineteen items, chiefly of shoals, rocks, banks and changes in bars, inlets and harbors. The Gulf Stream submarine hill-ranges, before mentioned, are the crowning discovery.—The 54 Sketches appended to the Report, embrace much new hydrography of importance to navigation, and among other subjects of congratulation is specially noticed, the completion of the troublesome but highly important hydrography of Nantucket Shoals.—Appendix No. 36, consists of notes on lithography and lithographic transfers. The sketches give evidence of the value of the transfer process, being all (23,000 copies from each of 54 plates) printed by its aid, without serious wear of the copper plates. The subject of adapting engraving to transfer printing is touched upon and is of much importance to such as are about using the transfer process.—Appendix No. 37, describes a novel instrument called the interranger, for enabling boats conveniently to run on range lines between opposite stations; also some account of various devices tried.—Appendix No. 35, gives the results of two analyses of deposits taken from the boiler of the steamer Hetzel. This is a subject of practical importance and it is to be hoped that some corrective may grow out of such analyses.—Appendix No. 43 will be interesting to those who feel how great a loss the country sustained in the death of Sears C. Walker.

There is much more in the various field and office operations, which might interest scientific readers, but space bids us refrain. In conclusion, we may remark, that taken as a whole, this report equals or exceeds any of its predecessors in the extent and value of its contributions to science; and that by its paper, typography, indexes and sketches, it goes far towards reasserting the admissability of a Congressional Document to respectable libraries. H.

Art. XXVI.—*On the use of Hydrogen Gas and Carbonic Acid Gas, to displace Sulphuretted Hydrogen in the analysis of Mineral Waters, &c.;* by Prof. W. B. Rogers and Prof. R. E. Rogers.

First.—On the use of *Hydrogen Gas* in the analysis of Sulphureous Waters.

One of the most difficult points in the analysis of mineral waters is the determination of the sulphur which is contained in many of them in the two conditions of Sulphuretted Hydrogen, and a sulphid, either of an alkaline metal or of magnesium or calcium. No satisfactory process has we believe yet been devised for this purpose. It is easy enough by the nitrate of silver or chlorid of copper to determine the total quantity of sulphur present in these compounds; but in the subsequent process of boiling the liquid preparatory to the precipitation of the sulphur of the sulphids, while we expel the free hydrosulphuric acid, we at the same time decompose the sulphid of magnesium or calcium which may be present, even when the process is conducted out of contact with the air, as in an atmosphere of hydrogen gas; and if we boil the liquid in the air or even expose it for some time to the atmosphere at common temperatures, the sulphids of sodium and potassium as well as of magnesium and calcium, are gradually decomposed by the carbonic acid of the air evolving their sulphur in the condition of hydrosulphuric acid. At the same time by the action of the atmospheric oxygen a portion of the alkaline sulphid is converted into hyposulphite. It is therefore desirable to discover some method of separating the free hydrosulphuric acid without at the same time affecting the other sulphur compounds present in the water. This we think we have attained by transmitting through the sulphureous water a stream of hydrogen gas. In repeated trials made with natural hepatic waters, among them the celebrated Blue Lick water of Kentucky, and with an aqueous solution of hydrosulphuric acid, prepared for the purpose, we have found that by continuing for a sufficient time the washing action of the hydrogen, we could reduce the sulphuretted hydrogen to an almost insensible trace. A volume of 25 cubic inches of the Blue Lick water submitted to this action for one hour retained only a minute fraction of the original charge, and in one and a half hours it gave only the slightest appreciable trace of sulphuretted hydrogen.

The hydrogen used for this purpose, before reaching the vessel which contains the mineral water, is conducted through a solution of potassa in order to remove any hydrosulphuric or carbonic acid it may contain. Thence it is made to pass into a second

vessel containing the sulphureous water through which it bubbles in a brisk but not violent stream. The gas, more or less charged with sulphuretted hydrogen, is led into a third vessel containing either a solution of nitrate of silver to which ammonia has been added, or an alkaline solution of arsenious acid, to arrest the sulphuretted hydrogen. The former solution is greatly to be preferred where the mineral water is only feebly sulphureous. The sulphur thus precipitated is to be determined in the usual way.

Suppose the mineral water to contain free hydrosulphuric acid together with sulphids say of potassium and magnesium, we may proceed as follows:

1. We determine for a given volume of the water the total amount of sulphur present by the use of chlorid of copper or nitrate of silver.

2. We subject an equal volume of the water to the hydrogen current until the escaping gas gives only the faintest trace of hydrosulphuric acid when the jet is received on a surface of white porcelain rendered moist by a mixture of nitrate of silver and ammonia. The mixed gas being passed into the silver or arsenious solution, gives a precipitate from which we determine the amount of free hydrosulphuric acid in the water.

3. We apply heat to the flask containing the sulphureous water which has been thus treated, so as to cause gentle boiling, at the same time supplying the upper space with hydrogen in a moderate but steady stream. It will be found that below the point of ebullition the issuing hydrogen will give scarcely a trace of hydrosulphuric acid, but as soon as the liquid begins to boil, the stream of vapor and hydrogen plainly shows the presence of this substance, then slowly evolved by the decomposition of the sulphid of magnesium or calcium.

4. We treat the remaining liquid with chlorid of copper, or the arsenious solution, to determine the sulphur of the alkaline sulphid which is the only sulphur compound left in the water. The sum of this and the sulphur of the free hydrosulphuric acid subtracted from the total quantity of sulphur gives that of the sulphid of magnesium.

We find that a proportion of hydrosulphuric acid too small to be quantitatively determined by precipitation from the water itself can be ascertained by the use of the stream of hydrogen. It is only necessary to pass the gas which has been transmitted through the water into an ammoniacal solution of nitrate of silver in a long test tube or Liebig's bulb. By continuing the action for one or two hours we obtain a precipitate capable of being separated.

When the water contains no sulphid of magnesium or calcium, it is merely necessary, after determining the total amount of sulphur present, to boil the liquid in an atmosphere of hydrogen

as long as the gas gives a distinct trace of S H by the tache on porcelain as before described; and then by precipitation to determine the quantity of sulphur in the alkaline sulphid of the remaining liquid.

From what has been said it is obvious that the only practical objection to the process here proposed is the tardiness of the displacing action of the hydrogen gas; but considering the acknowledged imperfection of the methods in use, we think that it may be found worthy of adoption.

Second.—On the use of *Carbonic Acid Gas* in the analysis of mineral waters containing Sulphuretted Hydrogen.

As might be inferred from its great absorbability by water, carbonic acid acts much more rapidly than hydrogen in separating hydrosulphuric acid from that liquid. To assure ourselves of this effect, we made several experiments with natural and artificial sulphureous waters, all of which led to the same result. The following example will suffice to show the efficiency and promptness of the displacing action of the carbonic acid.

Twenty-five cubic inches of Blue Lick water contained in a narrow necked bottle, were subjected to the washing action of a brisk stream of carbonic acid gas previously purified by transmission through water. In fifteen minutes the liquid, tested by ammoniacal nitrate of silver, gave a scarcely discernible trace of hydrosulphuric acid, and in twenty minutes not a vestige of it could be detected by the same reagent. The rapidity and completeness of the separation are as striking as the ease with which the experiment can be made.

When therefore a mineral water is known to contain sulphuretted hydrogen only in the free state, we would recommend as the simplest and most exact method for determining this ingredient, to pass through the liquid a stream of washed carbonic acid gas, and to arrest the hydrosulphuric acid, by conducting the current of mixed gas into an ammoniacal solution of nitrate of silver in a small flask or Liebig tube. The precipitated sulphuret being mingled with only a small volume of liquid, admits of more easy separation and determination than when formed in the usual way by adding a precipitant to a large mass of the mineral water. In the case of feebly sulphureous waters this method is we think greatly superior in accuracy as well as promptness to any of those in use. By operating on a considerable volume of the water, the flask or tube will furnish the precipitated sulphuret in sufficient amount for a quantitative determination in cases where in the ordinary way no separable precipitate would be obtained.

As carbonic acid is capable of decomposing the sulphids contained in a mineral water giving rise to free hydrosulphuric acid,

it cannot be employed for determining the quantity of the latter when associated in the water with a sulphid. In this case the stream of carbonic acid would carry with it the hydrosulphuric acid due to its reaction with the sulphids, as well as that existing ready formed in the liquid. For such a water, hydrogen gas used as above explained, is the proper displacing agent.

ART. XXVII.—*On Changes of the Sea-Level effected by existing Physical Causes during stated periods of time;* by ALFRED TYLOR, F.G.S.

(Concluded from page 32.)

PART II.

ALLUSIONS have already been made to the difficulty of proving whether or not the sea-level had been *gradually* elevated, because the rise of the waters would conceal the evidence of their former height except just at the mouths of rivers, where deposits of fluviatile alluvium might raise the land from time to time and keep it above the waves. The recent strata formed at a few such localities have been described by the best observers; and while there are appearances in several cases which might be to some extent explained by the supposition of a gradual rise of the sea-level, yet no proof could be obtained without the concurrent testimony of a much greater number of instances than have yet been brought forward. Sufficient information, it appears, exists to show that the quantity of alluvium in the deltas of such rivers as the Mississippi, Ganges and Po, is so enormous, that the accumulation must have occupied a period of time during which it would not be possible to conceive the sea-level stationary.

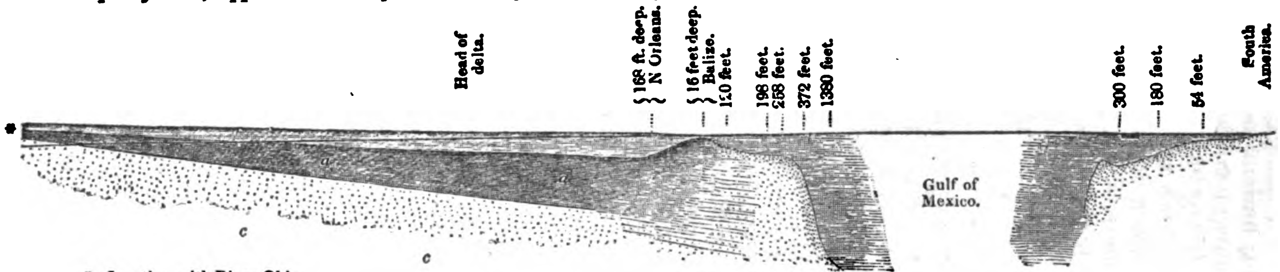
Little progress could be made in an inquiry of this kind without clear views of the operations of rivers. The recent reports of engineers upon this subject supply an important link in the chain of evidence, and enable us to understand the laws which govern the formation of alluvial plains along the lower parts of all river-courses.

The diagram (fig. 8) represents a section of 600 miles of North America, through the alluvial plains and delta of the Mississippi,* together with a section of the Gulf of Mexico, from a point 100 miles east of the Balize to the continent of South America. The sea-bottom is marked from the soundings on the Admiralty Chart, and the depth of the Mississippi and its fluviatile deposit are inserted from statistics collected by Sir C. Lyell.†

* For a most valuable detailed description of the physical geography, &c. of the Mississippi and Ohio valley, see Mr. C. Ellet's paper, Smithsonian Contributions, vol. ii, 1851.

† See note, page 26.

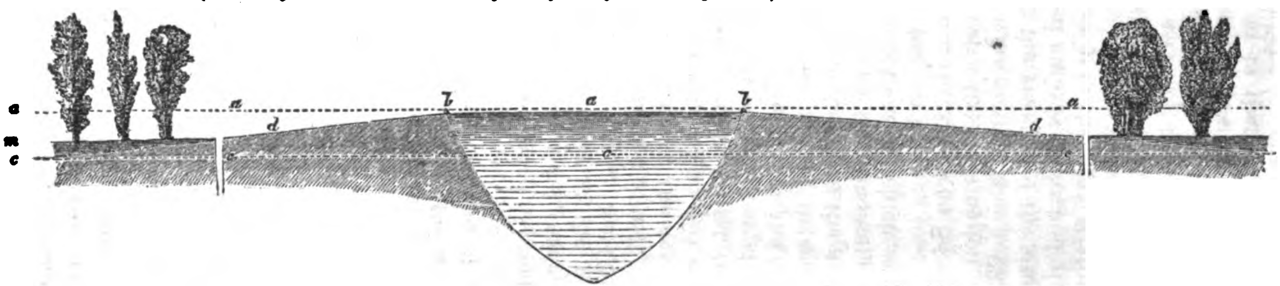
Fig. 8.—*Diagram showing depth of the Delta (supposed 600 feet); area* 14,000 *square miles; height of the river above the sea-level* 275 *feet at* *; *depth of river, supposed* 80 *to* 200 *feet in this diagram; ditto of plains, supposed to average* 264 *feet; area,* 16,000 *square miles.*



*. Junction with River Ohio.
a, a. Fluviatile strata of the plains of the Mississippi; the slope of these plains is determined by measurement to be about 1 foot in 10,000 towards the sea.
c. Marine strata.

Direct distances:—Junction with Ohio to Balize, 580 miles. Head of Delta to Balize, 180 miles. New Orleans to Balize, 70 miles.
[Vertical scale 1 inch to 1000 feet. Horizontal scale 1 inch to 150 miles.]

Fig. 9.—*Transverse section of the Mississippi, where it is* 1500 *feet wide and* 100 *feet deep, running in the midst of an alluvial plain* 50 *miles wide.* (*This diagram shows the section of slow-flowing rivers in general.*) Vertical scale 100 feet to the inch.



a, a. The level of water in the river during flood, which is 25 feet above the level of the distant marshes, *m, m.*
c, c. The level of water in the dry season.
b, b. Artificial banks or levees, 4 feet high.
d, d. The banks and plains.
m, m. Marshes, supplied with water by filtration from the river at all seasons of the year.

The whole body of water in the river must be in motion, so that even in flood time only a small per-centage of the water and alluvium in the stream can escape over the banks.

It will be seen that the level of the water in the Mississippi, near its junction with the Ohio, nearly 600 miles from the Gulf of Mexico, is 275 feet above that of the sea. The slope of the alluvial plains through which the river winds will therefore be less than 1 foot in 10,000.

The hills bordering the valley of the Mississippi are cut through in several places by the river, thereby exposing good sections of their component strata, consisting of alluvial deposits thought to be much more ancient than those we are about to consider.

An area of 16,000 square miles is occupied by the more modern alluvial formation between the head of the delta and the junction of the Ohio.* It is supposed to be, in the average, 264 feet deep, and is from 30 to 80 miles wide. The true delta extends over 14,000 square miles, occupying a frontage of 2¼ degrees on the coast-line of the Gulf of Mexico, and extends 180 miles inland. At its southern extremity its surface is hardly above the level of high tides, but it rises gradually as it passes inland, and at New Orleans is nearly 10 feet above the sea-level.

A boring near Lake Pontchartain, of 600 feet, failed to penetrate the modern alluvium; and wherever excavations are made, the remains of trees are frequently found, apparently in the places where they grew, but now far below the sea-level. Sir Charles Lyell computes its average depth at 528 feet, and consequently nearly the whole of this modern deposit is below the sea-level, yet is supposed not to contain marine remains. The fall of the Mississippi during a course of 600 miles is shown by fig. 8; the depth of the channel varies from 80 to 200 feet until it approaches the Balize, where it shallows to 16 feet. The rise of the tide at this point is only 2 feet. The depth of the alluvial deposit below the river-channel is also indicated, together with the surface of the more ancient formation upon which the Mississippi has formed this great alluvial deposit, the bottom of which is now more than 500 feet below the present sea-level.

Mr. Charles Ellet, Jun., in a Report to the American Secretary of War, January 29, 1851, communicates the information from which the diagrams figs. 1 and 2 are constructed. See p. 23.

The theory of Mr. C. Ellet is, that the velocity of the stratum of fresh water (fig. 1) is communicated entirely to the underlying stratum, composed of salt water, partially to the next stratum 3, but not at all to stratum 4, which is stationary: stratum 5 is also marine, but it flows in an opposite direction to the rest, and restores the salt water which is carried away by the friction of the upper stratum, No. 1, against the suface of No. 2.

It is supposed that the rapid increase of deposit at the bar, fig. 1, arises from stratum No. 5 carrying mud to that point, where its

* Lyell's Second Visit to the United States, 1849, vol. ii, pp. 146–152, 155, 169, 194, 195, 203, 243, &c.

velocity is partially neutralized by impinging against stratum No. 1.

From the following particulars of the deltas of the Ganges and Po, it would appear that they are similarly situated to the Mississippi. "An Artesian well at Fort William near Calcutta, in the year 1835, displayed at a depth of 50 feet a deposit of peat with a red-colored wood similar to that now living. At 120 feet clay and sand with pebbles were met with. At the depth of 350 feet a freshwater tortoise and part of the humerus of a ruminant were found. At 380 feet, clay with lacustrine shells was incumbent upon what appeared to be another dirt-bed or stratum of decayed wood. At 400 feet they reached sand and shingle."*

In the delta of the Po, a well bored 400 feet failed to penetrate the modern alluvial deposit; very near the bottom it pierced beds of peat, similar to those now forming. The coarser particles of mud which have already passed the mouths of rivers may contribute to the marine or fluvio-marine deposits forming outside deltas; but this can only be to a limited extent, as the great bulk of the mud is far too fine to settle near the coast. Little material could be obtained from cliffs along the sea coasts, but we have information of marine currents specially bringing sand and mud from other parts of the sea-bottom to the neighborhood of deltas. (See Mr. Ellet's observations.)

For these reasons, if the further examination of the deltas of the Mississippi and other rivers should lead to the discovery of some recent marine or fluvio-marine strata, it may turn out that such deposits have been more rapidly accumulated than the purely fluviatile beds with which they may be associated. In estimating the age of deltas, allowance, however, ought to be made for such contingences, and also for their organic contents.

Let us now turn to fig. 9, which exhibits Sir Charles Lyell's transverse section of the channel and plains of the Mississippi, and at all points throughout a course of several hundred miles. The dotted lines are introduced to show the variation of the water-level in the wet and dry seasons: *b*, *b* represent the artificial levée; *d*, *d* the banks and plains; *m*, *m* the swamps of the Mississippi. "The banks† are higher than the bottom of the swamps, because, when the river overflows, the coarser part of the sediment is deposited on the banks, where the speed of the current is first checked" (Lyell). The channel, however, is so wide and deep, that even if there were no artificial banks to prevent floods, the river would carry into the Gulf of Mexico the principal mass of the mud it had received with the water of its tributaries; for it is only for a short time in the year that the level of water in

* Lyell, *loc. cit.* p. 248; and Principles, p. 267–270.

† There is a similar section of the Nile and its banks published in the fourth volume of the Quarterly Journal of the Geological Society, p. 344, but communicated by Lieut. Newbold in 1842.

the river is above that of the adjoining plains. The swamps and the numerous lakes formed by deserted river-bends communicate at all times of the year with the main stream. In these places mud could be constantly deposited mingled with the remains of the vegetation which grows luxuriantly in the swamps. The only supply of inorganic matter for raising the level of the vast plains through which the river winds for hundreds of miles, must be the mud deposited upon them during the periodical floods. These are very much prevented by the artificial levée; but when they do occur, their force is augmented by the water being artificially dammed up.

"I have seen, says an eye-witness, when the banks of the Mississippi burst, the water rush through at the rate of ten miles an hour, sucking in flat boats and carrying them over a watery waste into a dense swamp forest" (Lyell). It would appear that the Mississippi differs in size and proportion more than in other respects from our rivers. For instance, when floods occur upon our own alluvial plains, they are most conspicuous at a distance from the stream which caused them, indicating that the parts of the plains nearest the banks are higher than those at a distance from it, and therefore that fig. 9 would also represent the *transverse section of slow rivers generally*. The similarity of the physical features presented by the lower parts of all rivers was particularly remarked by Hutton.*

It has been observed by engineers,† that in all rivers in this country the large quantities of silt brought into them by winter freshets do not tend to choke the channels, but that, at that period of the year, former accumulations of deposit are actually removed by the force of the stream; and therefore, that although winter-freshets bring down silt with them, they carry into the sea a larger quantity than they have introduced into river channels.‡ If it were allowable to assume that the unequal supply of water at different seasons of the year produces effects in the channel of the Mississippi similar to these just described on our own streams, the following consequences might be deduced from the fact that winter freshets remove more detritus than they bring down. The diminution of the speed of the current of rivers assists the deposition of silt upon their beds, as much as its increased speed in the winter season favors its removal. The summer deposit, however thin it may be, cannot occur without contracting the sizes of the channel.

* Theory of the Earth, vol. ii. p. 205-211.

† On this and the following points see First Report of the Tidal Harbors' Commission, above referred to, which contains the opinions of our most celebrated engineers on the phenomena presented by tidal and other rivers.

‡ The author has not met with any explanation of the causes that produces changes in river-channels, although the constant alterations taking place in them have been repeatedly alluded to.

Winter-freshets following a sudden fall of rain would raise the water-level of rivers rapidly, and carry it above the banks before the augmented current has time to scour the river-channel and raise it to its former capacity. Accumulations of silt, small at any one place, must each raise the water a little above its proper level, and the point of overflow will be where the sum of these small elevations amounts to more than the height of the banks, above last year's level. But floods leave a deposit of silt, &c. upon the banks they pass over, which increases the capacity of the channel; and until new deposit has again reduced the area of the stream below its proper size, inundation will not occur.

As each flood raises only the part of the bank it flows over, it is easy to see that the point of overflow will be changed from time to time; and every part of the alluvial plains through which a river flows will be visited in turn by floods, provided there are no artificial banks. These banks assist the scouring power of rivers in winter, because they retain more water in the river; but, on the other hand, silt that would have been carried over the banks is kept within the channel, and this may be the reason why the beds of all navigable rivers have become so much elevated during the historical period. The contraction of water-channels in summer, and their enlargement in winter, is thus directly traced to the unequal supply of rain at different periods of the year.

This being admitted, we have an explanation of the manner in which rivers may, by a succession of floods, build upon alluvial deposits along their courses, at the same time raising their beds in proportion to the height of their plains.

If river-channels were perfectly symmetrical in form, the identical sediment that had fallen in summer might be removed again in winter. It is, however, well known that river-channels are deep on one side and shallow on the other. The principal deposit therefore takes place on the shallow or quiet side, and the principal removal occurs from the deep side where the current runs more quickly.

This may explain why the traveller on the Mississippi sees for hundreds of miles a caving bank on one side, and an advancing sand-bar on the other (Lyell). When the action of the river is also unequal on its two banks in different places along its course, a channel consisting of curves instead of straight lines must be produced. When each curve, however, had assumed the complete horse-shoe form, the water, by travelling round the outer circumference of the bend, will have its effective speed reduced to that on the inner or shallow side. The current would thus become more nearly equal in all parts of the channel, and necessarily the deposit likewise; and in winter it would have a nearly equal tendency to excavate the banks on both sides, which condition of equilibrium might last for some time.

Hutton, in 1795, has remarked, that there is evidence of denudation in every country where at any time of the year the streams carry off any particles of the superficial soil.* The Mississippi must derive its vast supplies of mud from thousands of such tributaries; for it could obtain them from no other source, unless we suppose it abstracts them from its own plains. Certainly in many places soil is being removed from one part or other of its plains; but an equal quantity must be added to some other part, for the river could not make a permanent inroad into its plains without enlarging its channel. This it does not do, or it would be able to carry off the winter-freshets without overflowing, and the present artificial bank would be unnecessary.

I have thus briefly referred to observations made by British engineers which may throw some light on the causes of periodical floods, and changes of channel in rivers, and also upon the formation of alluvial plains along their course. These questions need not further be entered into, because the limited growth of alluvial plains and deltas may be best illustrated by tracing the alteration in the mean level of a large part of North America that would be consequent upon a denudation sufficiently extensive to furnish the alluvium said to exist in the valley of the Mississippi. On the borders of the Gulf of Mexico at the present time marine strata are forming within a short distance of the fluviatile, and frequently alternate with them, because spaces of the sea-shore are enclosed by banks of river-mud and converted into lakes ordinarily communicating with the river, but sometimes with the sea after high tides.

The present marine or fluvio-marine deposits must be composed of mud that has passed the mouth of the river, or washed up by the sea, while the freshwater strata must be entirely formed from sand and mud carried over the river banks, or deposited on the bottom of lakes supplied by the stream before it enters the Gulf of Mexico. An idea of the amount of denudation that has taken place in the interior of North America might be either obtained from the extent of the marine deposits formed of mud that had passed the mouth of the river, or from that of the purely fluviatile and contemporaneous deposits formed from mud which had never entered the Gulf of Mexico.

But it is also necessary to estimate what proportion of the total quantity of mud brought down by the river is carried completely out to sea, compared to what is left either upon the marine or fluviatile portion of the delta.

Sir Charles Lyell has remarked, that the alluvium now remaining in the valley of the Mississippi can only represent a fragment

* Our clearest streams run muddy in a flood. The great causes, therefore, for the degradation of mountains never stop as long as there is water to run; although, as the heights of mountains diminish, the progress of their diminution may be more and more retarded. *Op. cit.* vol. ii, p. 205.

of what has passed into the Gulf of Mexico; and this can readily be believed when we reflect upon the depth and breadth of the channel, and upon the short period of the year that the stream would throw any large-quantity of mud into the plains even if there were no artificial banks. We must also bear in mind that only the coarse mud could settle near the shore, for the finer particles could not deposit except in very deep water. For these reasons, even if the mud carried beyond the mouth of the river is only ten times the quantity left behind on the fluviatile portion of the delta and plains of the Mississippi, this amount of detritus could not be obtained without the mean level of one-fifth part of North America being reduced 100 feet by denudation affected by the action of rain, the atmosphere, and running water.* But Hutton (vol. ii, p. 401) remarks, in 1795, that wherever any stream carried off particles of soil in its waters at any period of the year, it might be said that denudation was taking place in that country; yet he particularly observed that the waste of land was very unequal, being much more rapid in the elevated than in the more level parts of any district. It is therefore possible that, during the reduction of the mean surface-level of the land drained by the Mississippi to the amount of 100 feet, some portions of the area might be lowered many times that amount, while other portions might suffer little, or be positively raised by the superposition of alluvial deposit. We are, however, informed by Sir Charles Lyell, that the Mississippi in one part of its course cuts through ancient fluviatile beds evidently antecedent to those recent deposits we have been considering. This formation is also stated to contain the remains of species of plants and animals now existing; so that evidence is to be obtained in this district of still greater denudations (by these results) than those of which we have spoken, and which would produce changes on the surface of the earth since the introduction of the present fauna and flora of extent enough almost to realize Hutton's vision of mountains wasted away by the action of rain, the atmosphere, and running-water, and carried along river-courses into the ocean. It is not necessary to take an extreme view of this subject to gain the object we have in view, which is to show that, during the time occupied by the formation of the Mississippi delta, the sea-level might be perceptibly raised† by the agency of physical causes now in operation.

The reasons for supposing that a rise of 3 inches in each period of 10,000 years might occur, have been already discussed, and it only remains to state that, at the present rate of denudation, it

* The data for calculating the annual quantity of detritus carried over the river's banks, in relation with that carried down to the sea, are very imperfect. Further information on this subject is much needed.

† This change of level may amount, under certain circumstances, to a great extent, but at the *lowest* calculation would be 15 feet.

would require five such periods to produce the quantity of detritus said to exist in the valley of the Mississippi; while it would require fifty such periods to produce the requisite quantity of alluvium on the supposition that only one-tenth of the mud in *transitu* through the river was appropriated for the accumulation of its alluvial plains and delta. Under these circumstances it appears a legitimate conclusion, that the level of the sea cannot be considered permanent for all practical purposes when it may be shown that it might be disturbed by the operation of present causes during the period occupied by the construction of a single geological formation. Elevations and subsidences of the land or sea-bottom would also effect important changes in the height of the sea-level, sometimes counteracting and at others adding to the effects produced by the continuous operation of rivers, &c. The effects produced by these important causes would be an additional reason for not considering the sea-level permanent.

It is hardly necessary to add, that the continual waste of the earth's surface by the carrying of materials into the ocean by rivers and breakers particularly attracted the attention of Hutton. He considered* that this was counteracted by elevatory movements of the sea-bottom from time to time, but particularly mentions that it was not necessary to suppose that the dry land was equally extensive at all periods. Since the fluctuation in the sea-level would be directly consequent upon the destruction of land arising from the operation of rain, the atmosphere, and running water on its surface, such changes would be in harmony with the spirit of the Huttonian theory.

Part III.

The average thickness of the deposit formed on the sea-bottom by the solid materials brought on to it from all sources has been estimated in the preceding part of the paper at 3 inches in 10,000 years, producing an elevation of that amount in the sea-level in the same period. Some portion of the oceanic area may be supposed to receive no part of this supply, while other localities nearer the coast-line obtain a great deal more than the average. In the interval between these places, where the rate of deposit is extremely high, and those where it is extremely low, must lie an extensive tract of sea-bottom, where the accumulation of detritus does not much differ from the average rate, which we have supposed to be 3 inches in 10,000 years. Such localities may be more extensive near those parts of the ocean-bottom which receive no supplies of detritus whatever, but they must stretch up to the coast-line in many places. For instance, if it is supposed

* These remarks of Hutton are here introduced because he takes an entirely different view of this subject to that promulgated by Sir Charles Lyell, who considers that there has been always an excess of subsidence. (See Principles, 1850, p. 542.)

that a supply of 10 cubic feet of sand or mud is obtained from each foot of frontage of any coast-line, and distributed between high-water mark and 20 miles distant, it might raise the mean level of that portion of sea-bottom 1 foot in 10,000 years.

Rivers opening on to the shore might also bring down a still greater quantity of material; but although tides and currents are at work removing the sea-bed in one place and forming sedimentary strata in others from the old and new materials, there must everywhere be portions of every sea-bottom where the rate of deposit is intermediate between the highest and lowest, and may often not differ much from that of 3 inches in 10,000 years. These portions of the great oceanic area, wherever they may be situated, are particularly interesting, because on them the accumulation of sedimentary deposit is taking place without any change in the depth of water, and yet without necessitating the supposition of gradual subsidence of the sea-bottom.* Even where deposits are taking place much faster than the mean rate, the variation in the depth of water would be proportionately less than if the sea-level had been permanent.

The limited supply of detritus derived from cliffs, and the wide distribution of that from rivers, renders it difficult to imagine any very extensive tract of sea-bottom where the rate of deposit derived exclusively from new materials should many times exceed the average. Even on areas where extreme cases of denudation and deposition occurred (in periods when the sea-bottom was unaffected by movements, subsidence and elevation), there would be many parts where the condition of depth would remain unaltered, because on them the rise in the sea-level would compensate the addition to the sea-bottom. Also if, in periods that are past, the supplies of detritus from rivers and cliffs were many times greater than at present, they must have caused proportionately greater fluctuation of the sea-level, and therefore under such circumstances there would also be parts of the oceanic area receiving deposit at the same rate that the sea was rising. There would thus have been opportunities for the accumulation of sedimentary rocks without any change taking place in the depth of the water they were formed in, during the intervals when the sea-bottom was undisturbed by subsidences and elevations. For these reasons, in examining the section of a marine formation containing throughout the remains of the same species of Mollusca, it would require independent evidence to determine whether the equal depth of water indicated by the organic remains had been preserved during the formation of the deposit by means of

* The effect of these causes on the general depth of the ocean would be of little importance in a geological point of view, except for *an extended period of time*, such as must have elapsed during the construction of a great serial group of strata.

changes of the level of the sea-bottom, or that of the sea itself, or of both conjointly.

Great caution must also be requisite in judging of the time occupied in the formation of the older rocks from their mineral character, as the following description of passing events will also apply to periods that are long gone by.

Mr. Austen relates in one of his papers, that "with a continued gale from the west large areas of the dredging-grounds on the French coast became at times completely covered up by beds of fine marly sand, such as occurs in the offing, and which becomes so hard that the dredge and sounding-lead make no impression upon it: with the return of the sea to its usual condition, a few tides suffice to remove these accumulations."*

Mr. Deane, the submarine surveyor, also reported to the Institution of Civil Engineers, that the turn of the tide is felt as soon near the sea-bottom at a depth of 120 feet as it is at the surface; and he represents that the loose materials covering the Shambles Rocks are moved backwards and forwards with every tide.

With these facts before us, what criterion can there be (even by estimating the sources of the detritus) for arriving at the minimum or maximum rate at which sands and marls become permanent additions to the sea-bed? For the materials may present all the appearances of hasty accumulation, and yet the interval of time between the deposit of two strata of sand now contiguous may have been occupied by countless temporary deposits, as quickly brought and as quickly removed by the tide, and leaving no trace whatever of their existence. For the same reasons, we cannot be certain that in the valley of the Mississippi we have an unbroken sequence of fluviatile strata, in which the accumulations of one century form the base for those of the next, from the bottom to the top of the series; because there, as in marine formations, the deposits of one period may have been entirely removed in the next. It is therefore possible that many such movements may have occurred, and that the delta of the Mississippi may have occupied a longer period of time for its formation than could be computed from any data remaining. In the preceding part of the paper the conclusion was arrived at, without taking an extreme view of the rapidity with which the materials may have been collected for its deposition, that the work could not have been completed within a period for which the sea-level could be considered permanent.†

There must be, however, many rivers which are only able to afford very small supplies of mud to any alluvial formations, either

* Quart. Journ. Geol. Soc., vol. vi, p. 79.

† It is hoped that in the course of a few years enough data will be forthcoming to determine more nearly the importance of this variation of level in a geological point of view.

from deriving their water from lakes or from countries with a very small rain-fall. During a period when the gradual elevation of the sea-level was not counteracted by the effects of more powerful causes, there would be conditions near the mouths of some rivers of this kind for the surface of their plains to be gradually elevated by the operation of winter floods at a rate somewhat similar to that of the sea-level. In this manner purely fluviatile deposits might be formed in the neighborhood of the ocean, occupying positions similar to that represented in the lower part of the longitudinal section of the Mississippi, without the necessity of supposing any subsidence of the land. In the upper portions of such rivers, the periodical floods, assisted by the accumulation of terrestrial remains in the adjoining plains, would add stratum after stratum during periods when the surface of the country was unaffected by subterranean movements. It is probable that the rate of deposit might be accelerated in periods of subsidence; but the manner in which rivers form plains along their course in all countries under ordinary conditions, when no subsidence or elevation is occurring, was traced by Hutton.

Even if, in ancient periods, the rate of denudation was greater than at present, and the supplies of detritus to rivers more extensive, the fluctuations of the sea-level and the elevation of the beds and plains of rivers would have been proportionately greater. There would, therefore, still have existed some localities where the rate of the formation of alluvial plains near the sea kept pace with the elevation of the waters; so that, as at the present time, conditions would have existed for the accumulation of fluviatile strata containing terrestrial remains without any subsidence of the land. This is a subject, however, that must be further studied, more especially when its value is considered in relation to the great masses of fluviatile strata either of the Mississippi, the Ganges, the Nile, or the Po. For the above reasons it would be difficult to determine, when examining sections of thick fluviatile strata, whether these accumulations of detrital matter had been formed during subsidence of the land, or during the gradual elevation of the level of rivers and seas, arising from the continual operation of ordinary physical causes.

Art. XXVIII.—*On Fuchs's method for the determination of Iron;* by J. R. Brant.

To determine the amount of iron in any substance, Prof. Fuchs of Munich proposed (Erdmann, xvii, 160) a method which for ease and rapidity of execution is unequalled. It consists simply in boiling the solution of the perchlorid of iron with a strip of bright sheet copper, until the iron is reduced to protochlorid—

the reaction being $Fe_2Cl_3 + 2Cu = 2FeCl + Cu_2Cl$. The quantity of iron is then calculated from the loss of weight of copper, for according to the reaction, as the atomic weight of copper is to that of iron, so is the loss of weight of copper to the quantity of iron sought.

As the idea contained in the method is capable of many analytical applications, it became a matter of interest to determine first the accuracy of the method itself. The iron used in the analyses was fine piano forte wire, and as a preliminary experiment the copper, which was of the purest Lake Superior variety, was boiled for one half hour in concentrated chlorhydric acid, with a loss of 0·69 per cent., and in acid of about one quarter the strength, 0·11 per cent.; showing that the very dilute free acid, in the solution of the perchlorid, can have no sensible effect on the result.

	Iron taken.	Copper dissolved.	Iron found.		
I.	2·0074	2·2010	1·9441 =	96·84	per cent.
II.	2·0591	2·3065	2·0372 =	98·94	"
III.	1·9262	2·1811	1·9265 =	100·01	"
IV.	1·6682	1·8875	1·6671 =	99·93	"
V.	2·2574	2·5045	2·2121 =	97·99	"
VI.	2·0084	2·2855	2·0187 =	100·52	"
VII.	1·9807	2·2015	1·9445 =	98·17	"
VIII.	2·0671	2·3074	2·0380 =	98·59	"
IX.	2·0618	2·3380	2·0651 =	100·16	"
X.	1·0637	1·1907	1·0517 =	98·87	"

The analyses show that although the iron is reduced to protochlorid, the change in color of the solution does not indicate with sufficient sharpness the exact time when such reduction is complete, thus rendering the method inaccurate and unreliable. The method can be made to give accurate results, as soon as some unobjectionable process is given whereby the reduction is rendered manifest independent of mere change in color.

Since the above investigation was completed, a paper on this same subject has been published (Erdmann, lxi, 127) by Dr. Julius Löwe, in which he states that the method, in point of accuracy, leaves nothing to be desired, and gives as proof the following examples:

	I.	II.	III.	IV.	V.	VI.	VII.
Taken,	0·185	0·161	0·126	0·212	0·084	0·282	0·202
Found,	0·179	0·158	0·122	0·212	0·081	0·278	0·201
Difference,	0·006	0·003	0·004	——	0·003	0·004	0·001

Although in these analyses the absolute difference is very small, unfortunately the error in parts per cent. is large; expressed in this manner we have:

	I.	II.	III.	IV.	V.	VI.	VII.
Found,	96·75	98·13	96 82	100 00	96·42	98·58	99·50
Difference,	3·25	1·87	3·18	——	3·58	1·42	0·50

Löwe's own analyses prove then that in point of accuracy the method leaves much to be desired; while by his inconsiderate manner of stating his results he has deceived himself, and probably many others.

New York, June 18th, 1854.

Art. XXIX.—*On Stibiotrizincyle and Stibiobizincyle, two new compounds of Zinc and Antimony, with some remarks on the decomposition of water by the alloys of these metals;* by Josiah P. Cooke, Jr., Cambridge.

During some experiments on Antimoniuretted Hydrogen, made the last winter, I noticed that the alloys of zinc and antimony, which had been used for preparing that gas, continued to evolve gas from pure water, even after they had been washed completely free from the dilute acid employed in the process. This gas proved to be pure hydrogen, and on boiling the washed alloy with water, I found the evolution so rapid as to recommend the reaction as a process of preparing hydrogen in a state of purity. This fact was announced at the last meeting of the American Association for the Advancement of Science; the new process of preparing hydrogen described, and proofs given of the purity of the gas thus obtained.

On investigating this unexpected reaction, I found that not only the alloys of zinc and antimony, but that also pure zinc would decompose boiling water, before they had been acted on by acids. The following table will show the amount of this decomposition. Column headed 1 gives the number of centimetres cubes of hydrogen obtained by boiling 200 grammes of different alloys (granulated) with water. The per cent. of antimony contained in the alloys is given at the left hand side of the table, opposite to the number of centimetres cubes. The composition is known only synthetically. The alloys were made by melting together the zinc and antimony of commerce in the required proportions, making no allowance for impurities. The zinc used was shown by analysis to be almost pure; the antimony was a good article of commercial antimony which contained rather over one per cent. of impurities. The antimony contained in the alloys is therefore to be rated at somewhat less than that given in the table according to the per cent. of antimony which the alloys contain. The two metals having been accurately weighed out, were melted together in clean crucibles and the alloys granulated as nearly as possible under the same conditions. Two hundred

grammes of each alloy were boiled with pure water, the gas collected over water, and the number of centimetres cubes evolved in an observed time read off after the gas had been cooled to 20° C. These amounts were afterwards reduced for ten minutes, and thus reduced are given in the table. As it was impossible to obtain the granules of a uniform size in all the alloys, another set of experiments was made in precisely a similar way except that the alloys were cast into small cylinders of a uniform size. As these cylinders had absolutely the same diameter, and almost the same specific gravity throughout, the same amount of surface was obtained by weighing out 200 grammes of each alloy, and taking care to have the same number of little cylinders in each lot. Column 3 gives the results of these experiments, where of course the same correction for impurities in the antimony must be made in the composition of the alloys. It will be seen that the two sets of numbers compare as closely as could be expected, it being remembered that the amount of surface in the first set of experiments was variable, while that in the second was constant and smaller than the first. These results however must be regarded only as approximations to the truth. The limits of variation in different experiments on the same alloy would quite cover the differences between the first ten numbers of column one, excepting the first, so difficult is it to granulate the alloys to a uniform size, and submit them during the experiments to precisely similar conditions. The numbers of column 3 from which the variations due to difference of surface have been eliminated, are probably relatively to each other very nearly correct.

Table of the Amounts of Hydrogen Gas evolved by 200 *grammes of different alloys of Sb and Zn, in ten minutes, at* 100° *C. measured at about* 20° *C.*

Per Cent. of Sb.	1.	2.	3.
0	2	63	
5	6	34	
10	4	28	3
15	4		
20	6	18	5
25	4	19	
30	4	31	5
35	5	49	
40	6	72	7
45	5	45	
50	8	44	9
55	17	46	
58	130	244	84
60	50	139	47
65	14	35	
70	10	45	7
75	6	36	
80	5	23	6
85	4	20	

A mere glance at the table will discover two facts:

1st. That up to 50 p. c. no great increase in the amount of hydrogen evolved is obtained by increasing the amount of antimony in the alloy.

2nd. That at the alloy containing 58 p. c. of commercial antimony, or about 57 p. c. of pure antimony, there is an immense maximum which is confined between at most two per cent. on either side.

Before passing to the result to which the last of these facts directly points, I will briefly state the few additional facts which I have observed in regard to the decomposition of water by the antimony alloys.

It is a well known fact that the rapidity of the evolution of hydrogen from dilute sulphuric acid and zinc can be very greatly increased by adding to the materials a few drops of a solution of chlorid of platinum. The platinum being immediately deposited on the zinc, forms with it a galvanic pair, and thus increases the affinity of the zinc for oxygen. The same increased action can be produced by the same means in the decomposition of pure water by the antimony alloys. Column 2 of the table gives the results which were obtained by boiling with pure water in a small flask 200 grammes of the granulated alloys, previously treated with the same amount in each case of a solution of chlorid of platinum. After the platinum had been deposited on the granules and the surfaces had been thus blackened, the alloys were thoroughly washed with water and the experiments conducted as in the other two cases. These experiments were made with the same alloys as those from which the numbers of column one were obtained. As however in the experiments with chlorid of platinum new and obvious causes of irregularity were introduced that did not exist in the other two sets of experiments, no great uniformity can be expected on comparing the results. The two main facts however noticed in columns 1 and 3 of the table are quite as prominent in column 2, and also the additional fact that the presence of platinum very greatly increases the rapidity of the evolution of hydrogen from the alloys.

One set of results given in the table requires particular notice; those obtained from pure zinc to be found on the first line opposite 0 p. c. of antimony. It is stated with great confidence by all chemical authors who have written on the subject that Zn does not decompose water at the boiling temperature. On this account the experiments with pure zinc were made with peculiar care and repeated several times, great pains being taken to ensure that both the zinc and water employed were perfectly pure. There is no doubt in regard to the fact of the decomposition which becomes, as is shown in the table, quite rapid when the affinity of the zinc is strengthened by the galvanic action of the platinum.

When the alloys of zinc and antimony are treated with strong acids, hydrochloric or sulphuric, they are as a general rule, and under favorable circumstances, completely deomposed, the zinc uniting with the acid and the greater part of the antimony separating as a black powder, only a very small amount ever, even under the most favorable circumstances, escapes as antimonuretted hydrogen. When the alloys are in granules it is almost invariably the case with those which contain more than 50 per cent. of antimony that after a short time the acid ceases to act, owing to the formation of a coating of antimony on the surface. The action is of course renewed on reducing the alloy to powder, but here as in other alloys, the less oxydizable metal appears to be able to protect entirely a certain amount of the other from the action of acids.

These facts in connection with those previously stated in regard to the increased action of the alloys on water in presence of platinum sufficiently explain the remarkably rapid decomposition of water obtained by means of alloys which have been previously acted upon by hydrochloric or sulphuric acids, even after the excess of acid and the salts formed have been completely removed by repeated washings. This decomposition is so rapid that I have obtained from 200 grammes of an alloy containing 58 per cent. of antimony prepared as just described and boiled with water, nearly a litre of gas in ten minutes. It is plain that the antimony acts here exactly as the platinum in the previous experiments by forming a galvanic circuit with the alloy. A set of experiments was made with alloys which had been acted upon by acids similar to those the results of which are given in the table. The irregularities however which resulted from the unequal action of the acids on the different alloys, from the differences of surface and from other causes rendered the final results so discordant that they were of no value for comparison. They were always much greater than those obtained by using platinum, with the exception of pure zinc, whose decomposing power was not increased by the action of acids.

This new mode of decomposing water is of value as a process for preparing pure hydrogen, and also for illustrating the composition of water to a class. When the antimony and zinc used are free from arsenic, and the water not in actual ebullition, the hydrogen obtained is chemically pure. If commercial antimony and zinc are used, the gas will be found contaminated with a small amount of arseniuretted hydrogen, so small however as to be with difficulty detected, and entirely inappreciable in the most refined eudiometric experiments.

Gas evolved from an alloy containing 50 per cent. of commercial antimony was burnt in Regnault's eudiometer with the following results:

Tension of hydrogen used, - -	0·379	metres.
Tension of hydrogen + oxygen, -	1·219	"
Tension after combustion, - -	·653	"
Tension of gas consumed, - -	0·566	"

$0{\cdot}566 \times \frac{2}{3} = 0{\cdot}378$, amount of hydrogen consumed.

My mode of preparing the alloy for making hydrogen is simply thus: I melt together equal parts of zinc and antimony (this alloy being nearly or quite as active after having been treated with acid as the 58 p. c. of antimony) and granulate as finely as possible. I place the granules in a deep porcelain basin, and pour over them enough hydrochloric acid of ordinary strength to cover them. An energetic action ensues, which I allow to continue until it becomes weak and the acid nearly exhausted. The excess of acid and also the chlorid of zinc formed, I now wash away by allowing a stream of water to pour into the basin until it runs off clear and tasteless. The alloy thus prepared is ready for use. It will evolve hydrogen from boiling water with almost as much rapidity as zinc and dilute sulphuric acid will in the ordinary process, and even after the temperature of the water has fallen to that of the air, the evolution continues though only very slowly. A flask containing about a pound of the prepared alloy covered with water, continued to evolve hydrogen during the last winter for over two months, where the temperature was seldom above 4° C.

The rapidity of the evolution of hydrogen from the alloy and boiling water diminishes quite rapidly and finally after several hours ceases altogether from the formation of a coating of oxyd on the surface. The activity of the alloy can be restored by dissolving off this coating with dilute acids; where however the alloys contain a large per cent. of antimony (above 50) the activity can not be renewed indefinitely in this way since the particles of antimony set free by the acid adhere to the surface of the alloy and soon form a coating impregnable to the strongest acids. For these reasons this process will not be found economical for preparing hydrogen in large quantities, although I think it will be useful where the gas is desired chemically pure.

The large maximum which was observed in the table opposite 58 p. c. of antimony indicated the existence at that point of a definite compound. The true composition of this alloy, considering the impurities of the antimony, was nearly Zinc 43 p. c., Antimony 57 = 100, which corresponds almost precisely to the symbol $Sb\,Zn_3$. The compound which this symbol represents I will term, following the analogy of the nomenclature adopted by the German chemists for similar compounds,

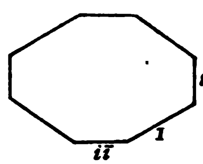
Stibiotrizincyle.

It can be obtained by melting together 58 p. c. of commercial antimony and 42 p. c. of zinc, and allowing the liquid mass when thoroughly melted together, to cool until a crust forms on the surface. On piercing through this crust and turning out the still liquid alloy, the crucible if broken open when cooled will be found filled with the most beautiful prismatic crystals. In order to obtain crystals of any size, it is necessary to use eight or ten pounds of the alloy, and cool the crucible very slowly in sand. These crystals present the following properties. The isolated crystals are small, a few tenths of a line only in diameter and not generally over an inch in length. They tend however to form compound crystals with parallel major axes which are often several inches in length and a quarter of an inch in diameter. Naturally they present a silver white color and a beautiful metallic lustre. The surfaces are often however iridescent, owing to a slight oxydation, and the true color is then only seen on the fracture. Sp. Gr. of crystals = 6·48, Homer.

Their form is that of a rhombic prism, with sometimes only one, but generally with both sets of edges truncated. A section through the lateral axes is given below with the angles between the planes of the prism. The crystals invariably, so far as I have observed, run out to fine points, and although I have examined many hundreds of these crystals, I have never seen one with a termination.

I on $i\bar{\imath} = 148° 30'$
I on $i\breve{\imath} = 121° 30'$
I on I over $i\bar{\imath} = 117°$

1.



I have observed variations from the angles given above on crystals of the same crystallization amounting to ten minutes. The angles given measured the same to a minute on crystals from three different crystallizations, and are therefore regarded as the most probable.

The composition of the crystals obtained as above was found on analysis to correspond very closely to $Sb Zn_3$. Of three analyses made by myself of crystals from different crystallizations, obtained by melting together 58 per cent. of commercial antimony and 42 per cent. of zinc, the greatest difference between either the zinc or the antimony found and that required theoretically was five-tenths of a per cent. If we assume that the atomic weight of zinc = 32·53, and that of antimony = 129·03, which accords with the best recent determinations, then the composition of $Sb Zn_3$ is as shown in column 1 below. Columns 2, 3, and 4 give the results of the three analyses just mentioned:

	1.	2.	3.	4.
Antimony,	56·94	57·24	56·50	56·93
Zinc,	43·06	42·83	43·06	43·15
	100·00	100·07	99·56	100·08

There can be no doubt therefore that an alloy which contains 57 per cent. of antimony and 43 per cent. of zinc will give crystals which have a composition corresponding to $Sb\,Zn_3$.

It was found however that the same prismatic crystals could be obtained from melted alloys which contained proportionally a much larger amount of zinc, but not from those which contained less. As the amount of zinc in the alloy was increased, the crystals became less and less abundant, until they seemed to fade out when the amount had been increased to about 84 per cent. A series of analyses were made in order to ascertain how far the composition of the melted alloy influenced the composition of the crystals which were formed in it. The results of these analyses are given in the following table. In the left hand column are given the per cents. of zinc in the alloys from which the crystals crystallized. In the right the per cents. of zinc found in these crystals on analysis. With a few exceptions in these analyses the zinc only was determined. The zinc per cents. marked with my name however, are from complete analyses.

Per cent. of zinc in the alloy.	Per cent. of zinc found in crystals.	
42 per cent.	43·09	*Cooke.*
43 "	44·14	*Cooke.*
44 "	44·26	*Eliot.*
46 "	46·77	*Eliot.*
48 "	48·66	*Eliot.*
50 "	46·89	*Cooke.*
52 "	47·28	*Homer.*

Here as in the former table the composition of the alloys is only known by synthesis, and as commercial antimony was used for making them, the per cent. of zinc in the left hand column is nearly one too low in each case. Considering this it will be seen that the crystals have the same composition as the alloy up to 49 per cent. of zinc, but that after having taken up 6 per cent. excess of zinc, they seem incapable of taking up any more, so that when the per cent. of zinc is further increased in the alloy, it falls off in the crystals, and the alloy of 53 per cent. of zinc gives crystals which contain one per cent. less zinc than those obtained from alloy of 49 per cent. It is unnecessary to say that well defined crystals were always selected for analysis.

Crystals were measured from most of the different alloys of the last table, and were found to have the same form considering the variation already noticed, as the one figured and described. I

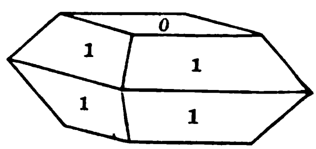
was not however able to obtain crystals from alloys either of 49 p. c. or 53 p. c. of zinc, whose angles could be accurately measured.

This result is certainly very remarkable and important in its theoretical bearings, and will be the more so should it be found that the same variations in composition appear in the compounds. Until I have investigated these I shall refrain from advancing my views on the subject. The facts just stated are substantiated by a very large number of analyses and measurements besides those which appear in this paper.

Stibiobizincyle.

This compound may be easily prepared like the last by crystallizing an alloy containing about 33 per cent. of zinc, and 67 per cent. of antimony. In its natural state like Stibiotrizincyle, it has a silver white color, and a very bright metallic lustre, often however its surfaces display prismatic colors owing to oxydation. It forms in right rhombic octahedrons with basal planes of the Trimetric System. Here as in the other crystals, I have observed variations in the angles amounting to 20 minutes between the extremes. The crystals are frequently very perfect and their faces so plane and bright, that the angles can be measured to a minute. The angles given were all obtained by measurement, except the one over X, which measured six minutes more than that required by the other two. These angles are nearly the mean of those observed.

2.



O on 1 = 122° 15′ measured on each side.

1 on 1 over Z = 115° 30′, measured.

1 on 1 over Y = 118° 24′, measured.

1 on 1 over X = 95° 24′, measured 95° 30′.

Axes $a = 1$, $b = 1{\cdot}042$, $c = 0{\cdot}793$.

These crystals were analyzed by Mr. Eliot with the following results:

	Analysis.		Theoretical Sb Zn_2.
Zinc	= 32·52	Zinc	33·55
Antimony	= 66·86	Antimony	66·45
	99·38		100·00

I have now given an abstract of the results on these two new compounds which I have obtained up to this time. I am now engaged in investigating their chemical relations and compounds. The results of this investigation I hope to be able to publish during the Autumn, in the form of a memoir, to which I must re-

fer for the details and proofs in relation to many points which have been stated in this paper. In concluding, I would express my warmest thanks to Mr. Charles W. Eliot, Tutor in Harvard College, and Mr. Charles S. Homer, assistant in my laboratory, for their assistance and zeal in prosecuting the investigation. Their names have already appeared in the course of the paper.

Art. XXX.—*On the Nature of Forces;* by Lieut. E. B. Hunt, Corps of Engineers, U. S. A.*

It is a peculiarly significant fact that all the great agencies of Nature which act from local centres or origins through sensible distances, follow the Newtonian law of variation in intensity with an inverse duplicate function of the distance. In this respect, light, radiant heat, sound, gravitation, electric and magnetic repulsions and attractions all agree. As the so-called forces of cohesion, elasticity, chemical affinity, electrolysis, crystallization, capillarity, friction, &c. exhibit sensible actions only at insensible distances, their laws of variation in intensity with varying distances of action are incapable of direct determination. We do not even know if these agencies are original and primary, or resultant and secondary forces. As inertia acts only in the ultimate units of matter, it cannot be supposed to be at all connected with distance. The mutual action of galvanic currents, in which both distance and inclination affect the attraction or repulsion, is clearly a complex result of the transmissive motion of the currents, and forms no static exception to the Newtonian law. It seems therefore a general fact that all primary natural agencies, which act from central origins through sensible distances, are embraced under one mathematical formula, which is that expressed in the all-embracing Newtonian law.

What then is the rational translation or philosophical significance of this law? To this question one answer may be given, which fully illustrates that crowning simplicity so uniformly characterizing natural facts of the highest generality. Newton's law rigorously expresses the necessary facts of simple outward emanation or procession from a central origin. Whatever case of agency emanating from a centre we may suppose, whether it be light, radiant heat, sound, force, or any other, this fact of emanation makes the Newtonian law an inevitable result. This law simply expresses the fact that the agency in question undergoes neither increase nor diminution by outward transmission. If any other than the inverse duplicate ratio be supposed, it must involve

* Read before the American Association for the Advancement of Science, at Washington, May, 1854.

either an increase or diminution of the aggregate agency, as consequent on mere transmission through space. But it is clear that mere transmission is totally incapablé in itself of affecting in the slightest degree the quantity of action originally put forth from the centre. Mere change of place cannot, by its very nature, be a producing or destroying cause. The inertia, the structure, the imperfect elasticity of the transmitting medium, may produce a decay of transmitted action, as in the case of light in an imperfect medium or of heat in air; but mere transmission as such, is as wholly powerless to destroy as it is to create action.

The more clearly to perceive that unresisted central emanation necessarily gives the Newtonian law, let us conceive a centre from which action of any kind issues or emanates by rectilinear radiation. Each ray throughout its entire length is the representative of the same quantity of action. Now if we suppose elementary concentric spheres around this origin as a centre, each sphere is pierced by all the rays and hence all the spheres become *loci* of the same amount of total agency in a given time. If the emanation or radiation be supposed uniform in all directions, then the total intensity of action on each unit of surface for any particular sphere, is inversely as its total surface, which is as the square of the distance of transmission. Hence the action on a given surface, or a given constant mass, is inversely as the square of the distance of transmission. Or, instead of rays, we may suppose the emanation to proceed by spherical undulations, where a like train of reasoning will lead to a like result. The two mechanisms of radiation and of spherical undulation concur in giving the Newtonian law, as the necessary expression of unobstructed emanation, the law being indeed but a simple assertion that the emanating agency is neither increased nor diminished by outward propagation or that translation through space, neither makes nor destroys light, heat, force, &c. The same facts in a negative order would characterize a central absorption of agency.

Since free emanation thus leads to the Newtonian law as a necessity, the reverse question arises: whether the existence of the Newtonian law does not of necessity involve emanation? It surely furnishes a powerful evidence of emanation, but is not a positive proof of it; for we can suppose the exact geometrical system of dynamic agency which emanation produces, to be by the original creation and constitution of matter, embodied in an identical static form. For instance, we may suppose an atom so constituted as to fill with its actual and organic self all the space to which its force action would extend, and thus to have everywhere a potentiality identical with that resulting from true emanation. This hypothesis literally makes each atom fill all space, and all atoms actually to coexist in each point of the universe. Thus too, if one atom be moved by its centre, it must every-

where move through every other atom. We are mathematically compelled either to admit this strange universal coexistence of all atoms in each point of space, each atom being everywhere truly distinct, or else to ascribe to an emanation from central points or nuclei, all the forces which follow the Newtonian law. But the exceeding improbability of this coexistence theory may best be estimated by inquiring into the chance of an original static creation being based on the inverse duplicate ratio. *A priori*, this particular ratio has no preeminence of probability over any *sub* or *super* ratio of variation. We may indeed ascribe the existence in Nature of this exact ratio to an intelligent Divine choice or selection, but as a question of chances, it is as infinity to one that some other ratio would have existed.

Between these two conceptions, each of which is a geometrical possibility, this consideration of chances almost compels us to choose the idea of emanation. When too we consider the exceeding complexity of mechanism and the great metaphysical difficulties involved in the idea of coexistence, and when we observe that the inertia of each atom appears thus to be diffused through the entire universe, the coexistence theory seems a hypothesis of the least promising character. For these reasons the existence of Newton's law in any type of force or other agency, seems legitimately and almost by constraint to be referable directly to the emanative outward transmission of the force or agency from its originating central points or nuclei. As all known primary forces do in fact follow this law when acting through sensible distances, the inference follows that all these forces are actually emanative.

But are all primary forces necessarily emanative? Certainly not: yet we ought not, except as a last resort, to hypothecate forces *not* emanative, as such a hypothesis is unwarranted by our actual knowledge. All such hypotheses involve generic force types, unlike that one which includes all forces whose laws are really known to us. Before assuming primary forces, varying inversely with the 1st, 3d or 14th powers of the distance, we are bound to exhaust all the resources for phenomenal interpretation, offered by forces following the Newtonian law. To assume that the same primary force is attractive at one distance and repulsive at another is like saying that yes becomes no by a change of latitude. The expedient of leading one primary force through various alternations of attraction and repulsion, as is apparently done in the theory of spheres of force, must to a reasoning mind appear too conveniently Protean and time-serving to be accepted as any thing better than the fig-leaf of our ignorance. We really know of but one type of force, and that one has a law which means emanation; yet speculation has run riot among all possible ratios of force decrease, and the *force entity* has been treated as a

shuttlecock between attraction and repulsion, just as present convenience dictated. We must have a more grand and simple idea of force, ere the labyrinth of molecular mechanics will yield its clue. In molecular studies, there is a strong and widespread tendency to complex hypotheses which but ill accord with the fundamental simplicity of Nature, and which by hiding our ignorance, effectively retard our progress towards knowledge. To exorcise this tendency would greatly promote the consistent extension of strict mechanical investigation over the rich fields of molecular constitution.

With a view to developing the principles now presented and as a preliminary to some discussion of the theoretical views advanced by Boscovich and Faraday, I will here proceed to develop a few of the properties of central forces varying with an inverse function of the distance, and which may be either emanative or static by coexistence.

Assume a centre of force (or other agency) at an origin of rectangular coördinates, and conceive the force to be radiated uniformly in all directions, each ray being in its entire length the representative of a constant intensity of action, or of an agency varying in intensity with any inverse function of the distance. This mechanism must it is evident, give results identical with those which would result from a corresponding spherical wave mechanism. Suppose now a circular disc to advance or recede relative to the origin, by being moved along the axis of X by its centre and being maintained perpendicular to it; the reception of rays by this disc will be a measure of effect so long as the obliquity of these rays can be disregarded. Calling the force or aggregate action y, when the disc is at the distance x from the origin, and y' when it is at the distance unity; we have $y' : y :: x^2 : 1$, or $y=\frac{y'}{x^2}$. If now we conceive each ray as having an intensity varying with a simple inverse function of x, we shall obtain $y=\frac{y'}{x^n}$, in which n exceeds by two the exponent of variation along each ray.

If we differentiate the equation $y=\frac{y'}{x^2}$, regarding y as a function of x, we obtain $dy=\frac{-2y'dx}{x^3}$, and this is the force decrement corresponding to the Newtonian law. This can best be appreciated by deriving it directly. Let the disc advance towards the origin through a distance $=dx$; the increase of ray reception by the disc, or the differential of the force is found by determining the elementary ring projected around the former position of the disc. Calling the disc radius r, and the width of the added ring dr, we shall have by proportionality $x : -dx :: r : dr$,

and $dr=\frac{-rdx}{x}$, also $y:dy::\pi r^2:\pi(r+dr)^2-\pi r^2$. Hence by reduction and by neglecting dr^2 as an infinitely small quantity of the second order, we obtain $dy=\frac{-2ydr}{r}=\frac{-2ydx}{x}=\frac{-2y'dx}{x^3}$, which is the expresssion above found as the differential of a Newtonian force. The signs of dx, dy and dr depend on the direction of the motion of the receptive surface.

It will be seen by inspecting the above, that not only is Newton's law derived from this consideration of ray-reception, but that the differential equation of that law expresses simply the relation between the differential of distance and that of ray reception. This law also involves the two assumptions which for all appreciable distances are entirely admissible, though not at all so for extremely small distances: first, that the effect of ray obliquity for the same receiving disc may be neglected, and second, that the diffusion over the disc may be regarded as uniform. By substituting spherical atoms for the disc, we at once obtain the case of nature, when the question is of actions between sensible masses.

If we construct the curve of the equation $y=\frac{y'}{x^2}$, we find that both axes of coördinates are asymptotes to its branches or that the force ordinate is infinite at the origin and zero at an infinite distance. But the most remarkable fact in this class of curves is that each particular radius of receiving disc corresponds to a particular curve. When the distance abscissa equals the disc radius, the differentials of force and distance are always equal in construction. The law of the increment is only satisfied by that curve which cuts the line through the origin making an angle of 45° with the two axes, in the point whose ordinate equals the radius of disc. Thus the radius of the receptive disc or atom determines the particular characteristic curve of relation between force and distance: a curve which is the same for a homogeneous sphere of atoms as for a single component atom.

Passing to the more general function $y=\frac{y'}{x^n}$, and differentiating, we obtain $dy=\frac{-ny'dx}{x^{n+1}}$, in which if we substitute y for $\frac{y'}{x^n}$, we obtain $dy=-ndx\frac{y}{x}$. If we suppose now that all the curves corresponding to any particular value of n are duly constructed, and if a right line through the origin make the angle x with the axis of x then $\frac{y}{x}=$ tang. $\alpha=$ a constant; or in $dy=-ndx\frac{y}{x}$, we

find a constant of force increment, for all the points in which a straight line from the origin cuts the various curves of the system. The significance of this result is obvious when we consider that radiation gives shape to the formula. By farther discussion, it would be seen that the increment for a given ordinate varies inversely with the abscissas and directly with the ordinate for a given abscissa.

If the series of parallel curves corresponding to $y=\frac{y'}{x^2}$ be constructed for all values of y' from *plus* infinity to *minus* infinity any possible attraction or repulsion curve for which the force varies as $\frac{1}{x^2}$ will coincide with some one of this series. No two curves of this series when referred to the same origin and axes can be made to intersect. This property is general for all central force curves, in which y varies as $\frac{1}{x^n}$ taken in sets for each value of n from zero to *plus* infinity. This signifies that if any number of central forces acting from the same centre according to the same law are in equilibrio at one point, they must be so at all distances. Also if any number of attractions and repulsions act from one point as $\frac{1}{x^n}$, their resultant also acts as $\frac{1}{x^n}$. Thus

$$y_{a'}+y_{a''}+y_{a'''}+\text{ \&c. } -y_{r'}-y_{r''}-y_{r'''}-\text{ \&c. } = ya \text{ or } yr = \frac{y'_{a'}+y'_{a''}+y'_{a'''}+\text{ \&c. } -y'_{r'}-y'_{r''}-y'_{r'''}-\text{ \&c.}}{x^n} = \frac{y'a \text{ or } y'r}{x^n}$$

which is of the original form. Hence all forces emanating from a centre and varying as $\frac{1}{x^2}$ are equivalent to a single resultant varying as $\frac{1}{x^2}$ which is wholly attractive or wholly repulsive: and the same for each value of n. Hence a medium composed of one species of atoms endowed with forces varying according to Newton's law must be unalterably compacted or unalterably elastic. Therefore a single type of atoms with Newtonian forces cannot represent the facts of nature, and we must either suppose other forces than those varying as $\frac{1}{x^2}$ or we must suppose more than one kind of matter.

If we suppose an atom to exercise two central forces, one attractive and one repulsive for which n has different values, their curves will intersect or the forces will balance at one and only one distance. Thus if the ordinary attraction, $y_a=\frac{y'_a}{x^2}$ and any

repulsion $y_r = \frac{y'_r}{x^n}$, act from the same centre their resultant, $y_a - y_r = \frac{y'_a x^{n-2} - y'_r}{x^n}$, can only be zero when $x =$ infinity, or when $x = \sqrt[n-2]{\frac{y'_r}{y'_a}}$. In general $x = \sqrt[n'-n'']{\frac{y'_r}{y'_a}}$, is the abscissa or radius of the point or sphere of equilibrium of these two forces. Hence such a primary attraction and a primary repulsion acting between simple atoms, can together give but one type or form of equilibrium, and thus must fail to give the solid, liquid and gaseous conditions of aggregations. Besides two forces involving different laws of action or values of *n*, can in no wise give the simple Newtonian form as it appears in gravitation. From this we can say with confidence that heat or the interatomic repulsive force is no mere radial repulsion varying as $\frac{1}{x^n}$.

To suppose three or more distinct atomic forces varying as $\frac{1}{x^{n'}}$, $\frac{1}{x^{n''}}$ and $\frac{1}{x^{n'''}}$, or to suppose a single force following no simple functional law, but being now repulsive, now attractive, now infinite and again Newtonian, is to give ourselves up to bewilderment and to achieve a chaos of explanation. Simple emanation and its resulting law bear *a priori* credentials of being the great facts of dynamic nature, and until they are shown to fail entirely in the exposition of molecular mechanics, a resort to other conceptions must be considered illogical and unhopeful. The hypothesis of two distinct species or kinds of matter, with original forces peculiar to each, but all following the Newtonian law, is far more promising than any other, and Mossotti has to some extent shown its ample power of explanation. One important element is overlooked in his analysis, nor has he developed the mechanical effects of heat on aggregation, though in this respect his views are a vast advance on the total neglect of heat by Boscovich's theory—as also on the expositions by Poisson.

From the point which this discussion has now reached, I wish to examine somewhat the theory of Boscovich and the speculation of Faraday, freely stating some objections not hitherto urged.

Boscovich by denying size to atoms, prevents them from presenting any material surface or volume on which to receive force action. If forces emanate from the atoms of a mass *A*, and penetrate the volume of a mass *B*, the atoms of the mass *B* would not according to the Boscovich theory, present any surface or volume whatever on which to receive the force action. How then is the Newtonian law, or indeed any action to be derived? As this law but expresses the condition of ray-reception, what

does it mean when this reception is precluded by the total lack of magnitude in atoms? Nothing remains but to conceive the force rays or their equivalents from the atoms in *B* as every where receiving the action of the intersecting rays from the atoms in *A*, and referring these actions back to their own centres. Now unless these rays are conceived as possessing magnitude, and as actually filling all space, the result of ray-intersections, depending as it must on the number of intersections, would not be the exact Newtonian law, but one essentially departing from this, by a difference which increases as the rays from each atom are supposed less completely to fill all space. Thus to obtain the Newtonian law, we appear to be driven again to that strange hypothesis of each atom filling all space and all atoms coexisting in each point, and to require still other special conditions; all simply as a consequence of denying size to atomic nuclei. As the power of receptivity must exist either in atoms of finite size or in atoms of infinite size, and as we must either locate inertia in a nucleal atom or in an infinite one; we seem quite justified in preferring emanation from and reception by definite nucleal atoms to the bold hypothesis of a static entity, activity and inertia belonging to infinite coexistent atoms. If we attempt to conceive a material mass, as a wall for instance, according to this coexistence theory, we shall find it signally inadequate to the realization of facts to the mind, which though not a logical objection, is a serious practical drawback. But by conceiving atoms as solid, impenetrable, definite volumes, from which force incessantly emanates, and by which force is incessantly received, the mental difficulties wane away, and matter becomes to the mind a localized reality. However small the atomic volumes be assumed, so long as they have a real and finite size, a receptive capacity and the Newtonian law result at once.

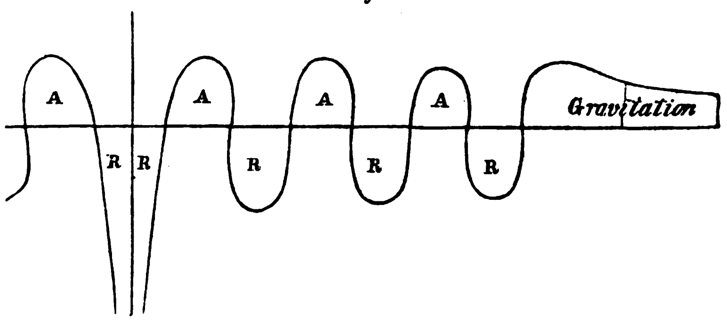
If I rightly apprehend Faraday's views (Phil. Mag., Nos. 157 and 188), they are such as would give a law quite different from Newton's. The interactions of rays conditioned as he supposes, could only give the actual result by so extending the amplitude and number of rays as that all points of space should be points of interaction between the rays of each atom and of all other atoms which is the coexistence theory again. To deduce the actual law from the views so modestly set forth by this excellent investigator, would, I think, be a mathematical impossibility; to say nothing of their inadequacy as they now stand, to serve the cause of molecular mechanics. The objection to the views of Boscovich and Faraday on the ground of their not providing for inertia has been well urged by Airy (Phil. Mag., No. 190).

There is another signal fault of the Boscovich theory, which at this time is peculiarly objectionable. While its mechanism is empirically devised with special reference to the solid, liquid and

gaseous states of aggregation, it really takes no account of heat, but at once refers these states to primary forces assumed for the purpose. Yet it is certainly the degree of heat, and that alone, which in fact mainly determines these forms of material existence. If any relation is supposable between heat and the force spheres of Boscovich, it remains to be discovered what it may be. But as the theory now stands, heat is ignored, and force spheres usurp the work actually performed by heat. This theoretical false causation is a positive stumbling-block in the way to clearer views of heat and molecular aggregation. The more we reflect on the wide range of actions due to heat, the more incompetent to their representation will we find the conception of primary spheres of force. In nature, aggregation is actually almost absolutely ruled by temperature, yet nothing at all like temperature seems legitimately derivable from the Boscovich theory. This theory indeed requires a mass of any given substance when not compressed, always to fill exactly the space given by its atoms being in positions of mutual indifference, and does not provide for expansions and contractions through heat variations.

I will now state a striking proof that the views of Boscovich have received their present somewhat wide acceptance among men of science, without being subjected to that criticism, analysis and comparison with nature, which alone can entitle them to any authority or weight. There is a glaring fundamental oversight involved in his theory as it now stands, and as it is figured in that exponential curve so frequently found in works on this subject, and even in mechanical text-books. This oversight con-

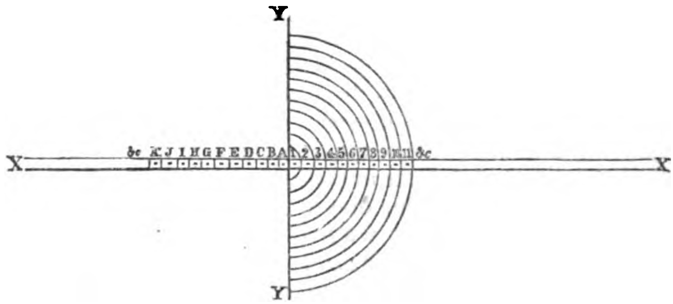
1.—*Curve of Boscovich.*



sists in his not having correctly extended his theory to masses of matter, which, unfortunately for his theory, is the only case occurring in nature. Taking his exponential curve of force between two atoms as it stands, and discussing a mass composed of such atoms, it appears that instead of the various types of aggregation and force manifestation hitherto supposed to result, there will be but a single cohesive type, which will be invariable for each mass.

To illustrate this, let us assume along the axis of X, (fig. 2), a line or thread of atoms, at mutual distances corresponding to a

2.



particular solid or liquid, and acting on each other according to the Boscovich force curve: call the atoms to the left of the origin A, B, C, D, &c., and those to the right, 1, 2, 3, 4, &c. In Vol. I. of Robison's Mechanical Philosophy, there is an exposition of Boscovich's theory, to which most of his disciples are indebted for their acquaintance with his views, and in this, the widest limit of cohesion is fixed at about one-thousandth of an inch, within which are several alternations of attraction and repulsion branches. Now by comparing this distance with the almost infinitely minuter threads, membranes, eye-points, &c. revealed by the microscope in infusorial and other organic forms, or with any of the countless facts showing the extreme divisibility of matter and the differential character of interatomic distances, it will become evident that many thousands of atoms lie within this outer limit of cohesion, or in the one-thousandth of an inch measured in a mass. Boscovich in his Theoria leaves the case essentially in the same condition. Hence the atoms A, B, C, D, &c., on the left of the origin act on 1, 2, 3, 4, &c. on the right of the origin, the attractions and repulsions alternating through many thousands of atoms on each hand. The atom A repels or attracts 1, 2, 3, 4, &c. according to distance, all atoms exterior to the last limit of cohesion being attracted. Thus in the force curve for A, many atoms will be found to correspond to each branch. We may therefore, with almost perfect accuracy regard the areas between the several branches and the axis of X, as proportional respectively to the corresponding aggregate actions of A on 1, 2, 3, 4, &c. The action of B on 1, 2, 3, 4, &c. is analogous, and so of C, D, &c., a portion of the curve towards the origin being cut off for each receding atom. Now on examining the areas between the different branches of the curve as drawn in Boscovich's Theoria, Robison's Mechanical Philosophy, Daubeney's Atomic Theory, Bartlett's Mechanics, &c., it will be seen that the outermost area, corresponding to gravitation, is very much greater than that for

the adjacent repulsion branch, and that the successive attraction and repulsion branches embrace about equal areas. Hence the attraction between A and the gravitating atoms in the line is decidedly greater in its aggregate than the sum of the adjacent repulsions; as is amply realized when we consider that the attraction area extends to infinity between the curve and the asymptote axis. Hence the action of A would be to draw the gravitating part of the column towards itself with a much greater force than it repels the adjacent portion, so that a large surplus of attractive pressure is passed along the column to the next attractive branch, which is thus made greatly to surpass the next repulsion and so on through the whole curve, until the interior repulsion is reached, where the aggregate attractive surplus is balanced by the final indefinite repulsion. The action of B, C, D, &c. is entirely similar, except in the successive pruning of the inner extremity of the curve. Hence the aggregate action of A, B, C, D, &c. on 1, 2, 3, 4, &c. is simply a prevailing attraction which is only effectively resisted by the final indefinite repulsion, and thus the whole mechanism of this curve serves only to make some perturbations in attraction with no palpable result whatever.

Passing now from a line of atoms to a medium or mass of matter, the same result is found only vastly exaggerated. Referring the medium to three rectangular axes of X, Y, Z, and conceiving the line of atoms already discussed as coinciding with the axis of X, we wish to determine the aggregate forces which counteract each other in a superficial unit of the plane Y Z. The total action of the column A, B, C, D, &c. on the matter filling the space beyond the plane Y Z, affords the true criterion of forces acting in a medium, since all the columns parallel to A, B, C, D, &c. can be similarly treated, and thus all forces acting through the plane Y Z will be included in the discussion.

To realize the action of A on the mass beyond the plane Y Z, construct with A, as a centre, consecutive spherical surfaces between atoms 1, 2, 3, 4, &c. to infinity. The quantity of matter in these hollow shells increases as the square of the radius: hence the aggregate force exerted by and on a shell involves a particular function of the square of the distance. The result already found for the action of A on the line of atoms 1, 2, 3, 4, &c., must therefore be multiplied by this function of x^2 to obtain the action of A on the series of spherical shells. For the gravitation branch of the curve the direct and inverse functions of x^2 will neutralize each other, and the curve will become a straight line parallel to the axis of X, giving an infinite aggregate attraction for an infinite medium. In other words, all gravitating shells give equal total attractions. For the atoms B, C, D, &c. nearly the same result will be found, by a like process, the only difference being in the cutting off portions from the ori-

gin end of the curve. The total action of A, B, C, D, &c., as of all the parallel atomic columns, will thus be to give this measureless preponderance to the attractions. Hence, a medium composed of atoms acting on each other according to the Boscovich force curve, would be unalterably cohesive, and the effects of the various attraction and repulsion branches interpolated between gravitation and the final repulsion would be totally insignificant. In an indefinite medium, the gravitation area would greatly exceed the area of the final repulsion, so that such a medium would of its own accord rush in upon itself, and become as it were but a single gigantic atom with a definite atmosphere surrounding it.

Whoever will follow this simple exposition of the effects of Boscovich's exponential or experimental curve of interatomic forces, when such atoms constitute a medium or mass, which case alone exists in nature, will see that this curve and the theory it expounds are deplorably false to facts, and destructive to their own pretensions. An exponential curve might doubtless be devised which would provide for the function of the square of the distance, and which would obviate the present gross exaggeration of attraction; though such an empirical curve seems little worth the labor. Surely however it is time for all to discard that misshapen and self-destroying exponential curve which has too long passed unchallenged, because Boscovich was a really great mathematician: great enough indeed, to have fully recognized the faults of his theory had they been actually pointed out to him. It is a strange oversight on his part, that he did not perceive the fallacy involved in his process of first constructing four atoms into a particle, four particles into a particle of the second order, &c.; as if his primary forces would recognize the ideal boundaries of such particles. He in fact neglects all actions except those between adjacent particles, when he passes to a medium, and Robison most pointedly does the same in his favorite conception of springs uniting atoms. If this neglect were really meant, the question would arise as to what becomes of the machinery for gravitation, and what springs are those binding the sun and earth? I cannot but regard the processes of reasoning employed by Boscovich and Robison as singularly rude and gross for such men, and such a subject; nor can molecular mechanics receive a more needful service than by the expurgation of views so abounding in error, and so obstructing the pathway to light. A fabric of objections and difficulties will surely arise in the mind of any well furnished investigator who will really think strictly on this renowned theory of spheres of force. The objections now presented are but specimens.

The speculation of Faraday lacks the definiteness of Boscovich's theory, and is not pushed into the field of molecular aggregation, nor indeed could it be with much hope of success. Ray

vibrations would be a very ingenious mechanism for gravitation, if the Newtonian law could be deduced in a tolerably simple manner from it, but this requisite seems to throw us back on the strange theory of a universal coexistence of all matter. Its inaptness for illustrating molecular mechanics is peculiarly striking if we attempt to imagine ray vibrations for the several phases of molecular constitution. In fact, the reduction of all forces to one law, such as that of Boscovich or Faraday, is like describing all animals as of the color of a chameleon.

In strange contrast with the Theoria and the Speculation, is the investigation by Mossotti, which is based on real mechanical principles, and which, though quite imperfect, leads to real results. By assigning definite size to atoms, and applying the simple Newtonian law of force to two kinds of matter, conditioned as in the Franklinian electrical theory, modified by Epinus, Mossotti has avoided most of the objections urged against the theory of spheres of force, and has given a glimpse at least of what heat is in the constitution of masses. By extending his investigation, and by supplying some deficient elements, molecular mechanics may at last be established on that simple and sure basis of ordinary mechanical principles, which Newton and Laplace have so distinctly foreshadowed, and which the expanding realms of physical science demand with a positiveness hitherto unknown.

Art. XXXI.—*Contributions to Mineralogy;* by James D. Dana.

1. *On the relation of Leadhillite in crystallization, to the Anhydrous Sulphates and Carbonates.*

The sulphato-carbonate, Leadhillite, shown to be trimetric in crystallization by Brooke and Miller, has three prominent points of interest: its approximation in form, viewed in one direction, to a regular hexagonal prism—its hemihedrism, which gives it a mono-

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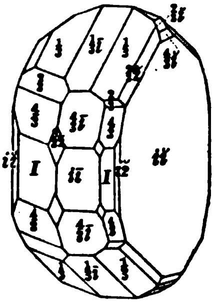

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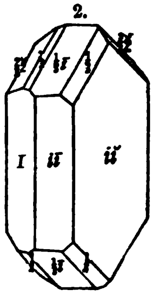

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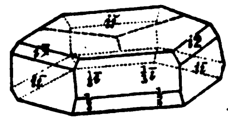

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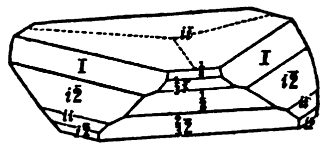


clinic aspect—its twin-composition, under which it takes a rhombohedral character. Figure 1 represents the known planes, taken

from a figure in Mohs's Mineralogy, pl. 13, fig. 97, and rendered nearly holohedral by adding (though of reduced size) the wanting planes. In the occurring crystal, the right-hand I, $\frac{4}{3}$, $\frac{2}{3}$, $i\breve{2}$ and $\frac{8}{3}\bar{2}$ are absent. Figure 2 represents an actual crystal of simpler form; it has but one plane I; and one plane $\frac{2}{3}\breve{2}$ on either side is obsolete.

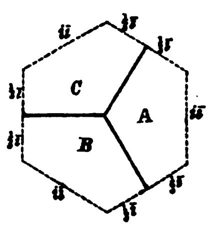
Figures 3 and 4 represent known twin forms, copied with altered lettering from tracings received by the author from R. P. Greg, Jr. The plane $i\breve{\imath}$ in these figures is made the base.

The prism $\frac{1}{3}\bar{\imath}$ has for its angle (at top) 120° 40′, which is very near the angle of a regular hexagon; so that these planes with the planes $i\breve{\imath}$ (fig. 1, 2) make up a hexagonal prism, varying 20 to 40 minutes from the angles of the regular hexagonal prism. And moreover as a consequence, the occurring planes between $i\breve{\imath}$ and $i\bar{\imath}$, and between $i\breve{\imath}$ and $\frac{1}{3}\bar{\imath}$, are nearly alike in angle—I and $\frac{2}{3}\breve{2}$ inclining towards $i\breve{\imath}$ at the same angle within 8′; and $i\breve{2}$ (not shown in the figures,—situated between I and $i\breve{\imath}$) and $\frac{1}{3}$, at the same angle within 6′. Again, the macrodome $1\bar{\imath}$, like $\frac{1}{3}\bar{\imath}$, is near 120° in its angles, giving 119° 40′ and 60° 20′, the acute edge of the dome in this case being above.

Brooke and Miller make the prism lettered $\frac{1}{3}\bar{\imath}$ the fundamental prism, and $i\breve{\imath}$ the basal plane. This gives simple expressions for the planes; but it does not appear to exhibit the true relations of the species. We arrive at this conclusion from the following considerations.

The twins consist evidently of *three* united crystals, as so regarded by Brooke and Miller, the plane $i\breve{\imath}$ (or planes in the series $i\breve{\imath}$, $i\bar{\imath}$, $i\breve{\imath}'$) forming three sides and only three out of the six (fig. 3). If the prism of 120° 40′ ($\frac{1}{3}\bar{\imath}$ above) be the fundamental prism, and analogous to that of Aragonite, the twins should be formed by composition parallel to the lateral planes of this prism, and moreover two of these lateral planes should be adjacent; or rather the perimeter would include at least four planes of the fundamental rhombic prism. But on the contrary, these planes are *alternate* instead of adjacent, and composition is not parallel to either of them, and hence there is no similarity to any known twin in the Aragonite group. This is seen in the annexed figure (fig. 5);—the sides of the hexagon lettered $\frac{1}{3}i$ are M of Brooke and Miller.

5.



The composition is in fact parallel not to $\frac{1}{3}\bar{\imath}$ (of figs. 1, 2), that is M of Brooke and Miller, but to $1\bar{\imath}$ which also is near 120°, having, as stated above, the angle 119° 40′. Hence this form $1\bar{\imath}$ ($i\breve{3}$ of Brooke and Miller) is better entitled from analogy and general principles, to be considered the fundamental prism of Leadhillite than $\frac{1}{3}\bar{\imath}$. In fact the prism $1\bar{\imath}$ (119° 40′) since it is the prism

parallel to which composition takes place, must be the true representative of the fundamental prism of Aragonite and the allied carbonates.

The relations of the sulphates and carbonates have been shown on page 53 of this volume, and had previously been brought out by Hausmann. It is there seen that the unit prism and domes of the sulphates and carbonates are as follows.

Barytes, a sulphate,	$(1\bar{\imath})$ 116° 20′	(I) 78° 20′	$(1\breve{\imath})$ 105° 24′
Anhydrite, "	" 118° 35′	" 77° 4′	" 107° 22′
Aragonite, a carbonate,	(I) 116° 10′	$(1\bar{\imath})$ 81° 40′	$(1\breve{\imath})$ 108° 26′

Adopting the prism of 119° 40′ in Leadhillite, as corresponding to the prism of 116° 10′ in Aragonite, as above shown, the corresponding angles are for

Leadhillite, . . .	119° 40′	76° 44′	107° 26′

For Barytes and Anhydrite (sulphates) the prisms of 78° 20′, 77° 4′ (the supplements of which angles are 101° 40′ and 102° 56′) are ordinarily taken as the vertical prisms; while in Aragonite, that of 116° 10′ is the vertical prism; the difference being one of position. The question now is, therefore, whether the prism of 119° 40′ is the true vertical prism of Leadhillite, and it is thence most closely related to the carbonates, or whether the vertical prism is that of 76° 44′ and 103° 16′, making its closest relation to the sulphates. The latter is the view adopted by the author in a former paper and in the lettering of the above figures, and it appears to be sustained by the following reasons.

1. The planes of the prism of 119° 40′ have not yet been observed, or if observed they are of very rare occurrence. In figure 1, the occurring planes are $\frac{1}{3}\bar{\imath}$ and $\frac{4}{3}\bar{\imath}$, without $1\bar{\imath}$; and in the other zones, we find $\frac{1}{3}$, $\frac{2}{3}$, $\frac{4}{3}$, without the plane 1. Or, putting the crystal on $\tilde{\imath\imath}$ as its base, according to the view of Brooke and Miller, neither the prism of 119° 40′ is represented, nor any plane in that vertical zone. This is strikingly in contrast with the facts in the prismatic carbonates, in which the fundamental zone is represented by two or more planes in each of the species.

There is therefore a wide divergence from all the carbonates in this respect. And the divergence appears still wider when we consider that the most prominent zone is that of the prism of 120° 40′ ($\frac{1}{3}\bar{\imath}$ in the lettering of figure 1).

2. The analogies between three trimorphous groups pointed out by the author in the last volume of this Journal, page 210, favor the view that the prism of 103° 16′ is the true vertical prism. As this point is important, the facts are here repeated.

Rhombohedral.	*Trimetric.*	*Monoclinic* (basal cleavage.)
Calcite ($\dot{C}a\,\ddot{C}$) 105° 5′	Aragonite, $\dot{C}a\,\ddot{C}$, 116° 10′	Barytocalcite ($\dot{C}a$, $\dot{B}a$)$\ddot{C}$, 95° 8′
Dreelite ($\dot{C}a$, $\dot{B}a$) $\dddot{S}$; 93°–94°	Anglesite $\dot{P}b\,\dddot{S}$, 103° 38′ Anhydrite $\dot{C}a\,\dddot{S}$, 102° 56′ Barytes $\dot{B}a\,\dddot{S}$, 101° 40′	Glauberite ($\dot{C}a$, $\dot{N}a$) $\dddot{S}$; 83°
Susannite, $\dot{P}b\,\dddot{S}+3\dot{P}b\,\ddot{C}$ 94°	Leadhillite, $\dot{P}b\,\dddot{S}+3\dot{P}b\,\ddot{C}$ 103° 16′	Lanarkite, $\dot{P}b\,\dddot{S}+\dot{P}b\,\ddot{C}$, 85° 48′

The rhombohedral and monoclinic forms, and the trimetric and rhombohedral, have nearly a common difference, 10 to 11 degrees; moreover the sulphates and carbonates in each column differ in angle by nearly a common difference, or 12 to 13 degrees. At the same time, in each, the sulphato-carbonates agree with the sulphates. The fundamental prism of aragonite has its angle 11 degrees larger than the rhombohedral angle of calcite; the prism of anhydrite 9 or 10 degrees larger than the rhombohedral angle of Dreelite; and therefore the prism of Leadhillite is that one which differs by some similar angle from Susannite. The parallelism above is so exact both vertically and horizontally, that the argument must be allowed to have much weight. Its authority becomes irresistible when viewed in a different light.

3. Dreelite is dimorphous with Anglesite and Barytes,—the same compound, essentially, occurring here under a *rhombohedral* and *trimetric* form. Moreover, Dreelite and Susannite are identical nearly in angle, or are homœomorphous; while Susannite and Leadhillite constitute a second case of dimorphism parallel with that just mentioned. Since now this sulphato-carbonate under its rhombohedral form (Susannite) is homœomorphous with the sulphate (Dreelite) and not with the carbonate, hence in its trimetric state (Leadhillite) it should be most closely related to the sulphate (Anglesite or Anhydrite). As the sulphuric acid dominates over the carbonic acid in the Susannite, one form of the sulphato-carbonate, it must also dominate over it in that same sulphato-carbonate under any other form it may present, or in Leadhillite; whence in either case, the forms should be homœomorphous with the corresponding sulphates.

It seems therefore to follow that 103° 16′ is the true vertical prism, as adopted in the figures.

4. We may find further objection to regarding Leadhillite as related most nearly to the prismatic carbonates in the fact that in this case, the most perfect cleavage would be basal, which is not true of any known species of the aragonite group. But after the arguments above stated, we hardly need look further for evidence. An objection to the view adopted might be suggested from the fact that through hemihedrism, one of the prismatic planes *I* is wanting. But there is no reason why hemihedrism should not result in suppressing one of these as well as other planes, and the objection is of little weight. Moreover the fact, which we learn from Brooke and Miller that the plane *I* is also a direction of twin composition, shows that this plane is at least one of prominent or fundamental value in the crystal.

Upon the view which has been discussed, the species *Anglesite*, *Anhydrite* and *Leadhillite* have the following dimensions:

	Prism $I:I$	Dome $1\breve{\imath}$	Dome $1\bar{\imath}$	Axes $a:b:c$
Anglesite	103° 38′	62° 42′	75° 29′	1·6415 : 1 : 1·2715
Anhydrite	102° 56′	61° 25′	72° 38′	1·6336 : 1 : 1·2557
Leadhillite	103° 16′	60° 20′	72° 34′	1·7205 : 1 : 1·2632

We add a remark with regard to the rhombohedral character of the twins.

In the twins, like fig. 4, the series of planes in each sextant are closely related; thus, as has been observed, the planes I, $i\breve{2}$, $i\bar{\imath}$, $i\bar{2}$, respectively have nearly the same inclinations on $\tilde{\imath}\tilde{\imath}$ as $\frac{2}{3}\breve{2}$, $\frac{1}{3}$, $\frac{1}{3}\bar{\imath}$, $\frac{1}{3}$ have on the opposite $\tilde{\imath}\tilde{\imath}$. From I the planes narrow downward, and from $\frac{2}{3}\breve{2}$ they narrow upward and so alternating around. In fig. 3, there is a corresponding alternation, though of less extent. Comparing it with the simple crystal, it looks like an inversion of the alternate sectors; but as the compound form contains only three simple crystals, an actual alternate inversion is impossible. As the plane I is a fundamental plane, the occurring one (see fig. 2) should have a supremacy in the twin, and with it, the series to which it belongs: and as this series in the simple form diminishes from I to the opposite side (the right in fig. 2), this would imply a reverse enlargement of the next or ($\frac{1}{3}$) series, and by this alternation the rhombohedral character would result. The fact, moreover, that the compound is dimorphous and that the other form is rhombohedral, with the same angles nearly as the twin of Leadhillite (as shown by Brooke and Miller) may suggest further reason why the twin should take the alternating or rhombohedral character.

These views are especially interesting as bearing on the subject of dimorphism, and illustrating the passage of a trimetric form to a rhombohedral.

2. *On the so-called Silico-Titanates and Silico-Tantalates.*

In a former number of this Journal it was shown that Sphene was a true silicate of the form $(\ddot{\ddot{R}})^3\,\dddot{Si}^2$, or what is equivalent $(\ddot{\ddot{R}})\,\dddot{Si}^{\frac{2}{3}}$, in which

$$\ddot{\ddot{R}}(\text{or } R^2O^3) = Ti\,O^2 + Ca\,O.$$

It is consequently *trimorphous with Andalusite and Kyanite.*

In this volume, page 130, the author also observes that the formula of Keilhauite, on the same principle, may be

$$(\dot{R}^3, \ddot{\ddot{R}})\,\dddot{Si}^{\frac{2}{3}},$$

and that of Wöhlerite

$$(\dot{R}^3, \ddot{\ddot{Z}}r, \ddot{\ddot{N}}i)\,\dddot{Si}.$$

The special formula of this last afforded by the analyses, is

$$(\tfrac{6}{10}\dot{R}^3 + \tfrac{3}{10}\ddot{\ddot{Z}}r + \tfrac{1}{10}\ddot{\ddot{N}}i)\,\dddot{Si}, \text{ or } 6\dot{R}^3\,\dddot{Si} + 3\ddot{\ddot{Z}}r\,\dddot{Si} + \ddot{\ddot{N}}i\,\dddot{Si} = (\dot{R}^3, \ddot{\ddot{R}}, \ddot{\ddot{R}})\,\dddot{Si}.$$

The analysis by H. Rose of *Tscheffkinite* (Pogg. lxii, 591) appears to lead to the same general formula with that of Keilhauite, or

$$(\dot{R}^3, \ddot{\ddot{R}})\,\dddot{Si}^{\frac{2}{3}}.$$

Schorlomite afforded Whitney the formula—

$$\dot{C}a^3\,\dddot{Si} + \ddot{\ddot{F}}e\,\dddot{Si} + \dot{C}a\,\ddot{T}i^3$$

Making the $\ddot{T}i$ a base as above, the oxygen ratio for the bases and silica is 6 : 11. But if the silica as obtained be a little too high,

the ratio may be 5½ : 11 or 1 : 2, whence would come the formula

$$2\dot{R}^3\,\dddot{Si}^{\frac{1}{2}} + \bar{R}\,\dddot{Si}^{\frac{1}{2}} \quad \text{or } (\tfrac{2}{5}\dot{R}^3 + \tfrac{3}{5}\bar{R})\,\dddot{Si}^{\frac{1}{2}} \quad = (\dot{R}^3, \bar{R})\,\dddot{Si}^{\frac{1}{2}}$$

(analogous to that of staurotide) = Silica 23·3, titanic acid 22·9, peroxyd of iron 22·4, lime 31·4 = 100. It gives 2 per cent. too little of silica, according to the analyses, while agreeing closely with the results in other respects.

Mosandrite, in a similar manner, gives for the oxygen ratio of the protoxyds, peroxyds and silica 1 : 2 : 3, or of bases and silica 1 : 1 (precisely 16·57 : 15·86); affording the formula

$$\dot{R}^3\,\dddot{Si} + 2\bar{R}\,\dddot{Si} + 4\tfrac{1}{2}\dot{H} \quad \text{or} \quad (\tfrac{1}{3}\dot{R}^3 + \tfrac{2}{3}\bar{R})\,\dddot{Si} + 1\tfrac{1}{2}\dot{H}, = (\dot{R}^3, \bar{R})\,\dddot{Si} + \text{Aq},$$

which, excluding the water, is the formula of epidote. The crystallization of mosandrite has not been clearly made out.

It is probable, that there are no true silico-titanates or silico-tantalates, the titanic or tantalic acid being a base in each case.

3. *Tourmaline.*

The author has shown that the general formula of Tourmaline is

$$(\dot{R}^3, \bar{R}, \dddot{B})\,\dddot{Si}^{\frac{3}{4}},$$

the oxygen ratio between the silica and all the other bases being 3 : 4, as ascertained by Rammelsberg, and this being the only constant ratio. The oxygen ratio for the protoxyds, peroxyds, and boracic acid, as deduced from Rammelsberg's analyses, varies greatly. Group I, affords mostly the ratio 4 : 12 : 4,—Group II, the ratio 4 : 15 : 5,—Group III, the ratios 4 : 21 : 6, 4 : 24 : 7, etc.—Group IV, the ratios 4 : 36 : 11, 4 : 40 : 12, etc.—Group V, the ratios 4 : 48 : 13, 4 : 56 : 12, etc.*

For Group I, the special formula is hence

$$\dot{R}^3\,\dddot{Si}^{\frac{3}{4}} + 3\bar{R}\,\dddot{Si}^{\frac{3}{4}} + \dddot{B}\,\dddot{Si}^{\frac{3}{4}} \quad = (\tfrac{1}{5}\dot{R}^3 + \tfrac{3}{5}\bar{R} + \tfrac{1}{5}\dddot{B})\,\dddot{Si}^{\frac{3}{4}}.$$

For the Red Tourmaline of Elba, in Group V,

$$\dot{R}^3\,\dddot{Si}^{\frac{3}{4}} + 14\bar{R}\,\dddot{Si}^{\frac{3}{4}} + 3\dddot{B}\,\dddot{Si}^{\frac{3}{4}} \quad = (\tfrac{1}{18}\dot{R}^3 + \tfrac{14}{18}\bar{R} + \tfrac{3}{18}\dddot{B})\,\dddot{Si}^{\frac{3}{4}}.$$

These appear to be the extreme variations in the species Tourmaline, if we exclude the analysis (No. 30) of a somewhat decomposed variety from Rozena. The formulas for the other ratios may be easily written in like manner, in accordance with the general formula above.

Axinite has in like manner the general formula

$$(\dot{R}^3, \bar{R}, \dddot{B})\,\dddot{Si};$$

the analyses afford, as the special formula under this type

$$\dot{R}^3\,\dddot{Si} + 2\bar{R}\,\dddot{Si} + \tfrac{1}{3}\dddot{B}\,\dddot{Si},$$

and this formula was suggested by Rammelsberg in his Handwörterbuch, i, 72.

* The oxygen ratios deduced by Rammelsberg for the protoxyds, peroxyds and silica, *the boracic acid* being included with the silica, are for Group I, 1 : 3 : 5; II, 1 : 4 : 6; III, 1 : 6 : 8; IV, 1 : 9 : 12; V, 1 : 12 : 15.

ART. XXXII.—*Notice of the Life and Writings of the late Dr. Waldo Irving Burnett;* read before the Boston Society of Natural History, July 19th, 1854, in accordance with a vote passed at the previous meeeting. By JEFFRIES WYMAN, M.D.

Mr. President—From time to time Death has entered our circle, and taken from our number one and another of those who have been our most active associates, and to whom we have been bound by the ties of personal regard or of friendship. In nearly every instance they have been removed in full manhood, or even at a later period, when the labors of a life of the ordinary length had been nearly finished. But never before has there been taken from amongst us one who, in his devotion to natural science, has, in so brief a life, left so many memorials of zeal and industry as he, to whose memory we would now pay our tribute of respect.

WALDO IRVING BURNETT was born in the town of Southboro', Mass., July 12th, 1828. His father (the late Dr. Joel Burnett) was a man of distinguished excellence in his profession, and to the qualities of a good and useful citizen united those of an ardent lover of nature, of whose works he was a close and faithful observer. Botany and Entomology especially received his attention, and without the aid of genial spirits, or the intercourse with kindred minds, were studied with no ordinary zeal during the few leisure moments which were left him after the demands upon his time by a laborious profession had been satisfied. His love of nature was transmitted to the son, and was manifest in early boyhood, when the observation and study of insect life took a strong hold upon his mind. His father experienced a just pride in witnessing these tendencies; but in place of encouragement, which he at first extended with delight, he was soon, though reluctantly, obliged to substitute restraint. His son's mind was too intently absorbed in his pursuits, and fears were excited lest his studies, prolonged into hours stolen from the usual period of repose, should be attended with disastrous results to his physical constitution. His passion, however, grew with his growth and strengthened with his strength, and in the face of all obstacles, through health and through sickness, from an early youth to his early grave, it was never abated.

He had not the advantage of a collegiate education; this he chose to forego, not from any indifference to its value, but from a sensitive unwillingness to subject his father to any unnecessary expenditure of his means. He gave early indications of great mental activity, and mastered with ease all the studies of the Academy; in mathematics, especially, he was unusually proficient, and drew from his teacher the confession that in this de-

partment he was no longer capable of giving him instruction; and it was the habit of other teachers in the neighborhood to send to young Burnett for the solution of difficult questions which they themselves were incompetent to master. Almost without assistance, at a later period, he made himself familiar with the French, Spanish, and German languages, and during the latter part of his life had made some progress in the Swedish.

At the age of sixteen he had become thoughtful beyond his years; and then commenced the development of those tendencies in his mind which ever afterwards were so conspicuous, and which continued to exert a controlling influence, viz.: the desire of gaining an insight into the nature of things, and of forming philosophical ideas and conceptions of natural processes, conceptions and ideas which can be obtained only by the exercise of the higher powers of the mind. Mesmerism, materialism, and theological questions occupied his thoughts, and were frequently written upon and discussed by him. On all of these he manifested independence and continnity of thought, and persistence in whatever direction his mind was turned. It was at this early age that his interest in the study of medicine commenced, when he accompanied his father in his professional visits, and witnessed the effects of disease, as manifested in the examination of bodies after death. Entomology now especially engrossed his thoughts, and nearly all his leisure moments were occupied in collecting, studying and classifying insects. While yet in his sixteenth year his father died. This event materially changed his prospects, and was met with firmness and decision, and in the course of the following year, finding that something must be done for his support, he commenced teaching school, and at the same time gave his attention to the study of medicine.

The subsequent years of his student life were spent under the direction of Dr. Joseph Sargent, of Worcester, with whom there grew up warm mutual personal regard and friendship: in the Tremont Medical School in Boston, which has given to the profession so many zealous and productive laborers in medical science: and in the Massachusetts General Hospital. He was ardent and industrious as a medical student, but never allowed his attention to be withdrawn from the study of nature, the microscope becoming his constant companion, and a source of neverfailing pleasure. As evidence of his ability it may be stated that in two successive years he gained the annual prize offered by the Boylston Medical Society. The subject of the first essay was *Cancer*, treating especially of its microscopic structure; and of the second, *The Sexual System, or the production of being, considered as to its physiology and philosophy.*

In 1849, at the age of 21, he graduated in medicine, and soon after visited Europe, where his attention, especially at Paris, was

given almost exclusively to natural history and microscopic observation. The expectations of intellectual progress which he now looked forward to with so much interest, were soon doomed to severe disappointment. It was in Paris that he received the first serious warning that consumption, the disease which eventually destroyed his life, had already marked him for its early victim. After an absence of only four months, he re-embarked for America, to receive the benefit of a more genial climate in one of the southern states, and each successive winter he passed either in Carolina, Georgia or Florida, in order to avoid the inclement and uncongenial climate of New England. He had now no permanent location, was constantly shifting from place to place, to mitigate, as far as possible, the steady progress of his disease. Everything seemed adverse to anything like connected study. Nevertheless, it was during these few unsettled years that he accomplished an almost incredible amount of intellectual labor. He was incessantly occupied with his microscope; his mind was ever on the alert, and he allowed scarce a day to pass without some observation, without something added to his stock of acquired knowledge.

In the winter of 1851 he delivered at the Medical College in Augusta, Ga., a successful course of lectures on Microscopic Anatomy. In the summer of 1852 he prepared the principal work of his life, the Essay which received the prize from the American Medical Association. His two former prizes were competed for only by his fellow students; but the third, it is no small praise to say, was open to the competition of the whole medical profession throughout the country.

While yet a medical student he became an active member of the Boston Society of Natural History, and was soon after elected Curator of Entomology. In 1851 he was elected a member of the American Academy of Arts and Sciences—one of the youngest members ever admitted into that body. His communications to different scientific bodies and journals were very numerous and on a great variety of subjects, and give such evidence of industry and enthusiasm as cannot fail to excite our wonder and admiration. They are too numerous for analysis or even enumeration in this place; but some of the more important ones are found under the following list of subjects, which comprises those of about one-third of the whole number of his memoirs and communications, and which serves to show that his mind was interested in a great variety of questions, and that whenever an opportunity for investigation presented itself, he was always ready with a cheerful heart and patient industry to enter upon his work.*

* His various scientific papers or abstracts of them may be found in the *Proceedings*, also in the *Journal of the Boston Society of Natural History*. In the *Proceedings of the Boston Society for Medical Improvement*, in the *Proceedings* and in

"*On the Hybernation of Insects, and its Relation to their Metamorphosis.*"

"*An account of certain microscopic animals found in a person who died of an enlarged spleen.*"

"*On the external parasites of warm-blooded animals.*" This was a subject to which he had devoted much attention, and in illustration of which he had made large collections of specimens preserved for microscopic study.

"*On the embryology of the Articulata,*" including remarks on the alternation of generations in the Humble bee, (Bombus Americanus,) in which last he ascertained that three generations are produced from one impregnation.

"*On the luminous spots of the great Fire Fly of Cuba.*"

"*Observations on the seventeen-year locust.*"

"*On Spermatozoa.*"

"*On the origin, development and structure of the kidneys throughout the vertebrated division of animals.*"

"*Notes on the Rattle-snake, relating to its dentition, to the physiological effects of its poison, and to alcohol as a remedy.*"

"*Some account of an Insect, (Rhinosia pomatella,* Harris,) *and its recent injuries to the fruit and forest trees of New England.*"

"*On the development of Viviparous Aphides, or plant lice.*" This is a subject of great interest, and it was investigated with great ability. Since the days of Bonnet it has been well known that several successive generations of Aphides are produced after a single impregnation. Dr. Burnett studied the successive generations as they first appear in the body of the parent, as illustrated by the species infesting the hickory. If a fully developed, but wingless Aphis is examined in the spring, it is found to contain an embryo nearly mature; and this embryo contains already the first germs of the third generation, in the form of single cells or a small number of cells enclosed in a sac. While a few germs are thus formed, others are formed by their subdivision from constriction, until the requisite number is obtained. When they have reached the size of about one three-hundredth of an inch, a yellowish mass forms at one extremity of the egg, and then commences the development of the parts of the insect, which eventually enclose the mass just mentioned. It is this last yellowish mass which furnishes the materials for the next generation. All this, it should be remembered, is effected without the aid of any distinct reproductive organs. There is no ovary or oviduct, but the embryos are developed in the cavity of the abdomen, and discharged through a genital opening merely. In view of the fact that the Viviparous aphides are sexless, Dr. Burnett

the *Memoirs of the American Academy of Arts and Sciences*, in the *American Journal of Science*, in the *Boston Medical and Surgical Journal*, and in the *American Journal of Medical Science.*

regards their mode of reproduction as belonging to the gemmiparous type. Viewed in this way, the different broods cannot be looked upon as so many generations; but on the contrary, the whole suite, from the first to the last, that is, till the production of a winged Aphis, constitute but a single generation. This explanation by a species of budding seems far more satisfactory than that which supposes that either cells or nuclei of the first individual are transmitted by successive inclusions to the last. As this latter idea cannot be supposed to be the result of direct observation, and as no proof is adduced that identical cells and nuclei really pass from one generation to the other, the whole stands merely as an ingenious theory, while Dr. Burnett's explanation [and this view is not proposed for the first time by him,] is in accordance with direct observation. But, in accepting his view, we are compelled to admit the hypothesis, that the germinating force imparted to the first ova is transmitted to the successive broods without the aid of spermatozoa.

"*On the microscopic appearances presented in the intestinal discharges and muscular fibres of a patient who died of the epidemic cholera.*"

"*Tissue and its retrograde metamorphosis.*"

"*On the Geology and other points connected with the natural history of Florida.*"

"*Considerations on a change of climate by northern invalids, and on the climate of Aikin, S. C.*"

"*Considerations of some of the relations of climate to tubercular disease.*"

To these should be added his various critical notices of recent scientific publications in Silliman's Journal, which in view of the short time he occupied the position of associate editor, were quite voluminous, and serve to give us a good idea of his powers of analysis and discussion.

There is no one of his productions, which embodies more of the results of his labors, than the prize dissertation, consisting of two hundred closely printed octavo pages, presented to the American Medical Association in the year 1851, and entitled "*The Cell, its physiology, pathology and philosophy, as deduced from original observations; to which is added its history and criticism. 'Natura in minimis maxima est.'*"

To those who are acquainted with modern physiology, it will be seen at once that he had selected a great subject, one which even the most accomplished minds might approach with distrust. The nucleated cell! that minute organic structure which the unaided eye cannot discern, yet constituting the first stage of every living being, the seat of so many of the complex phenomena of animal and organic life, and the agent by which even the mind itself retains its grasp and exerts its influence upon the living

structures with which it is associated. In entering upon so difficult a subject as this, it was not expected, nor is there any reason to suppose that he himself expected, that he should not lay himself open to criticism. The ablest living histologist, Kölliker, in speaking of the subject of the development of tissue, uses the following language: "Not only does histology not possess a single law, but the materials at hand from which such could be deduced are as yet relatively so scanty, that not even any considerable number of general propositions appear well founded." As laws and general propositions were among the especial objects of Dr. Burnett's researches, it will be seen at once that he has entered boldly into a contested field. But it is to follow him in his labors, and not to hold up to criticism his results, that we have at present to do.

His subject is discussed under the following heads:

1st. *Cell-genesis*, under which he treats of the origin of cells, and advocates a peculiar mode of development, which he claims as original with himself, and the result of his own observations.

2d. *Cell physiology*, or healthy function.

3d. *Cell pathology*, or diseased function.

4th. *Cell philosophy*, or 1st, the relations of cells to the teleological view of organization; 2d, the direct agency of cells in the production and manifestation of nervous power, the intellectual processes, &c.

The general results of his studies of cell life and cell genesis are in his own words as follows: "The great outstanding fact which appears before us as the result of these studies is, that there is fundamental unity of organization. This we have seen to consist in elementary particles, which in both animals and plants are formed upon a common plan. It was the opinion of Schwann and Schleiden, who truly originated this view, that this plan consisted in the preëxistence of a solid fundamental body, (the nucleus) around which is formed a membrane ultimately expanding and constituting the cell. It has been one of my objects to show, that this is not of universal application, by an attempt to demonstrate another mode of cell formation, which is that the fundamental idea of a cell is a simple vesicle, and that the nucleated cell is simply one cell containing another within its walls. With Schwann the nucleus is *exogenous* and *germinative*—with me the nucleus is *endogenous* and reproductive.

"The two conclusions of the studies of cell life are then 1st. The existence of an elementary particle, having an invariable unity of expression, *the cell.* 2d. The universality of the application of this particle for the formation of organized parts, *the tissues.*"

In studying cells in relation to pathology, he regards this last as an erring physiology, and concludes, that, both as to their gen-

esis and general aspect as cells, those which belong to abnormal cannot be distinguished from those belonging to normal conditions of life. The genetic and general relations of cells in physiology and pathology are therefore the same. Their difference does not relate to structure, but to their destiny. Physiological cells must be considered teleologically, but pathological ones have no ulterior object.

Each of the different heads of his dissertation he discusses with great ability, and gives ample evidence that he is not only familiar with the scientific labors of others, but that he is perfectly at home in the different departments of investigation which his essay involves. If it be allowable to express an opinion of its merits in general terms, it may be truly said that it gives evidence of wonderful zeal and industry in research, of acute powers of observation, and of great readiness in perceiving general relationships. It is in connection with this latter faculty that he seems the most liable to error. He appears to have partaken something of the spirit of Oken, and to have given way at times to the suggestions of the imagination, instead of subjecting himself to the severe mandates of reason, and the rigid rules of induction. This is naturally the fault of youth, and for which scientific minds, at the present time, with their tendencies to hasty generalization, may be justly said to be in part responsible. But in one who combined industry, a desire for truth, and an almost unlimited patience in observation, it might have been fairly anticipated that, sooner or later, the better and safer qualities of the mind would have eclipsed all others.

While constantly active as an observer, Dr. Burnett found time to engage in another service which occupied some of the latest hours of his life, and the non-completion of which was a source of anxiety to him in his last moments. This was the translation from the German, of the Comparative Anatomy of Siebold and Stannius. All who are familiar with the published volume, will not fail to see in it another proof of his industrious habits as exhibited not in the translation merely, (itself in this case no ordinary labor) but in the numerous additions to science which, scattered far and wide through scientific journals, have been brought together, and in the contributions he himself has made from his own stores of accumulated observations.

The last scientific investigation to which his time was devoted was into the natural history of the *Orange insect*, which is so destructive to the orange trees of Florida. The habits of this insect he had studied during his last winter's residence in Florida, and had prepared a memoir in reference to it for the American Association for the Advancement of Science, but his ill health prevented his attending their recent meeting.

Such is an imperfect sketch of the scientific labors of our late associate. It only remains to consider his life from another point of view, in regard to its moral aspect. Of this I do not feel justified in treating at length, as my relations to him were not sufficiently intimate to speak from personal observation; but from all I can learn from his associates, from his fellow-students and his more intimate friends, he was a kind and affectionate son and brother, one who enjoyed to an unusual degree home and all its associations; he was a man of a truly benevolent heart, into which irreverent thoughts seemed to gain no admission, or from which they certainly obtained no expression. In all of his studies of nature he seems to have had a pervading perception of God in his works, and often in eloquent words gives expression to his feelings, when some new manifestation of divine wisdom was uncovered to his inquiring mind.

Dr. Burnett's zeal and devotion could not fail to awaken a warm interest wherever he went, among those with whom he associated. He became acquainted with the leading naturalists of the country, and obtained from them and others, willing aid and counsel, as well as respect for his great acquirements. To them he always felt warm feelings of gratitude. But there was one, to whom, more than all others, he was especially grateful, a friend and relative, who at an early period, perceived the indications of uncommon promise for the future, and who with kind heart and benevolent purpose aided and encouraged him in all his undertakings.

He had religious faith and religious hope. To a speculative mind like his, it seemed almost a matter of necessity that the momentous questions which the problem of life involved, should sooner or later, have been presented for examination and discussion, and that before any settled convictions could be reached, they should have found him perplexed and in doubt. Doubts and perplexities in his mind did exist, but eventually they gave way and were replaced by faith and hope, which lightened his burden when, weary and exhausted, he approached the end of life. He had been long accustomed to look upon death and to talk about it as an event that he must meet at an early period. But death, if not imminent, is something that all look forward to calmly and without emotion, and when we speak of it we are not sure that we give utterance to our most solemn feelings and convictions. But there is one moment when, if ever on earth, the heart, if it opens itself, does so without disguise, if it give utterance, does so without reserve; it is that dread moment when death approaches so near that there is no alternative but to look upon earthly life as finished, its account made up, and when all that remains for the mind to dwell upon, is the dissolution of the body and the realization of another life. A few days before he

died our late associate returned after a winter's absence, to the home of his family, his bodily health exhausted, his energies prostrate. At first he entertained the hope that as before, rest and quiet might restore him partially at least to his usual health, and that he might have yet another opportunity of continuing those labors which he so fondly cherished; but his fast declining strength, the anxiety of those around him, the announcement of his physician and his own quick perceptions soon told that life was drawing to a close, and that for him the great moment was near. In all this he was calm and serene, conversed on the approaching separation without faltering, gave utterance to expressions of deep affection to those who were bound to him by the ties of kin, uttered his prayer for forgiveness, and expressed the solemn conviction, which now rose paramount to every other, that if there yet remained much for him to live for, there was yet far more to die for. On Saturday morning, July 1st, a few days before the completion of his twenty-sixth year, he died.

We cannot but sensibly feel, that in his death we have lost an associate of no ordinaty talents; we can point to no other member of our Society, and to not more than one other naturalist in our country, who has given such proofs of zeal and industry, and who, in so short a life, has accomplished so large an amount of scientific labor. Had he been spared to future years, we cannot but feel the assurance that he would have acquired for himself a far higher place and a still more honorable name in the annals of science. Let us cherish his memory and profit by his example.

The Resolutions which follow, prepared at the request of the Society by Prof. Wyman, were unanimously adopted:

Resolved, That the members of the Boston Society of Natural History have learned with deep regret the death of Dr. Waldo Irving Burnett; that, in his decease, we have lost a most active and zealous associate, and science an ardent, disinterested and productive laborer.

Resolved, That to the family of our late associate, we would offer our deep sympathy for their affliction, in the loss which they have sustained by the early death of one, with whose memory is associated so much of honorable devotion and noble self-sacrifice.

On motion of Dr. Abbott, it was voted, that Dr. Wyman be requested to prepare a copy of the Notice and Resolutions for publication in Silliman's Journal.

[It is with deep sorrow that in place of the usual Contribution from Dr. Burnett for this Journal, we have to present to our readers his obituary. One of the most earnest, faithful and profound laborers in science in the country has ceased from his work while

yet in the midst of research and with new truths constantly developing before his scrutinizing eye. During his connection with this Journal, he exhibited a deep interest in the progress of his special departments, gathering from every available source the means of enriching his papers; moreover he brought to bear upon the subjects before him a large amount of original research which enabled him to select the truth from error and pronounce judgment on the observations of the best investigators. Dr. Burnett was among the few in the land who not only knew well the latest results of the studies abroad in his department, but also labored successfully in testing those results, and more than this, contributed directly to the further progress of science.

The just tribute to the memory of our friend and colaborer by Dr. Wyman, renders it unnecessary for us to indulge in further remarks. His death is a grief to his friends; but science has even more cause to mourn.—Eds.]

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the continuity and force of the current of the magneto-electric machine.*—Sinsteden has published an interesting paper on this subject from which we extract those portions which appear of most importance in a practical point of view. The author's experiments were made with special reference to the employment of the magneto-electric machine in telegraphic operations, but it is clear that they are of equal importance for another practical application of the instrument, namely, to the process of electrotyping. When the inductor of a large machine was made to revolve thirty times in a second so that the iron cores approached and receded sixty times from the poles of the magnet, 120 single current impulses were of course produced, separated by as many intervals of about equal length. A single interval lasted therefore about the $\frac{1}{240}$ of a second, and as an experienced telegraphist with the key of Morse's apparatus opens and closes the circuit about twelve times in a second, there are in this case on the average twenty current impulses for every closure of the circuit. This is quite sufficient to work Morse's apparatus even upon very long lines; a preliminary experiment made by the Royal Telegraph Bureau in Berlin shewed that on the line of 74 (German) miles from Berlin to Dantzic, Morse's registers were worked with uniformity and certainty even when registers were placed at many intervening stations. During a long correspondence not a single uncertainty or error occurred while the magneto-electric machine was used, and the needle of a galvanometer at Dantzic gave a deviation several degrees greater than with the zinc-carbon battery usually employed. Other experiments showed that the machine was capable of transmitting signals with perfect regularity and certainty to a distance of 100 German (about 400 English) miles. In order to

demonstrate still further the continuity of the current, the author examined its magnetic and physiological effects. Two electro-magnets introduced into the circuit supported together 750 lbs., and a vibrating hammer apparatus gave regular and very rapid beats but no musical tone. When the intermitting spring of the apparatus was removed, the conductors held in the hands wet with salt water, and the inductors made to revolve three times in a second, strong shocks were felt in the arms which however were not sharp and sudden like those produced when the intermitting spring was used, but rose, swelled, and sank gently, so that they were received like waves and consequently easily borne. When the entire force of the steel magnet was diminished by laying on armatures and the rate of rotation increased till the inductor made thirty revolutions in a second, no shocks were felt, but instead, a very painful lasting contraction, first in the arms and then in the throat. These phenomena are analogous to those which are produced by a battery of 100 or 150 pairs, though of course it is not for an instant to be supposed that the current of the machine is as continuous as that of the battery. The result is the same, as Neeff and Pouillet have shown, when a battery and an interrupting wheel (blitz rad) are employed. From these experiments the author concludes that the current of the magneto-electric machine possesses sufficient continuity and constancy to be used in telegraphing, and that it approximates to the degree of constancy of a galvanic battery of many pairs. With respect to the intensity of the current of the magneto-electric machine and to certain peculiarities which give it in some measure a *specific* character, the author's experiments are particularly interesting. When the uninterrupted current from an inductor with free helices was conducted through a platinum wire ten inches long and one-tenth of a line in thickness, eight inches of the wire were ignited to whiteness: when a platinum wire one millimeter in thickness was employed as the interrupting spring, dense solid white sparks two lines thick and nearly four lines long were produced, in which the point of the platinum wire was ignited and fused. When a steel watch spring was employed in place of the platinum wire, one-fourth of inch of the steel was ignited and burnt off and a piece of the watch-spring a foot long could be fused in less than a minute. In this experiment the steel spring should form the positive and the interrupting wheel the negative pole; in this way the spring burns most brilliantly and the iron wheel is not injured. When the current was conducted through a voltameter having bright platinum plates immersed in dilute sulphuric acid, about three cubic inches of gas were evolved in a minute. After the plates had been used a long time, they had entirely lost their metallic lustre and become dull and dark gray, while upon the bottom of the voltameter a fine black powder had collected. Both the platinum plates and the black powder, possessed in a high degree the property of causing oxygen and hydrogen to unite: the powder proved to be metallic platinum while the microscope showed that the surface of the plates was rough as if eaten into. With two voltameters five cubic inches of gas were obtained per minute; with six voltameters in the circuit, having however plates of different dimensions, 8·8 cubic inches of the mixed gases were produced. In this last experiment it was very remarkable that

no strong polarization of the platinum electrodes of the six voltameters took place, since on uniting them by a delicate galvanometer, the needle shewed only a very feeble secondary current. Polarization of the electrodes in a very remarkable degree was however produced when the magneto-electric current was passed for a short time through decomposing cells in which the electrodes were lead, silver or nickel in dilute sulphuric acid, or zinc in solution of caustic potash. Two cells containing very dilute sulphuric acid and electrodes of pure silver 3 inches long and 1⅝ inches wide, were introduced into the circuit. After the current had passed, during a few seconds, the silver plates became covered with gas, the positive plates then became blackish gray, the negative plates covered with a delicate gray coating which rapidly passed to velvet black, increased in thickness, and finally dropped from the plates becoming at the instant whitish gray. The same change of color took place every time that the current ceased, the coating again becoming black when the current again began to pass. When the plates had stood a long time without the current passing, both negative and positive plates were covered with a thick yellowish white coating, and the precipitate had the same color. Spread out on paper this precipitate shews the whitest silvery lustre and proves to be finely divided metallic silver. If after the current had passed half a minute the electrodes were removed from the machine and used as a battery, sparks could be obtained, water decomposed and iron and platinum wire one inch long ignited and fused. When this battery was closed by the spiral of the magneto-electric machine—of course at rest—and by the body, severe shocks were received every time the current was broken: brilliant and loud sparks could also be obtained by this arrangement. The polarization of the battery lasts fifteen minutes in tolerably uniform intensity, and the battery gives, when closed only four or five times in a minute and then immediately opened again, during this whole time equally strong sparks and shocks. When a solution of caustic potash is added to the dilute sulphuric acid in which the plates are dipped the result is very different; no secondary current is produced but there is a strong evolution of *ozone*. Plates of lead in dilute sulphuric acid gave as strong and lasting a secondary current as plates of silver. In this case the positive electrode became covered with a dense layer of peroxyd of lead; the negative electrode became blackish gray but no precipitate was deposited upon it; no ozone was evolved during the passage of the current. The addition of a solution of caustic potash to the sulphuric acid produces a strong evolution of gas and of ozone, but in this case also there is no secondary current. Plates of nickel gave a strong and lasting secondary current. Plates of zinc in a solution of caustic potash give off no ozone; the secondary current is like that of the other metals, but the decomposing cell is soon rendered useless by a heavy white precipitate.

[*Note.*—It is to be hoped that the value of the magneto-electric machine in telegraphing operations will receive in this country the attention which is certainly due to it.—W. G.]—*Pogg. Ann.*, xcii, 1, *May*, 1854.

2. *Galvanic reduction of metallic chromium.*—Bunsen has communicated the results of some further investigations in electrolysis and has shown how chromium, manganese, and several other metals may read-

ily be reduced in small quantities from solutions of their chlorids. The author in the first place has found that the *density* of the current exerts a most important influence on the nature of the products of the electrolytic action; by the *density* of the current is understood the intensity divided by the surface at which the electrolysis takes place. The power of the current to overcome chemical affinities increases with this density. If for instance a constant current is passed through a solution of chlorid of chromium in water, it depends on the section of the reducing pole or electrode whether we obtain hydrogen, sesquioxyd of chromium, protoxyd of chromium, or metallic chromium. The relative mass of the constituents of the electrolyte exerts also a not less important influence. As the unit of measure for the density of the current, Bunsen assumes the current having the absolute intensity 1 distributed upon 1 square millimeter. The intensity of the current was measured by a Weber's tangent's-compass, and reduced to absolute measure by the formula $I = \frac{RT}{2\pi} \tan. \varphi$, in which R is the radius of the ring in millimeters, φ the deviation of the needle, and T the horizontal component of the earth's magnetism expressed in Gaussian units. The value of T was determined by the decomposition of water with the help of the electro-chemical equivalent of water. If A represents the quantity of water decomposed in t seconds by the current whose intensity is I, a being the electro-chemical equivalent of water, we have by Faraday's law, $I = \frac{A}{at}$, and this equation combined with the former gives $T = \frac{2\pi A}{at R \tan. \varphi}$. For the density of the current whose absolute intensity is I, the electrode having a section O measured in square millimeters, we have $D = \frac{I}{O} = \frac{RT}{2\pi O} \tan. \varphi$. Bunsen determines the quantity of water decomposed by the loss of weight of the flask in which the decomposition takes place; he finds that the formation of peroxyds of hydrogen may be easily and completely avoided by adding only a small quantity of sulphuric acid to the electrolyte, and keeping the decomposing cell during the experiment at a temperature above 60° C. The error which arises from the catalytic recombination of the two gases upon the platinum plates may also be completely avoided by first amalgamating the plates and then igniting them till the mercury is completely expelled. In overcoming powerful affinities the author uses a decomposing cell, one pole of which consists of the inner surface of a carbon crucible filled with chlorhydric acid, placed within a porcelain crucible and kept hot in a water-bath. A small earthenware cell within the carbon crucible serves to contain the fluid to be decomposed. A narrow strip of platinum dips into this fluid and serves as the second battery pole, the current being compressed upon this to a great density. With this apparatus and a solution of the mixed proto- and sesqui-chlorids of chromium, metallic chromium is easily reduced in leaves having a surface of 50 square millimeters. These are very brittle, and where they adhere to the platinum have a bright metallic lustre: in ex-

ternal appearance chromium resembles iron, but resists better the action of moist air, and on heating in the air burns to sesquioxyd of chromium. Chlorhydric and sulphuric acid dissolve it with difficulty, forming protochlorid and sulphate of protoxyd of chromium. Nitric acid, even when boiling, scarcely attacks it: its density corresponds almost precisely to that deduced from the atomic volume of the metals of the magnesia group. In one experiment the reduction of the metal took place when the density of the current was 0·067: when this density is diminished a point is soon reached when no metal is reduced, but when there is an abundant production of a combination of the two oxyds of chromium as an almost black uncrystalline powder. Chlorid of manganese is decomposed like chlorid of chromium; the metallic manganese is obtained in large brittle leaves which oxydize in the air almost as easily as potassium. When an amalgamated platinum wire is employed as the reducing pole of the battery in concentrated boiling solutions of the chlorids of barium, calcium, &c. the density of the current may be increased to 1, and even beyond that. In the solution of chlorid of calcium acidulated with muriatic acid, the wire becomes covered with a gray layer of calcium which contains but little mercury. The reduction of calcium is however difficult; that of barium is much easier, and masses of amalgam weighing 1 gramme are easily obtained. This amalgam is solid, silver white, and very crystalline: heated in a current of hydrogen it leaves a dark porous mass, in the cavities of which silver white metallic surfaces are visible. The fused chlorids of barium, strontium and calcium are not reduced by the galvanic circuit like the chlorid of magnesium. The chlorid of calcium, even when fused, obstinately retains water, and thus the negative pole becomes covered with a non-conducting layer of lime. When melted tin is made to form the negative pole, an alloy containing from 8 to 12 per cent. of calcium is easily obtained. The author proposes to continue these experiments with other compounds than the chlorids.—*Pogg. Ann.*, xci, 619, *April*, 1854.

[*Note.*—The illustrious German chemist does not appear to have been acquainted with the experiments made in this country by Dr. Hare on the reduction of Barium, Strontium and Calcium by the galvanic battery, as these are not alluded to in his memoir. The discovery of the influence of the *density* of the current upon the intensity and character of the electrolytic effects is undoubtedly one of very great importance. It appears reasonable to suppose that this influence is also very powerful in the battery cells themselves, as well as in the decomposing cells. In those batteries in which, like Grove's and Daniell's, two metals and two liquids are employed, and in which a reduction takes place at the surface of the negative metal, it would seem that such dimensions should be given to the surface of the negative metal that the electric density shall be exactly sufficient to produce the particular chemical effect required at that surface, whether reduction of copper or oxydation of hydrogen. The writer would furthermore suggest that the explanation of the remarkable effects of polarization produced by the magneto-electric machine and mentioned in the previous abstract, is to be found in the peculiar *density* of the magneto-electric current, under the circumstances mentioned, and not merely in its discontinuous

character. *It is not improbable that the density of the current required to produce a particular chemical decomposition will serve as an accurate and available measure of the force of chemical affinity under various circumstances of temperature, mass and pressure.*—W. G.]

3. *On the losses of weight which minerals undergo by heat.*—H. St. Claire Deville and Fouqué have communicated to the Academy of Sciences a memoir on this subject containing results which if confirmed will prove of much importance for mineral chemistry. The losses of weight which minerals experience by heat may arise from the presence of water of fluorine and of boron. The authors assert that the temperature at which the water is expelled from a mineral lies far below that at which the fluorine begins to volatilize. They employ therefore two lamps, one fed with a mixture of alcohol and oil of turpentine, the other a blast lamp in which the vapors of oil of turpentine are consumed. The former perfectly expels the water without a trace of fluorine; the latter completely drives off the fluorine. The nature of the loss of weight which a mineral containing fluorine undergoes depends upon its constitution. The authors prepared a basic silicate of soda, and fused it over the large lamp, with a weighed portion of pure fluorid of calcium. In this case the whole of the fluorine was volatilized as fluorid of sodium, while a silicate of lime remained, containing the whole of the silica. A second experiment was made with topaz: this lost over the large lamp 23 per cent. of pure fluorid of silicon. The authors ingeniously showed this by placing the topaz in the centre of a combination of inverted concentric platinum crucibles, the intervening spaces being filled with lime. This system lost no weight on ignition and the lime was found converted into a mixture of silicate of lime and fluorid of calcium, the fluorine and silicon being in the ratio of 3 to 1 in equivalents. A silicate of alumina remained from the topaz which withstood the heat of melting platinum. The authors assume $4Al_2O_3$, $3Si(O, Fl)_3$ as the true formula of topaz, and explain the observed differences between the varieties of this mineral by considering fluorine as replacing oxygen in greater or less quantity. Between those minerals which lose fluorid of silicon and those which retain all their silicon, there is another class composed especially of minerals which contain lithium. Lepidolite gives over the large lamp a very intense red flame which proves that lithia is volatilized, and explains the variations in those analyses in which lithium and fluorine have both been determined after ignition.—*Compt. Rend.*, xxxviii, 317. W. G.

4. *Illustrations of Chemical Homology.*—Under this title Mr. T. Sterry Hunt presented a paper to the American Association at Washington in May last. It was an extension of some of the views already advanced by him in this Journal for March and September, 1853, and discussed many points with regard to the homologies of organic and mineral species. He considers the neutral and basic salts of an acid and base, as members of a homologous series. Thus the three nitrates of lead in the ordinary notation are, PbO, NO_5; $HO, 2PbO, NO_5$; and $HO, 4PbO, NO_5$. Doubling these we may write them, $(Pb_2O_2)N_2O_{10}$; H_2O_2, $(Pb_2O_2)_2.N_2O_{10}$; and H_2O_2, $(Pb_2O_2)4.N_2O_{10}$; so that, excluding the water, the common formula of the lead nitrates becomes

$(Pb_2O_2)_n . N_2O_{10}$. These salts vary in solubility and in physical characters, but resemble each other in yielding nitric acid and oxyd of lead, as results of their decomposition, and are completely analogous to the homologous series of Gerhardt, which differ by $n(C_2H_2)$. From the relations between basic and hydrated salts, the same view is to be extended to the latter, and species differing by $n(O_2H_2)$, and by $n(O_2M_2)$, may thus be homologous. The above formulas are intended to involve no hypothesis as to the arrangement of the elements, for in the author's view, each species is an individual, in which the different species that may be obtained by its decomposition, have no actual existence.

He regards those silicates which like eudialyte, sodalite, and pyrosmalite, contain metallic chlorids, as oxychlorids, $=(M_2O_2)_n . M Cl$, and nosean, haüyne and lapis-lazuli as basic sulphates $= (M_2O_2)_n S_2O_6$, while cancrinite, and perhaps some scapolites, are basic carbonates. All other silicates are reducible to the same type as the spinels, $n(M_2O_2)$, the formula of silica being SiO, with an equivalent weight of $7 \cdot 1 + 8 \cdot 0 = 15 \cdot 1$. Boracic, titanic, niobic and tantalic acids are reduced to the same formula as silica.

Homœomorphous species have similar equivalent volumes, so that the density in species thus related, enables us to determine their comparative equivalent weights, and to fix their positions in a homologous series. The proportion between the silica and the other oxyds may vary greatly in related species, while the characters of the genus or the order are preserved. This is illustrated in hornblende, diopside, and aluminous pyroxenes like hudsonite, where the oxygen of the bases being represented by 8, that of the silica is respectively equal to 18, 16 and 9.

The triclinic feldspars, of which albite and anorthite may be taken as the representatives, furnish another example; the one is a lime feldspar, $\dot{C}a^8 \dddot{S}i + 8 \bar{\bar{A}}l \dddot{S}i$, and the other a soda feldspar, $\dot{N}a \dddot{S}i + \bar{\bar{A}}l \dddot{S}i^3$; multiplying the first by $\frac{8}{3}$, and the second by 4, and expanding, they are reduced to a common formula $M_{64}O_{64}$. Petalite, a lithia feldspar, also enters into the same formula, with a similar equivalent volume, while orthoclase belongs to a homologous genus, which is $M_{60}O_{60}$. The formulas with their equivalent weights, densities, and volumes, are as follows:

		Eq. wt.	Dens.	Eq. vol.
Anorthite . .	$(Si_{32}al_{24}Ca_8)O_{64} =$	$1118 \cdot 4 \div$	$2 \cdot 76 =$	$405 \cdot 0$
Albite . . .	$(Si_{48}al_{12}Na_4)O_{64} =$	$1054 \cdot 4 \div$	$2 \cdot 62 =$	$402 \cdot 4$
Petalite . . .	$(Si_{51}al_{10}Li_3)O_{64} =$	$983 \cdot 9 \div$	$2 \cdot 45 =$	$401 \cdot 5$
Orthoclase . .	$(Si_{45}al_{12}K_3)O_{60} =$	$1026 \cdot 7 \div$	$2 \cdot 56 =$	$402 \cdot 6$

The above formula of petalite requires silica 78·27, alumina 17·31, lithia 4·42 = 100·00, and that of orthoclase, silica 66·18, alumina 20·02, potash 13·80 = 100·00, which agrees with a large number of analyses, although there are varieties which appear to contain more alkali.

Between anorthite and albite, may be placed vosgite, labradorite, andesine, and oligoclase, whose composition and densities are such that they all enter into the same general formula with them, and have the same equivalent volume. The results of their analysis are by no means constant, and it is probable that many, if not all of them, may

be but variable mixtures of albite and anorthite. Such crystalline mixtures are very common; thus in the alums, aluminium, iron and chromium, potassium and ammonium, may replace one another in indefinite proportions, and the hydrated sulphates of copper and magnesian metals, are obtained in similar mixtures. Heintz has shown by fractional precipitation, that there are mixtures of homologous fatty acids, which cannot be separated by crystallization, and have hitherto been regarded as distinct acids. The author insists that the possibility of such mixtures of related species, should be constantly kept in view in the study of mineral chemistry. The small portions of lime and potash in many albites, and of soda in anorthite, petalite, and orthoclase, are to be ascribed to mixtures of other feldspar species.

The paper concludes with a view of the homologous and homœomorphous relations of the metallic oxyds, titanates, niobates and tantalates, and their affiliations with the silicates.

II. Mineralogy and Geology.

1. *Notice of von Kobell's paper on Series of Isomorphous and Homœomorphous Forms*, published in Schweigger's Jour., vol. lxiv, p. 410; by J. D. Dana.—In this early paper of von Kobell, published in 1832, (but which we had not the privilege of consulting till receiving quite recently a translation from Mr. G. J. Brush, now in Munich,) the subject of the relations of form among minerals of the Dimetric and also of the Hexagonal System is presented in a similar manner as regards the general principle, to that of the writer in the last volume of this Journal. Von Kobell aims to show that in the dimetric and hexagonal systems as in the monometric, there are many species related in form that are unrelated in composition. After some details with reference to certain analogous compounds, as Scheelite and Wulfenite, he enters upon a comparison of dimetric species differing widely in composition, and shows the similarity of form, of Meionite and Wernerite; Copper Pyrites and Braunite; Apophyllite and Anatase: to the group of Copper Pyrites, and Braunite, he afterwards adds Nagyagite, Corneous Lead, Idocrase, Uranite, Mellite; and to Apophyllite and Anatase the species Zircon, Rutile, native Calomel. Thus relations among the dimetric species are brought out, very similar to those presented by the writer in his recent paper.

Von Kobell next takes up the Hexagonal System, and shows the relations between the Corundum group (including, as first shown by Mitscherlich, Specular Iron and Ilmenite) and the Calcite group. The relation of the Calcite and Corundum groups is shown by comparing $\frac{1}{4}$R of Calcite with $-\frac{1}{2}$R of Specular Iron; and the rhombohedron of Copper Mica 68° 41′ is shown to be near -2R of Specular Iron; while again Red Silver Ore is observed to be near Calcite in angle, the arsenical variety giving the angle 107° 40′ to 107° 36′. Von Kobell also further remarks that "other hexagonal species may be brought into similar series, if differences of 1½ degrees be neglected. The series of quartz and corundum are united by beryl with the series of apatite and pyromorphite; magnetic pyrites and chlorite both have pyramids with the basal angle 120°." The fact of a relation in form among

hexagonal species is here exhibited, as in the writer's recent paper already referred to; but the relation is somewhat different in kind, and depends on less simple ratios, as is seen in the method of comparing Calcite and Corundum. The relations between hexagonal and dimetric species is not touched upon.

Von Kobell thence concludes that entirely different compounds may have closely similar or identical forms; and similarity of form is, therefore, not an indication of similar chemical composition. This important conclusion is also supported by a brief mention of some examples of similar import, in the trimetric and monoclinic systems, as that of Borax and Augite; Prehnite, Antimony Glance and Epsomite; Amphibole and Augite.

2. *On the Chloritoid of Bregratten in the Tyrol, and Clinochlore of Markt Leugast in Bayreut;* by Prof. VON KOBELL, (J. f. pr. Chem., 1854, lxii, 92, from the Gel. Anz. k. k. Bayr. Akad. d. Wissensch.)—The Bregratten *Chloritoid* has a blackish-green color and is associated with quartz. An analysis, made with special precautions, afforded Prof. v. Kobell:

	$\ddot{\text{Si}}$	$\overline{\text{Al}}$	$\overline{\text{Fe}}$	$\dot{\text{Fe}}$	$\dot{\text{Mg}}$	$\dot{\text{H}}$
	26·19	38·30	6·00	21·11	3·30	5·50 = 100·40
Oxygen	13·59	17·90	1·79	4·68	1·32	4·88

from which results he deduces the formula

$$(\dot{\text{Fe}}, \dot{\text{Mg}})^3 \overline{\text{Al}} + 2\overline{\text{Al}}\,\ddot{\text{Si}} + 3\dot{\text{H}} \quad \text{or} \quad \dot{\text{Fe}}\,\overline{\text{Al}} + 2\overline{\text{Al}}\,\ddot{\text{Si}} + \dot{\text{Mg}}^3\,\dot{\text{H}}^3.$$

The *Clinochlore* shows lines of cleavage parallel to a rhombic prism of nearly 120°, and also parallel to the shorter diagonal, and like the clinochlore of Pennsylvania, it is optically biaxial. B.B. fuses on the edges to a pale yellowish mass. In borax dissolves with intumescence, and slowly yields a chrome-green glass. Fuses imperfectly with soda to a yellowish mass. Decomposed perfectly by sulphuric acid, and imperfectly by muriatic. The analysis afforded von Kobell:

	$\ddot{\text{Si}}$	$\overline{\text{Al}}$	$\overline{\text{Fe}}$	$\overline{\text{Cr}}$	$\dot{\text{Mg}}$	$\dot{\text{Fe}}$	$\dot{\text{H}}$
	33·49	15·87	2·30	0·55	32·94	4·25	11·50 = 100·40
Oxygen	17·38	7·18	0·69	0·17	13·17	0·94	10·22

giving him the formula

$$3\dot{\text{Mg}}^3\,\ddot{\text{Si}} + 2\overline{\text{Al}}\,\ddot{\text{Si}} + 3\dot{\text{Mg}}\,\dot{\text{H}}^2.$$

This Clinochlore occurs in Serpentine.

3. *On the Alteration of Scapolite;* by G. v. RATH, (Pogg., xc, 288.)—This paper is a continuation of an extended examination of Scapolites by von Rath. The author here gives analyses of different altered Scapolites.

(1.) *Mica* after Scapolite, from Arendal, Norway. The altered crystals are 6 inches in length; they are covered with mica crystals, and consist of an aggregation of scales of the same. G. = 2·833. H. = 2·5. Analysis—

$\ddot{\text{Si}}$	$\overline{\text{Al}}$	$\overline{\text{Fe}}$	$\dot{\text{Ca}}$	$\dot{\text{Mg}}$	$\dot{\text{K}}$	$\dot{\text{Na}}$	$\dot{\text{H}}$	$\dot{\text{Ca}}\,\ddot{\text{C}}$
44·49,	24·91	4·84	2·14	0·36	6·71	1·11	3·44	11·11 = 99·11

whence the oxygen ratio for $\dot{\text{R}}$, $\overline{\text{R}}$, $\ddot{\text{Si}}$, 0·38 : 2·12 : 4. The analysis shows an addition of 6·71 of potash, not found in the unaltered Scapolite of Arendal, besides a removal of the larger part of the lime by carbonic acid, and also other changes.

(2.) *Yellow Scapolite* from Bolton, Mass. This is a massive, pale yellowish variety. G.=2·787. Analysis gave

$\dddot{\mathrm{Si}}$	$\overline{\mathrm{Al}}$	$\overline{\mathrm{Fe}}$	$\dot{\mathrm{Ca}}$	$\dot{\mathrm{Mg}}$	$\dot{\mathrm{K}}$	$\dot{\mathrm{Na}}$	$\dot{\mathrm{H}}$	$\dot{\mathrm{Ca}}\ddot{\mathrm{C}}$
49·99	23·01	1·64	8·85	1·78	7·09	0·35	4·28	7·80=99·19

Here also the addition of potash is large, being 7·09 per cent., and the composition is near that of Potash mica.

A *Red Scapolite* of Arendal, is next described. The analysis affords 4·42 of potash and 4·31 of soda. A *black scapolite* from Arendal afforded no alkalies and a large amount of magnesia, showing a removal of soda and an addition of the magnesia. Another from the *same* locality, Arendal, has the constitution of epidote.

[The yellow scapolite of Bolton is very similar in composition to the Algerite of Hunt, and sustains the view of Whitney that Algerite is a result of the alteration of Scapolite. It is a singular fact that Bolton alone has afforded to analysts the oxygen ratios for $\dot{R}$, $\overline{R}$, $\dddot{Si}$, as follows:

1 : 2 : 4   1·2 : 3 : 4   1·2 : 2½ : 4   1 : 2½ : 4   1 : 3 : 6   1 : 2½ : 6   1 : 3·9 : 9

a wide variation of composition for a single locality, but readily explicable when the ease with which scapolite undergoes alteration from atmospheric agencies or infiltrating waters is considered.—J. D. D.]

4. *Report on the Salt and Gypsum of the Preston Salt Valley of the Holston River, Virginia;* by Prof. H. D. Rogers. Boston, 1854.—This is an interesting topographical, geological and economical shetch of the saliferous deposits of southeastern Virginia, embraced in a rich valley of the Apallachians in Washington county. The deposits of salt, salt water, and gypsum occur on the line of an extensive and profound dislocation of the strata parallel to the main mountain ridges and to the trend of the plain which they enclose. The soil of the valley is wet and peaty; and beneath it to an enormous depth, there appears to be no solid rock, but a deposit of clay and earth, imbedding in places, large bodies of rock salt and of gypsum and saturated in its lower portions with highly concentrated brine. The vertical movement or heave of the rocks along the line of the fracture before mentioned is excessively great, inasmuch as the strata on the southeastern side of the fissure belong to the great Auroral magnesian limestone, the lowest of the Appalachian limestones, equivalent to the Cambrian or lowest fossiliferous system of England, while those on the other or northwestern in immediate contact with them are the saliferous and gypsiferous beds of our Appallachian umbral series, the near representatives in age of the European carboniferous limestone, and in original position of horizontal stratification removed by many thousand feet of interposed deposits from the older lower masses, into contact with which they have been forced by the heave along the fracture. The author in connection with Prof. Wm. B. Rogers, has made approximate measurements of the Preston salt valley, from which he infers that this vertical displacement by which the older Auroral strata have been inverted from the southeast upon the umbral beds, amounts to not less than 8000 feet.

Five productive wells are now in use in this valley, bored to the moderate depth of 200 to 300 feet, and four inches diameter. In the case of one well, over 300 feet of rock salt, divided by a little clay, were passed through without tapping any brine at all. The brine rises in the

well to within 45 feet of the surface, from which depth it is lifted by pumps and distributed to the evaporating houses by wooden logs. The quantity of pure salt now produced is 300,000 bushels per annum, worth about 50 cents. This is the present demand; vastly larger quantities can be supplied when there is a market. It appears from the statements of Prof. Rogers that the brine of these salt wells is stronger and purer than any other brine known in the United States. The usual proportion of salt in it is about 23 per cent.; eighteen gallons yielding one bushel of salt. It is entirely free from the chlorids of calcium and magnesium, and as a consequence of this, and of the absence of iron, the salt is dry and colorless—no appreciable quantity of *bittern* being produced in the evaporation. Gypsum is the only foreign ingredient present in any notable quantity, and this is easily and completely separated in the process of evaporation.

Prof. Rogers estimates the quantity of merchantable gypsum fit for agricultural and other uses, and within 60 feet of the surface, as determined by numerous borings, to be not less than 500,000 tons. It occurs, as this mineral usually does, in detached masses, often of considerable extent, but forming no part of the regular bedding of the adjacent rocks. Three beds of *anhydrite* have been developed on the Preston estate, confirming the view that volcanic steam and thermal waters have been concerned in the production of the gypsum beds. In view of all the facts relative to the actual mode of occurrence of the deposits of gypsum and anhydrite, along the line of this great fissure, and of the known analogies of existing hot springs, Prof. Rogers says: "may we not even venture to imagine that the several gypsum-containing plains or valleys along the line of the great fracture, the Preston salt and plaster valley in particular, were so many basins, filled at times with turbid water kept heated and replenished by constant or by intermitting boiling springs and jets of volcanic steam charged with salt and sulphate of lime extracted from the rocks." Prof. Rogers sustains this probable suggestion by several sufficient chemical reasons, and ingenious arguments.

A report on the Geology and mining resources of that part of the Lackawanna Coal Basin which includes the lands of the Delaware, Lackawanna and Western Railroad Company, and those of the Lackawanna Iron and Coal Company in Pennsylvania, by the same able author, we are compelled to pass without further notice, although an abstract of its contents would not fail to interest and instruct our readers. Twelve overlaying beds of workable coals are described, with an aggregate least thickness of 54 feet, yielding 37½ feet of good coal, with a yield of 42,000 tons to the acre. Upon this solid foundation of prosperity, accompanied with abundant good iron ores and other minerals, the manfacturing town of Scranton has been built, and on a scale of magnitude unexampled hitherto in the United States.

5. *The Metallic Wealth of the United States described and compared with that of other Countries;* by J. D. Whitney. 8vo, pp. 510. Lippincott, Grambo & Co. 1854.—We have read this volume with pleasure and no small share of instruction. While, as its title implies, the main object of the author is to unfold the metallic resources of the United States, he has given us by way of comparison a connected and

as far as space and facts would permit, a complete view of the same resources in all countries. The plan of the work is simple and methodical. Each metal, beginning with gold and ending with iron, is taken up, and after a discussion of the mineralogical and geological occurrence of its ores, their distribution throughout the world is described, with more or less fullness, according to the importance of the subject. Carefully prepared statistical tables follow, showing the annual production both in quantities and values for all countries where it has been possible to procure accurate information. The work bears marks of diligent research both in the archives of mining and in the mineral districts themselves. A vast mass of valuable facts has been methodised, conflicting statements have been explained or set aside and the maze of local terms for values, quantities and qualities has been translated into intelligible English.

An introductory Chapter of 77 pages is devoted to a concise and clear explanation of the laws which characterize the deposits of the metals and their ores, and a brief description of the general methods followed in mining operations. This is a most useful chapter, and will be read with instruction by all who feel any interest in this great branch of our national wealth. In it the technical laws of the miner's art are explained, and clear ideas are given as to the various modes of occurrence of the useful ores and the importance in all mining operations of carefully recognizing these distinctions in their practical bearing.

Each mine in the United States is taken up under its appropriate head and described as completely as the materials which could be collected by personal examinations and from official or reliable published accounts would allow. In doing this the author does not withhold his opinions wherever he has deemed it advisable to express them. He alludes to the value of some of our metalliferous deposits with a degree of candor and sincerity which must command respect even where it fails to convince those of an opposite opinion. His remarks in the Introduction, upon the false and fraudulent schemes by which the public are deceived or swindled and the whole subject of mining brought into disrepute, will meet with universal approbation.

The Chapter on the Copper mines of Lake Superior contains a very complete summary of the progress and present position of the most remarkable mining development ever made in the eastern United States. The remarks on the present and future value of this region derive great additional interest from the intimate personal knowledge of the author respecting this remarkable region. The same may be said of the Chapter on the Lead districts of Wisconsin and Missouri, which are regarded as having reached their point of highest development and not as possessing in any respect the permanent character belonging to true veins. The data respecting the Iron manufacture of the world are particularly full and valuable. It is not possible in a short notice to review the contents of Mr. Whitney's able volume with such detail as to make them fully known. It is a book to be studied and will be on the table of every person who, from whatever motive, may desire to know our resources in metallic wealth.

A general summary at the close of the volume is given, accompanied by a tabular statement of the estimated amount and value of metals

produced throughout the world in 1854. The metals selected are gold, silver, mercury, tin, copper, zinc, lead and iron. The aggregate of these are as follows:

Gold.	Silver.	Mercury.	Tin.	Copper.	Zinc.	Lead.	Iron.
lbs. troy.	lbs. troy.	lbs. av.	tons.	tons.	tons.	tons.	tons.
481,950	2,965,200	4,200,000	13,660	56,900	60,550	133,000	5,817,000

The product of the United States in gold is set down at 200,000 pounds, Australia and Oceanica at 150,000, and Russia at 60,000, Mexico and South America 47,100. Of silver, the New World supplies 2,473,700, pounds, leaving only the small residue of 491,500 lbs. for all other countries. Of mercury, Spain gives the world 2,500,000 lbs. and the United States 100,000 lbs. England and Australia furnish over half of all the copper produced by the world: the present product of the United States being in this metal only 3,500 tons. Prussia and Belgium furnish four-fifths of all the zinc used in the world (viz. 16,000+33,600 tons.) Lead is distributed between Great Britain, Spain and the United States in the ratio of 4, 2, 1 (viz. 61,000, 30,000 and 15,000 tons each.) England furnishes more than half the Iron of the world, 3,000,000 tons, and the United States 1,000,000 tons. France is the next most productive country in iron, 600,000 tons. Russia produces but 200,000 tons, and Sweden 150,000 tons, quantities bearing a very small relation to the celebrity of product of those countries.

The following table exhibits the comparative value of the metallic productions of different countries, from which may be seen the ratio of their production, as compared, first, with that of this country taken as the unit, and, secondly, with that of Great Britain.

	Value of Metals produced.	Ratio of production to that of U. States.	Ratio of production to that of Gr't. Britain.
United States,	$79,827,000	1·	5·6
Great Britain,	96,169,800	1·205	·1
Australia,	39,428,000	·494	5·12
Mexico,	30,480,000	·382	1·3
Russian Empire,	25,240,000	·316	1·6
France,	15,252,500	·191	4·15
Chili,	13,144,000	·165	2·15
Rest of South America,	16,176,000	·203	1·6
Austrian Empire,	11,708,000	·147	1·8
Prussia,	9,680,000	·121	1·10
Belgium,	9,375,000	·118	1·10
Spain,	8,016,416	·100	1·12
Sweden and Norway,	5,460,896	·068	1·17
Saxony,	1,455,000	·018	1·67
Hartz,	1,147,588	·014	1·86
Italy,	832,500	·010	1·120
Switzerland,	375,000	·005	1·240

The great importance of our own metallic resources will be at once apparent from an inspection of the above table. It will be seen that we are second only to Great Britain in our production, as we are also in our consumption, of the metals. The two great Anglo-Saxon countries stand far before all others; and Australia, a colony of England of but a few years growth, is the next competitor on the list. As our production of gold which now forms so important an item of our metallic wealth, falls off, as it assuredly will, the deficiency may be more

than made up by the development of our resources for the production of other metals.

Mr. Whitney's work is certainly a most important addition to scientific literature, not merely in the English language but in all countries. It is a model of pure scientific style, and the mechanical part of the work is equalled only by the faultlessness of the proof-reading. In a pretty careful perusal of the whole volume we have failed to detect the first typographical error.

6. *City of San Salvador destroyed by an Earthquake.*—On the night of the 16th of April last, the city of San Salvador was completely destroyed by an earthquake. The population of the city in 1852 was estimated at 25,000. The following facts relating to the country and the catastrophe are cited from different sources.

The hills around the plain of San Salvador are covered with verdure, which, as the dews are considerable, keeps green through the dry as well as the rainy season. The city, with its white houses and churches, seemed, therefore, to be set in living emerald. About three miles to the westward of the city is the great volcano of San Salvador. The cone, which rises on the northern border or edge of the crater, is (approximately) 8,000 feet in height. The volcano proper, however, is a vast mass, with a broad base of irregular outline, its summit serrated by the jagged edges of the crater, and is much less in altitude than the cone. This cone seems to have been formed by ashes and scoriæ thrown out of the crater, which is represented as a league and a half in circumference, and a thousand *varas*, or three thousand feet deep—almost large enough to receive the whole mass of the volcano of Vesuvius. At the bottom of this crater is a considerable lake of water. Very few persons have had the temerity to venture into the chasm of the volcano, and none of these are likely, judging from the accounts which they give of their efforts, to repeat the undertaking. Two Frenchmen, who ventured down a year or two since, became exhausted and incapable of returning. They were rescued, with great difficulty, by a detachment of soldiers from the garrison.

The entire line of the coast range of mountains or hills, in San Salvador, bristles with volcanos. Thirty-five miles to the eastward of the city is the great forked peak of San Vicente, 10,000 feet in height; and thirty miles to the westward, on the same line, is the vast bulk of the volcano of Sta Ana with its dependent peak, the volcano of Isalco, which is in a state of constant eruption, and is called "El Faro de San Salvador," the Lighthouse of San Salvador. Besides these are numerous other volcanos, occurring, in conjunction with those just named, in the following order, commencing at the eastern extremity of the State:—San Miguel (active), Chinemeca, Sacatecoluca, Tecapa, Usulutan, San Vicente, San Salvador, Guasapa, Izalco (active), Sta Ana, and Apenaca, besides some others of less note, to say nothing of extinct craters, volcanic orifices, or extinct vents, which are now generally filled with water, constituting lakes without outlets, and of which the water is brackish. One of these, called "Joya," occurs about four miles to the southwest of the city of San Salvador.

It would be impossible to describe here the numerous active vents, emitting smoke, steam and sulphurous vapors, which occur at or near

the bases of some of these volcanos, and which are called "*Infernillos*," literally "little hells." There are, also, numerous other volcanic phenomena and results, of exceeding scientific as well as popular interest, but which it would exceed the scope of this article to describe adequately. In a word, it may be said with truth, that San Salvador comprehends more volcanos, and has within its limits more marked results of volcanic action, than probably any other equal extent of the earth. For days the traveller within its borders journeys over unbroken beds of lava, scoriæ and volcanic sand, constituting, contrary to what most people would suppose, a soil of unbounded fertility, and densely covered with vegetation.

San Salvador stands, or rather stood—for its destruction has been so complete as to justify the use of the past tense—upon a table land wholly made up of scoriæ, volcanic ashes, sand and fragments of pumice, overlying, to the depth of hundreds of feet, the beds of lava which had flowed from the volcano before their ejection. Those who have seen the scoriaceous beds, which buried Pompeii, can form an accurate idea of the soil on which San Salvador was built.

The channels of the streams are worn down to a great depth through this light and yielding material, and constitute immense ravines, which render the approaches to the town almost impassable, except at the places where gradual passages are cut down on either side, paved with stone, and sometimes walled, to keep them from washing out and becoming useless. Some of these approaches are so narrow that it is customary, when mounted, to shout loudly on entering, so as to avoid encountering horsemen in the passages, which are frequently so narrow as to preclude either passing or turning back. San Salvador has more than once owed its safety, in time of war, to these natural fortifications, which confounded the enemy with their intricacies and difficulties, while affording means of defense to the inhabitants.

The facility with which the soil above described washes away has been the cause of considerable disasters to San Salvador. During a heavy rain of several days duration, called a "*Temporal*," which occurred in 1852, not only were all the bridges which crossed a small stream, flowing through one edge of the town, undermined and ruined, but many houses destroyed in the same manner. One of the principal streets, extending into the suburbs, began to wash at its lower extremity, and the excavation went on so rapidly that no effort could arrest it. A considerable part of the street became converted into a huge ravine, into which the houses and gardens on either side were precipitated. The extension of the damage was guarded against, when the rains ceased, by the construction of heavy walls of masonry, like the faces of a fortification. How serious an undertaking this was regarded, may be inferred from the fact, that its completion was deemed of sufficient importance to be announced in the annual message of the President.—*Correspondence of the N. Y. Herald.*

The attention of the dwellers and sojourners upon the south-western part of that elevated plain which lies above the city of San Salvador, upon the 12th and 13th of April last, was forcibly called to a hollow, rolling, subterranean sound, which was repeated at intervals, and at

times continued several minutes without ceasing. It seemed to proceed from the mountain-chain, which extends southwesterly from the neighboring volcano, and forms a semi-circle. The awe-inspiring sound was most distinctly heard at Monserrat and at a little *hacienda* (farm) belonging to a German family, named Bogen, from East Prussia.

About 7½ o'clock on the morning of Good-Friday, (April 14,) two slight shocks of earthquake were felt at San Salvador, and in the immediate vicinity, succeeding each other with little interval, and followed some ten minutes later by one more severe. I saw the roof and walls of my little habitation trembling without at first perceiving the cause. "Es un temblos," said Martino, my young New-Spanish attendant, very quietly. He was a native of the country, and therefore accustomed to a phenomenon which fills the mind of an inhabitant of the North with such profound horror. The environs of San Salvador have a bad name throughout the country, on account of the frequent shaking of the earth, and the natives have given the region a name expressive of the fact. But though these slight shocks are constantly occurring, especially at the beginning and end of the dry season, (December and May,) there has never, since the memory of man, been any instance of these terrible catastrophes, which, as at Lima or Valparaiso, are expected about once in a century to overwhelm the city in total destruction. Beside, the volcano of Isalco, sixteen leagues south of San Salvador, being in constant activity, is considered as a sort of chimney, conducting off the vapors and liquid matter from the vast fires beneath, or, to quote Humboldt, as a safety-valve against destructive earthquakes.

The shocks continued throughout Good-Friday, with pretty regular intervals, about as often as two or three per hour, and having all the same direction, west-southwest to east-northeast. In this direction, at a distance of a short league from the city, and at an elevation of about 500 feet above it, is the great crater of Guscatlan. This crater seems to be of a more ancient formation than that of San Salvador, and is partly filled up by a lake. Here the shocks seem to originate, and not at the volcano.

In San Salvador, where the holy week is celebrated with all possible religious pomp, the people paid little attention to the earthquake that took place on Good-Friday, and were but to a very small extent hindered in their participation in the procession, and in their visits to the cathedral. Still, several times in the course of the day, as the shocks grew of greater force, the devout multitudes were seen flying from the holy halls, pale with terror, rushing in wild haste to the doors; their fear of the subterranean powers overcoming their faith in the celestial.

About 8½ o'clock P. M., the houses were shaken to their foundations, and the roofs began to crack. Walls were filled with fissures, the plastering fell from ceilings, and many tiles were thrown from the roofs. This shock lasted at least some eight seconds, with an undulating motion, and had the houses not been so exceedingly well adapted for resistance to earthquakes, they would probably have come down in masses. These houses are all low, very broad, and of only one story, the walls of loam-mud possessing considerable elasticity, and covered with flexible cane—no better construction being possible to meet the case.

Every body fled into the open air. An hour passed without further motion, yet most of the people resolved to put up their couches in their court-yards in the open air. The shocks continued more or less violent at intervals during the whole night; in the course of the twenty-four hours we counted forty-two distinct ones. On Saturday morning it became quiet again.

The capital of the State of San Salvador is situated at an elevation of 2100 Spanish feet above the Pacific, upon a most fertile plain, about 7 square leagues in extent, on the northwest side of which rises the volcano, hardly a league from the city. Seen from the town, the old fire-mountain forms a most beautiful cone, with a gently sloping summit, crowned to the highest peak with thick forest. The crater is perfectly well preserved, more than half a league in circumference, and partially filled with water. It rises about 1000 feet above the table-land on which it stands. The other hills, both those which belong to the volcanic range south, and those of the semi-circle above mentioned, rise not more than 1500 feet above the level of the plain.

There is no historical account of any period of activity in the volcano of San Salvador. There is a tradition, however, of an eruption of lava having taken place in 1659, which is said to have destroyed and covered with ashes the *pueblo* of Nehapa on the northwestern side. According to other traditions, this was no eruption of fire, but an overflow of mud from the crater.

Easter-Sunday was welcomed by the discharge of rockets and the music of the military bands, while the multitude moved in festive procession to hear high-mass in the cathedral. Most of the houses were beautifully decorated with pisang leaves and branches of palm trees. The "Sanctissimum" was carried in triumph through the streets. A long procession followed, and the senoras and senoritas displayed their most splendid toilets. In the afternoon, the grand procession of saints took place. Colossal statues of saints, carved in wood, and most luxuriously equipped in costly silk dresses, were carried from the churches through the streets; and wherever they chanced to meet, the processions stop to give the saints an opportunity to embrace. The multitudes greet these scenes with extravagant delight, and rockets by the hundred are sent rushing through the air. The good Catholic people devote themselves upon Easter-Sunday, first to religious exercises, then to cheerful enjoyment; and so the day concluded with music, fireworks, and banqueting.

Soon after 9 o'clock in the evening, came a severe shock, more powerful than the severest on Good-Friday, accompanied, during its entire continuance, by a hollow, rumbling noise. Walls tottered to their foundations, bricks and tiles fell to the earth, and many houses were rent with fissures. I was lying in bed, suffering under an attack of ague, and had fallen into a feverish slumber, from which the noise awakened me. At that instant a portion of the ceiling of my room fell, beating me upon my head and face, and for some minutes blinding me with dust. I sprang from my bed, and groped my way in the darkness to the door, which was unfortunately locked. I succeeded at length in finding it, and reached the court-yard in safety, where I found all the other inmates of the house assembled, crying and praying in a breath.

In a few minutes, though, the panic was over again, and one heard even laughter and joking at the sudden consternation and flight from the house. These frightful phenomena occur too often to rouse more than a passing anxiety, even when the shocks are of unusual strength. They seem to be content if their dwellings do not sink at once. Still, the inmates of the houses brought their beds into the open air, and opened the doors of their houses. My next neighbor, a young doctor, remarked, that probably no other severe shock would occur that night; to which a Catholic priest replied, that the house was old, the roof rotten, and caution was at all events commendable. The people of the house went in again, and with open doors returned to their Easter feast, the conversation for the next hour turning almost exclusively upon the horrible "temblos."

In the mean time, I, being sleepless, was looking out upon the nightly sky. The day had, as usual, been very warm; the thermometer rising at noon to 88° Fahrenheit. Heaps of cloud (cumulo-stratus) were piled up mountain-like about the declining moon, but at about 10 o'clock disappeared. The moon was now shining merrily through a clear and calm atmosphere, a few vaporous veils of cloud (cirrus or cirro-cumulus) only, still hung immovable about some points of the horizon. Nothing appeared in the atmosphere to announce any uncommon phenomenon.

Half an hour later, ($10\frac{1}{2}$ P. M.) came the frighful shock which laid San Salvador in ruins. It began with a loud noise and undulating motion, the ground moving as if shaken by a subterranean sea. This motion, with its accompanying subterranean thunder, in the same direction with the previous shocks, lasted some ten or twelve seconds. The cracking and falling of roofs made a roar through which the appalling sounds below could scarcely be heard. A colossal cloud of dust arose. The terror, the cries and lamentations of the people were beyond description. Then followed prayers and a universal, loud, wailing invocation to *Maria Sanctissima* and all the saints, and finally a low, lamenting, and supplicating song from thousands of voices rising simultaneously from all the places of refuge to which the multitude had fled for safety.

And now began a scene which my pen is unable to describe. How insignificant appeared now the most frightful points in my past life! how mean appeared all the episodes of war and revolution which I had witnessed in the Old World! There, one had to deal with known agencies, with adversaries of flesh and blood, and not, as now, with unknown powers of the depths of whose existence we hardly are aware.

The shocks continued, sometimes light and sometimes with fearful force, with but short intervals, throughout the night and the next day, at the evening of which their number amounted to 120. I can compare the awful rumbling noise attending them only to heavy discharges of artillery in some subterranean battle. Sometimes the noise was more of a rattling character, and the ground waved for minutes without a real shock. No one thought of goods and chattels; the people trembled still for their lives; the motion of the ground had opened it in all directions, and no one knew but that the next moment a yawning chasm would open beneath his feet and swallow him for ever. After each

succeeding shock the multitude changed their prayers, and called upon some new saint for help. But whether the saints did not hear, whether they could not or would not help, the earth continued to tremble, the subterranean artillery to roar. A few hours more, and the more resolute had become accustomed to the roar, and began to take measures for the public safety, the ravages of the Indians being feared.

About 1 o'clock in the morning, a gentleman of my acquaintance climbed over the ruins of my house into the yard to look after me. Seeing me somewhat better, he proposed a walk in the moonlight through the town. We went first to the market-place. The cathedral was still standing, but the town, I now saw, was involved in one general ruin; not a single house had resisted the last frightful earthquake! The cathedral—a more elegant than imposing building, of the last century—had to a certain extent sustained the shock. But its belfry had been thrown down, its porch was in ruins, its walls were cracked and full of fissures. All the other churches, save that of the old Franciscan convent, had suffered far more severely, and their interiors presented sad pictures of solitude and ruin, being covered with dust and rubbish from the fall of tiles and stones from the heavy roofs. Colossal statues had tumbled from their pedestals, and their splendid and gorgeous robes were dragged in the dirt. There they lay, utterly uncared for by the multitudes who the day before had carried them in triumphant procession. Life and property were at this moment of more importance than images, the worship of which had done so little to arrest the footsteps of the calamity. A wing and a newly finished tower of the university still stood, and strangely enough, the clock was still striking the hours with all due regularity. In the Episcopal palace the ceiling had given way, and the bishop, Don Tomaso Saldana, a man justly admired for his piety and virtues, had fared no better with his consecrated head than we profane. Señor Duenas, ex-President of the Republic, once a monk, then lawyer and diplomatist, and incontestably in capacity the first man in the country, was somewhat more seriously injured.

The streets were now deserted, save by military guards posted here and there, and we found our progress much impeded by the piles of ruin and rubbish. Inside the houses was the quiet of death. The people, fearing to remain even in the widest streets, had collected, high and low, rich and poor, and were seated upon the ground in the centers of the public squares. The stiff Spanish etiquette which generally so completely divides the several ranks of the population, was completely forgotten in this night of terror. Rich men and beggars joined their tears, their cries, their prayers, supplications and hymns, at each new shock of more than common severity.

Don José Maria San Martin, the recently elected President of the Republic, exhibited great presence of mind and resolution, and gave wise and energetic directions for the protection of property. At the corner of the cathedral we met the Franciscan monk, Don Estavan Castillo, a beloved acquaintance of ours, a member of one of the first families in the country. He is the most ingenuous and remarkable man whom I have met in Central America, extremely inquisitive, much given to philosophical speculation, and the possessor of one peculiarly

characteristic trait in common with Pascal; he delighted to talk of the great mysteries of the world, those upon which the thinkers and philosophers of all nations and ages have speculated in vain. Our last conversation on the blind ruling of the powers of Nature seemed singularly adapted to the scene which surrounded us now. The monk pressed my hand in silence. He, with a band of strong fellows, was engaged in a search for persons buried in the ruins, while the bishop and all the clergy of the higher ranks had fled to Cojutepeque. At day-break, already a hundred bodies had been drawn from the rubbish, but what the real number of deaths was I have not yet been able to learn. That several thousands were not killed, we must attribute to the caution inspired by the severe preliminary shock above mentioned; nor in all probability, would the writer of these lines, without that warning, have ever held the pen again, as the last principal shock brought down the heavy ceiling of his dwelling.

On Easter-Monday the rising sun illuminated a most sorrowful scene. The inhabitants, pale with fear and fatigue, were wandering at random through the town without shelter or place of rest. The greater part fled in the direction of Apopa and Cajutepeque, leaving every thing behind. Among the women, who were mostly in extreme négligée, I noticed the wife of the President, entreating her husband to flee also from this place of horror. The President, however, remained faithful to his duty, and continued to act with energy. Most of the State prisoners perished, owing to their inability to leave their rooms; and in one case where two were chained together, one was killed, and the other forced to bear his body until relieved of his horrid burden by some police officers. A court-martial held under a tent in the University-place, sentenced every thief caught in the act of stealing and proved guilty by two witnesses, to be shot.

The ruins of San Salvador no longer affording me shelter, I returned next morning to the *hacienda* of an acquaintance on foot, as all the mules had either been stolen or were fully employed by the merchants in removing goods, and therefore not to be procured for any amount of pay. During my walk thither there were four shocks, one of which was one of the severest of all, lasting about seven seconds, and being accompanied, as usual, by a loud detonating sound, just like the noise accompanying the vollies at Vesuvius, to one standing near the crater at a time when volumes of smoke and stones are hurled into the air. My convictions grow stronger every moment that the seat of this subterranean action is very near, and that the vapors and glowing masses of the depths are seeking some new outlet.

Every fugitive of whom we asked the reason of this hurried flight, going off as they did with no thought of even the most necessary articles of comfort, answered alike, that after the destruction of the town, the bishop had said, that the entire region around San Salvador would, "before another moon had passed, sink into the depths of the earth." This prophecy has not, however, been fulfilled. The new moon is shining upon the ruins of the town, and upon the haciendas scattered over the plain. But the earthquakes and the subterranean noises still continue, and upward of a hundred shocks are felt each day. In all probability the powers below are striving to burst the crust of the earth,

and build up a new chimney for their gases, vapors, and molten masses. Woe to him who lives within the reach of the new crater.

P. S.—A month has passed since the above sketch was written. The shocks have decreased in number and intensity, but still they are occasionally felt, and the sounds below are heard. None but the poorest of the population of San Salvador have ventured to return to their former habitations.—*M. Wagner, in N. Y. Tribune.*

III. Botany.

1. *Hooker's Icones Plantarum*, vol. x, plates 901–1000. (London, Pamplin, 1854.)—This volume closes an important work, which, like most botanical publications not of an elementary character, in England especially, has been carried on at a heavy pecuniary sacrifice on the part of the liberal and distinguished author. This last volume is exclusively devoted to Ferns, can be purchased separately, and makes a *pendant* to Sir William Hooker's Species Filicum, being of the same 8vo size. Three of our own rare Ferns are figured in this volume; viz., *Asplenium pinnatifidum*, Nutt.; *Anemia Mexicana*, Klotzsch (No. 572 of Lindheimer's Texan collection, and No. 826 of Wright's); and the pretty little *Trichomanes Petersii*, described last year in this Journal. A. G.

2. *J. D. Hooker's Flora of New Zealand*, part 6, published in June, completes the account of the true *Mosses*, by Mr. Wilson, and contains the greater part of the *Hepaticæ*, which are elaborated by Mr. Mitten. Thirteen more plates are devoted to Musci and Hepaticæ, and about six species are illustrated on each plate. The remaining plates are devoted to Fungi and Algæ. A. G.

3. *Genera Plantarum Floræ Germanicæ Iconibus et Descriptionibus Illustrata.*—This well-known work, begun in 1833 by the late Th. Nees von Esenbeck, continued after his death by Prof. Spenner, after his death by Putterlick and Endlicher down to fasc. 24, and since their decease suspended for several years, is now "conjunctis studiis plurium auctorum continuatum." We have seen nothing of fasc. 25 and 26, but are told that they were issued a few years ago, edited by Prof. Schnizlein. Fasciculus 27, published in 1853 by R. Caspary of Berlin, has come to hand, and comprises 16 genera of *Cruciferæ*, one of *Ranunculaceæ* (Caltha), and 2 of *Papaveraceæ*. The letter-press is for the most part increased to four pages for each genus, and one or two genera have two plates devoted to their illustration. The plates, 20 in number, are crowded with details, particularly of the ovules, ovary, and embryo, and are apparently extremely faithful, but not very elegant, owing to the style of engraving adopted. Fasc. 28, by Prof. Schnizlein of Erlangen (1854), is devoted to several of the earlier monopetalous orders: the letter-press and the plates accord with those of the later preceding parts, and have none of the elaborate details of development given by Caspary. There is some prospect that this valuable work will now go on steadily. We wish we could say as much for the similar undertaking in our own country. A. G.

4. *Floræ Danicæ Supplementi fasciculus I*, 1853.—This great national work, carried on from 1761 to 1845, and comprising 14 folio

volumes with 2460 plates, is to be extended, at the sugg
Fries, so as to embrace Swedish and Norwegian plant
figured. Professor Liebmann is the editor. The 1st fas
supplement contains 60 plates, a great part of them c
plants, very faithfully represented. Almost all the speci
to the boreal American flora; which gives to the work a
est in this country.

5. *The Micrographic Dictionary*, by Drs. GRIFFITH
part II. (June, 1854) has been received from the enlighte
ful scientific publisher, Mr. Van Voorst (whose name,
most deservedly commemorated by a beautiful new gen
work or lace-like Algæ, recently discovered on the south
lon, by Dr. Harvey). The Introduction (pp. xxv–xl), h
a close, treats of *illumination* for the microscope, und
cumstances, and of the methods of *microscopical analys*
mining the structure of microscopic objects, from th
which they present under various conditions; of *chem*
and of *measurement*. The Dictionary, pp. 17–32, exten
analytic crystals. Of the engraved plates there are tw
Desmidaceæ, one of *Diatomaceæ*, one exhibiting the mic
ture of hairs, and one of *Infusoria*. Evidently the w
promise, and will be a vade-mecum of the microscopi
naturalist.

6. *Linnæa: ein Journal für die Botanik: herausgeg*
L. VON SCHLECHTENDAL, which was said to be suspen
on again with renewed spirit. The 26th volume (the 1
series) bears the date of 1853; but the 1st part was no
the cover properly informs us) until February, 1854,
April. The two parts received contain a monograp
group of *Stackhousiaceæ*, by T. Schuckardt; an elabor
on *Bouvardia*, and the first page of a critique upon the
cum and *Paspalum* in Steudel's new Synopsis Plantarum
by the editor; an attempt at a new arrangement of G
Hanstein; continuations of *Plantæ Mullerianæ* (Australi
Wagenerianæ Columbicæ, by various authors, and one
contributions. We hope this long-established and valu
Journal will now go on with regularity and success.

The *Botanische Zeitung*, a weekly Gazette, edited b
tendal and von Mohl, now in its 12th year, is well sustai
cipal articles are upon vegetable anatomy and developm
ing the pages of the Linnæa clear for memoirs and lo
systematic botany.

A rival German Botanical gazette, now in its secon
Bonplandia; Zeitschrift für die gesammte Botanik, ed
MANN, in London, but published at Hannover, a sheet o
ally more pages, in small folio, issued on the first and fi
month. It is handsomely printed, and is valuable for in
late there is some unpleasant controversy between it and
Zeitung, which is to be regretted.

7. *Annales des Sciences Naturelles*, etc., *redigée po*
par M. MILNE EDWARDS, *pour la Botanique par* MM. A

et J. Decaisne.—The twentieth volume closes the third series of this most important of natural-history periodicals; and a fourth series commences with the year 1854. The only fault to be found with the work is, that it is, as usual, behind date, only three monthly numbers having as yet appeared during the current year. But this is much better than in former times, when it had fallen almost a twelvemonth behind. Of the Botanical volumes, the most important articles within the last three years have been: Naudin's monograph of the Melastomaceæ of the Paris Museum, an elaborate revision of the family; Tulasne's papers on the *Antidesmieæ*, on the Ergot of Grasses, the reproductive organs of Fungi, and especially his full and incomparable memoir on the Organography of the *Lichenes* (embracing 240 pages of letter-press and 16 well-filled and admirable plates); Thuret's fine researches upon the antheridia of Cryptogams, especially of the Algæ, &c.; a series of very important papers by Trécul on the origin and development of ligneous fibres in the stems of plants, the mode of increase of dicotyledonous stems in diameter, and on the formation of leaves (in all which the author is winning a high reputation as a vegetable anatomist); some good papers by Garreau on what he calls, perhaps with good reason, the respiration of plants, he confining this term to the decomposition of vegetable products and the conversion of carbon into carbonic acid, which he shows to go on at all times, while the plant is effecting any transformations, although masked in the foliage, under sunshine, by the vast predominance of the opposite process (in which *vegetation* consists); an admirable investigation, by Boussingault and his assistant Léwy, of the composition of the air contained in a fertile or manured soil, showing that the roots of our esculent plants and grain, as ordinarily cultivated, are surrounded by an atmosphere which contains from 20 to more than 240 times more carbonic acid gas than the exterior atmosphere (the direct bearing of which against certain views too hastily propounded by Liebig, in former years, is abundantly evident); some good papers by M. Payen, on the organogeny of the flower in several families; and an article by Lucaze-Duthiers on the nature and development of galls and other abnormal productions of plants caused by the puncture of insects, &c.

The two fasciculi of the 1st volume of the fourth series contain some papers of descriptive botany; one by Groenland of Altona on the germination of certain Hepaticæ, beautifully illustrated; one by Trécul, elaborately investigating the formation of vessels and the so-called radicular fibres under adventitious buds, explaining quite differently from M. Gaudichaud the well-known facts which found their readiest explanation according to the theoretical view of the latter; this is illustrated by three admirable plates. M. Dareste endeavors to prove, with some success, that the red and yellow hues of the Chinese sea are due to the presence of a microscopic Alga, probably identical with the *Trichodesmium erythræum* of Montagne, from which the Red sea derives its occasional hue, and, it is thought, its name. A. G.

8. *Mammoth Trees of California.*—Some details may be expected, from the writer in the Sonora Herald cited in the last number of this Journal, respecting several large Coniferous trees in California. Mean while, it may be well to state, first: that the hollowed section of a

trunk exhibited last winter in Philadelphia, and which furnished the principal data of the estimate published in the May number (p. 440), was not taken, as we were led to suppose it was, from the famous giant "Wellingtonia" felled near the head of the Stanislaus river. We learn that it came from a less gigantic tree, which grew much nearer to San Francisco, and that the tree was a true *Redwood* or *Sequoia sempervirens.*

(2.) This tree, although considerably smaller, is apparently as old as, and probably older than the great Wellingtonia, although doubtless not surpassing the estimate already given. For, whereas its oldest wood exhibits about 48 layers to an inch, that of the Wellingtonia, of which we now have specimens, has as few as 25 or 20, or even fewer layers to an inch, at or near the circumference. Moreover, Dr. Torrey has recently had an opportunity to count the layers on a complete radius of the trunk of the famous Wellingtonia now exhibited in New York, and he finds that they are only 1120 in number! From the data which Dr. Torrey has furnished we find that, on the radius examined, the 1st hundred layers occupy a breadth of 17½ inches.

2d	"	"	"	14	"
3d	"	"	"	12½	"
4th	"	"	"	13	"
5th	"	"	"	16¼	"
6th	"	"	"	8¾	"
7th	"	"	"	7¾	"
8th	"	"	"	11	"
9th	"	"	"	10	"
10th	"	"	"	11	"
11th	"	"	"	11¼	"

The remainder of 20 layers occupies over 1 inch.

Eleven hundred and twenty layers on the semidiameter of 135 inches, or 11 feet 3 inches. We had ventured to reduce by more than one-third the accredited statement or estimate that this tree was 3000 years old. The facts show that the tree lacks almost three centuries of being half as old as it was said to be! Its enormous size is owing to its continued rapid growth rather than to any very extraordinary age.

(3.) A comparison of the cones of the two brings to view no adequate generic differences between the *Wellingtonia* of Lindley and *Sequoia.* Unless the male flowers furnish characters, the so-called Wellingtonia will hereafter bear the name imposed by Dr. Torrey, namely that of *Sequoia gigantea.* The flowers, however, are still a desideratum.

A. G.

9. *On the Fossil Plants found in Amber;* by Professor GOEPPERT, (Berlin Academy, Bulletin, 1853, pp. 450–476; Edinb. N. Phil. Jour., lvi, 365.)—Since Prof. Goeppert recognised the *Taxodites dubius* of Sternberg, which occurs abundantly in the plant-bed at Schosnitz, Silesia as the *Taxodium distichum*, Rich., now living in the southern parts of the United States and in Mexico, and found also some fossil Plants from Schosnitz to be identical with living species, thus pointing out the identity of some tertiary plants with the living, he has had the opportunity of examining a collection of 570 specimens of Amber, containing plant-remains, belonging to M. Menge of Dantzig, and 30 specimens

bequeathed by M. Berendt. With these the author has been enabled to raise the number of the species of plants in the Amber Flora from 44 to 163, of which only *Libocedrites salicornioides* and *Taxodites Europæus* occur fossil out of the Amber, and 30 are identical with existing species. The constitution of the Amber Flora, as at present known, is shown in the following table.*

	Number of Species.	Number identical with existing Species.
PLANTÆ CELLULARES.		
I. Fungi	16	4, certainly; perhaps all.
II. Algæ	1	1
III. Lichenes	12	6 or 7 (with species on the E. and W. coasts of Arctic America.)
IV. Musci hepatici: Jungermannieæ } 39 specimens	11	11
V. Musci frondosi	19	2 or 3, certainly; perhaps all.
PLANTÆ VASCULARES.		
III. Cryptogamæ (Acotyledones.)		
Filices.		
Pecopteris Humboldtana, *Göpp. & Behr.*		
IV. Monocotyledones.		
Cyperaceæ.		
Carex eximia, *Göpp. and Menge.*		
Gramineæ.		
Fragmenta.		
Alismaceæ.		
Alisma plantaginoides, *Göpp. & Menge.*		
V. Gymnospermæ.		
Cupressineæ	20	2
Abietineæ	31†	1
Gnetaceæ	1	
VI. Monochlamydeæ.		
Betulaceæ	2	
Cupuliferæ	10	
Salicineæ	3	
VII. Corolliflora.		
Ericineæ	22	3
Vaccineæ	1	
Primulaceæ	2	
Verbasceæ	2	1
Solaneæ	1	
Scrophularineæ	1	
Lonicereæ	1	
VIII. Choristopetalæ.		
Lorantheæ		
Crassulaceæ	1	1

The whole Flora as yet known includes 24 Families, 64 Genera; comprising 163 species.‡

The following are the general results of Prof. Goeppert's researches.

A considerable number of tertiary species of plants (especially *Plantæ cellulares*) are still living.

* For the lists of genera and species, see the works above referred to.

† Of these, eight (the species determined from the fossil wood) afford Amber.

‡ The number of species may probably be raised to about 180, by additions from about 50 specimens of which the relations are barely determinable.

The flora of the Amber being destitute of tropical and sub-tropical forms, it is to be referred to the Pliocene period.

The remains only of forest-plants have been preserved in the Amber. This flora much resembles the present, especially in the Cellular plants; the *Cupressineæ*, however, are now almost wholly wanting in our latitudes, and the *Abietineæ* and the *Ericineæ* are not abundant. The four species, of *Thuia*, *Andromeda*, and *Sedum*, which are identical with the living, are indeed northern forms; on the other hand, the *Libocedrus Chilensis* is found on the Andes of Southern Chili.

The flora of the northern parts of Europe, Asia, and America, is at present less rich in species of *Cupressineæ* and *Abietineæ* than that of the Amber, although it possesses some of the species found in the latter; nor are the existing northern species of *Coniferæ* so rich in resinous products as were the trees of the Amber Flora with which the *Dammara Australis* of New Zealand can alone in this respect be compared, the branches and twigs of this tree being stiff with white resin-drops.

If we take into consideration the enormous extent which the forests of

Abies alba,	Abies balsamea,	Abies ovata,	Larix Sibirica, and
—— nigra,	—— Sibirica,	Larix Dahurica,	Pinus Cembra,

at present attain in North America and Northern Asia, we are led to infer a similar extension in former times of the Amber-forests throughout the northern regions; to which, indeed, the wide distribution of amber in the late tertiary deposits of North America, Holland, North Germany, Russia, and Siberia to Kamtschatka, bears evidence.

If we judge from the proportion which the fir-forests bear to the trees of our northern flora generally, we shall infer, from the prevalence of the *Coniferæ* in the Amber, the existence of a very rich flora contemporaneous with the latter, and of which but a small part has as yet been presented to our notice. Germany contains 6800 species of *Cryptogamæ*, according to Rabenhorst, and 3454 species of *Phanerogamæ*, according to Koch. The proportions are—

	THE GERMAN FLORA.			THE AMBER FLORA.		
	Classes.	Species.		Classes.	Species.	
Cryptogamæ	8	6800		6	60	
	Families.	Species.		Families.	Species.	
Phanerogamæ	185	3454		20	102	
Cupuliferæ	..	12		..	10	
Ericineæ	..	28		..	24	
Proportion of trees and plants	$\left\{\begin{matrix}383\\3121\end{matrix}\right\}$	$=1:10$		$\left\{\begin{matrix}94\\9\end{matrix}\right\}$	$=10:1$	

Amber is never found isolated in large or small masses in the bituminous wood of the Brown-coal with resin-ducts of a single row of cells, which never contain yellow masses of resin, but only dark-brown transparent resin-drops, as in the *Cupressineæ* or the *Cupressinoxylon*, of Goeppert. The compound resin-ducts of the *Abietineæ* alone are filled with amber.

It is probable that the amber and its plant-remains have been drifted to the places in which they are now found. The author knows of no well-authenticated instances of the occurrence of amber in the Brown-coal formation itself; it occurs in the drift-beds above it, where, how-

ever, it does not appear to have originally belonged. Scheerer has found it in Norway; Von Brevern, at Gischiginsk in Kamtschatka; Rink, in Haven Island, near Disco Island, Greenland; and in these instances it is generally in drift-beds. The supposition, however, that it belongs to the Drift-period is difficult to substantiate, the flora of that period being as yet but little known. The stomach of the fossil Mastodon found in New Jersey contained twigs of *Thuja occidentalis* (found in the Amber-flora); and in the Erie Canal in New York State, at a depth of 118 feet there have been found freshwater shells, together with portions of *Abies Canadensis*, which still grows in the neighborhood, and leaves of which are still recognised (though with some doubt) in the amber. The fossil wood of the Drift-beds of Siberia, also, is nearly related to that of the present day.*

The height at which amber is found at the Castle on the Riesengebirge near Helmsdorf is nearly 1250 feet [German] above the sea level, and at Grossman's Factory near Tannhausen, at 1350 feet.

The amber is not derived from one species of wood only *(Pinites succinifer)*, as Professor Goeppert formerly thought, but also from eight other species, including the *Pinus Rinkianus*, in which Vaupelt observed the amber of Disco Island.

It is probable that all the *Abietineæ*, and perhaps the *Cupressineæ*, have furnished their share of the resinous matter (at first consisting of various specifically different resins) that afterwards by fossilization becomes amber; and this is supported by the author's experiments in the formation of amber from resin by the wet process, as in his experiments on the formation of coal from recent plants.†

In form the amber is either like drops, indicative of a former semifluid condition, or as the casts of resin-ducts and cavities. Large nodular masses occur, which must have been accumulated in the lower part of the stem or the root, as in the Copal trees.—*(Quarterly Journal of the Geological Society*, vol. x, No. 37.)

IV. Astronomy.

1. *New Comet*, (Gould's Astron. Journal, Aug. 11.)—A comet was discovered by Klinkerfues, at Göttingen, June 4, 1854. The following elements of its orbit were furnished by Prof. R. Keith, of the Washington Observatory, from observations at Bonn, June 11, and Washington June 26 and July 11.

Perihelion passage,	1854, June 21·7751.	
Longitude of perihelion, - -	273° 6′ 52″·4	Mean eqx.
" " asc. node, - -	347 40 8 ·0	1854·0
Inclination, - - - - - -	71 18 46 ·0	
Log. cos. φ, - - - - -	8·635714.	
Log. q, - - - - - -	9·811593.	
Motion	Retrograde.	

2. *Orbital Elements of Bellona and Amphitrite*, (Comptes Rendus, June 19, 1854.)—The following elements of the new planets *Bellona* and *Amphitrite* are computed by M. Oudemans, of the Observatory of

* See Quart. Journ. Geol. Soc., vol. vi. Part 2. Miscell. p. 66.—Transl.

† Ibid, p. 83.—Transl.

Leyden: the first from observations of March 1 at Bilk, April 12 at Berlin, May 26 at Leyden; the second from observations of March 1, at London, April 11 at Berlin, May 31 at Leyden.

Epoch, March 0, m. t. Berlin.

	Bellona.	*Amphitrite.*	
Mean anomaly, . .	39° 19′ 3″·13	127° 23′ 56″·9	
Longitude of perihelion,	117 23 5 ·6	54 4 26 ·2	} M. eqx.
" " asc. node,	144 57 56·3	356 15 54 ·6	} 1854·0
Inclination, . .	9 27 16·15	6 4 6 ·35	
Angle (sin = excen.) .	9 53 4·50	3 55 43·	
Log. mean daily motion,	2·882097	2·941143	
" semi-axis major,	0·445273	0·405909	

V. Miscellaneous Intelligence.

1. *New Process for Desulphurizing Metals.*—Dr. Homer Holland has presented a process for separating gold from ores of copper, iron, &c., with which it is found associated. It consists in the employment of nitrate of soda with heat. The ores in a state of fine division are mingled with pulverized nitrate of soda in atomic proportion to the sulphur of the ores. The reaction is between two atoms of oxygen from the salt and the sulphur to form sulphurous acid, which may be saved for making SO_3 in the ordinary chambers. The nitrogen escapes as an effete product, and sulphate of soda with sulphate of copper, oxyd of iron and metallic gold remain in the dry mass. A very gentle heat suffices to bring on the action. The resulting dry mass soon falls to powder under the influence of the air, and is lixivated to separate the soluble sulphates. The copper may be separated by the action of scrap iron, or if very abundant may be crystallized from the mother liquor as blue vitriol. The gold is left in the lixivium by this process in a condition remarkably adapted to speedy and perfect amalgamation by mercury, for which purpose any of the machines now in use may be employed. The application of this process upon a large scale will depend upon the ability to procure an ample supply of crude nitrate of soda, abundance of which is said to exist in the province of Iquique.* Dr. O. M. Lieber, who has published a report upon Dr. Holland's process, states that by it he obtained quantities of gold from the tailings of the best amalgamators, greater than had been previously separated from the entire ore. It is well known that the sulphurets of the North Carolina gold-bearing quartz are all auriferous, but the gold exists in such a state, whether mechanical or chemical, is perhaps hardly settled, that it cannot be removed by the ordinary use of mercury. In such cases Dr. Holland's process seems to be peculiarly valuable. Its use for ordinary copper ores appears to be less promising, owing to the present cost of nitrate of soda.

2. *Separation of Nickel and Cobalt by Wöhler's process*, (communicated by Francis E. Dakin, Albany University.)—Having a solution of nickel and cobalt in hydrochloric acid, precipitate by potassa, filter, redissolve in cyanid of potassium, add peroxyd of mercury, triturated in water, little by little till in excess, boil half an hour, filter, wash, dry

* See John H. Blake. this Journal, [1], vol. xliv, p. 1.

and ignite till all the mercury as well as the cyanogen is expelled, leaving pure oxyd of nickel. Precipitate the cobalt by hydrochloric acid, filter, wash, dry and ignite as before, and thus obtain the oxyd of cobalt.

3. *Researches on the Tides of the White Sea.* 2d Memoire; by M. TALYZINE, (L'Institut, No. 1044, Jan. 4, 1854, p. 7, St. Petersburg Ac. Sciences.)—In this Second memoir, the author applies himself chiefly to the rise and fall of water during the flood and ebb in the River Kuja, and indicates the mode of observation he adopted. By comparing observations and seeking to unite them by a formula, he has found that the oscillation of the water of the river is composed of three partial oscillations. The first of these has a period equal to that of the flood and the ebb; it may have a height of 30 inches. He calls it the semi-diurnal oscillation. The second has a period which is only the half that of the flood and ebb, it has a height of 5·8 inches. The third has only the third of the period of flood and ebb; it has a height of 3·8 inches. The high waters of the 3d oscillation happen later than those of the 1st by a very considerable quantity, whilst those of the 1st and 3d almost coincide. On tracing the curve representing the rise of water, a period of inflection is seen to belong to this line, which seems to indicate that in the phases of the tide which correspond to it, the water rises more slowly than elsewhere. But this inflection represents the phenomena, called (*manicha*) the stand, and which consists in this, that near the middle of the tide-wave the water ceases to rise and after remaining for some time at the same height, slowly falls. These phenomena are always attended by another which forms an exception to the ordinary flood and ebb. It is known in fact, that between headlands and at the mouths of rivers, the ebb lasts longer than the flood. This also takes place in the White Sea, in localities where there is no stand (manicha). But in localities where it exists the contrary takes place, the flood lasts longer than the ebb, and in the river Kuja this period is an entire hour. It is the 2d and 3d oscillations which cause this predominance of flood over ebb, and M. Talyzine seeks to decompose these influences. On examining the curves of projection, it appears that *the stand* should depend on the second oscillation; in fact, if the high waters of the 2d come later than those of the 1st there ought to be a stand, and if they come sooner this phenomenon should vanish. The examination of the curve of the 3d oscillation seems also to indicate a slackening in the rise of the water, but this delay differs from the stand, because the duration of flood equals that of ebb, for this oscillation has no influence on this duration. Finally by combining the curves of these two oscillations, it appears that the 3d increases the inflection of the 2d, that is, the delay in the falling of the water. From all these considerations, it results that the stand arises from the high waters of the 2d oscillation coming later than those of the 1st, and that the 3d increases the stand produced by the 2d. E. B. H.

4 *Application of the electric telegraph to the determination of the difference of longitude.*—On Nov. 25, operations were commenced for determining the difference of longitude between Brussells and Greenwich by electric telegraph. Three or four days sufficed for connecting the Brussells Observatory with the central station of that city, and to es-

tablish immediate correspondence by this station with the Royal Observatory of England. Some details on this subject follow.

Signals were made every evening between 10^h and 11^h, amounting to about 150 per hour. The first portion of these operations needed not to extend beyond three days; but as the state of the sky, especially in England, did not permit the making of the requisite astronomical observations for properly regulating the clock rates, a condition absolutely essential in this delicate enterprise, the operations were necessarily continued, and even probably prolonged until the evening of December 4, so as to give over 1200 signals. The astronomical observations at Brussells were very numerous, but while in that city the weather was magnificent, the sky at Greenwich was almost constantly overcast. M. Quetelet sent thither M. Bouvy, one of his aids, while Mr. Airy, Astronomer Royal of England, sent Mr. Durakin, one of his aids, to Brussells. The two aids are now to return to their respective posts, and will recommence a new series of electric signals and astronomical observations to eliminate the effects of *personal equations.* Every night, precisely at ten, Brussells mean time, four signals given at three seconds interval, announce that the English observers are at their post. Thirty seconds after, four like signals sent from Brussells, also announce the presence of the Belgian observers. At $10^h\ 1^m$, Greenwich begins to give signals at 10 or 16 seconds interval, but first making known the number of stars observed during the evening by as many 3^s interval beats. Brussells operates then in like manner from $10\frac{1}{4}^h$ to $10\frac{1}{2}^h$, when Greenwich resumes, and then Brussells at $10\frac{3}{4}^h$. The signals have never seemed to lose any of their intensity, in spite of the distance between Greenwich and Brussells; and considering the extraordinary velocity of the galvanic current, we may say that the needles of two galvanometers, placed in the two observatories, traverse simultaneously, and move parallel to each other.

M. Quetelet thought that in his communication to the Academy he ought to limit himself to these simple indications, leaving to Mr. Airy the care of discussing and publishing hereafter the aggregate of the observations undertaken at his request. E. B. H.

5. *Paris Observatory.*—Since the decree of reorganization of the Paris Observatory, several official acts of different dates, the last being March 2, have provided for two places as Astronomers, three as Adjunct Astronomers and three of Astronomical pupils. The following is the present composition of the Observatory personnel.

Director—M. Le Verrier.

Astronomers—M. Faye, M. Yvon-Villarceau.

Adjunct Astronomers—M. Babinet, M. Emile Goujon, M. Chacornac. M. Babinet also is charged with the meteorological observations.

Astronomical Elèves—M. Butillon, M. Reboul, M. Liais. E. B. H.

6. *On manufactured Sea-Water for the Aquarium;* by P. H. Gosse, A.L.S., (Ann. Mag. Nat. Hist., [2], xiv, 65.)—The inconvenience, delay and expense attendant upon the procuring of sea-water, from the coast or from the ocean, I had long ago felt to be a great difficulty in the way of a general adoption of the Marine Aquarium. Even in London it is an awkward and precarious matter; how much more in inland towns and country places, where it must always prove not only

a hindrance, but to the many an insuperable objection. The thought had occurred to me, that, as the constituents of sea-water are known, it might be practicable to manufacture it; since all that seemed necessary was to bring together the salts in proper proportion, and add pure water till the solution was of the proper specific gravity. Several scientific friends to whom I mentioned my thoughts, expressed their doubts of the possibility of the manufacture; and one or two went so far as to say that it had been tried, but that it had been found not to answer; that though it looked like sea-water, tasted, smelt, like the right thing, yet it would not support animal life. Still, I could not help saying, with the lawyers, "If not, why not?"

Experientia docet. I determined to try the matter for myself.

I took Schweitzer's analysis; but as I found that there was some slight difference between his and Laurent's I concluded that a very minute accuracy was not indispensable. Schweitzer gives the following analysis of 1000 grains of sea-water taken off Brighton:

Water,	964·744
Chlorid of sodium,	27·059
Chlorid of magnesium,	3·666
Chlorid of potassium,	0·765
Bromid of magnesium,	0·029
Sulphate of magnesia,	2·295
Sulphate of lime,	1·407
Carbonate of lime,	0·033
	999·998

The bromid of magnesium and the carbonate of lime I thought I might neglect, from the minuteness of their quantities; as also because the former was not found at all by M. Laurent in the water of the Mediterranean; and the latter might be found in sufficient abundance in the fragments of shell, coral, and calcareous algæ, thrown in to make the bottom of the aquarium. The sulphate of lime (plaster of Paris) also I ventured to eliminate, on account of its extreme insolubility, and because M. Laurent finds it in excessively minute quantity. The component salts were then reduced to four, which I used in the following quantities:

Common table salt,	$3\frac{1}{2}$ ounces.	
Epsom salts,	$\frac{1}{4}$ "	
Chlorid of magnesium,	200 grains	Troy.
Chlorid of potassium,	40 "	Troy.

To these salts, thrown into a jar, a little less than four quarts of water (New River) were added, so that the solution was of that density that a specifiic gravity bubble 1026 would just sink in it.

The cost of the substances was—sulph. mag. 1*d.*; chlor. mag. 3*d.*; chlor. pot. $1\frac{1}{4}$*d.*; salt, nil;—total, $5\frac{1}{4}$*d.* per gallon. Of course if a larger quantity were made, the cost of the materials would be diminished, so that we may set down 5*d.* per gallon as the maximum cost of sea-water thus made. The trouble is nothing, and no professional skill is requisite.

My manufacture was made on the 21st of April. The following day I poured off about half of the quantity made (filtering it through

a sponge in a glass funnel) into a confectioner's show-glass. I put in a bottom of small shore-pebbles, well washed in fresh water, and one or two fragments of stone with fronds of green sea-weed *(Ulva latissima)* growing thereon. I would not at once venture upon the admission of animals, as I wished the water to be first somewhat impregnated with the scattered spores of the *Ulva;* and I thought that if any subtle elements were thrown off from growing vegetables, the water should have the advantage of it, before the entrance of animal life. This too is the order of nature; plants first; then animals.

A coating of the green spores was soon deposited on the sides of the glass, and bubbles of oxygen were copiously thrown off every day under the excitement of the sun's light. After a week therefore I ventured to put in animals as follows:

2 *Actinia mesembryanthemum.*	*Coryne ramosa.*
7 *Serpula triquetra.*	*Crisia eburnea.*
3 *Balanus balanoides.*	—— *aculeata.*
2 *Sabella* ——?	*Cellepora pumicosa.*
2 *Sabellaria (alveolata?)*	*Cellularia ciliata.*
2 *Spio vulgaris.*	*Bowerbankia imbricata.*
1 *Cynthia (quadrangularis?)*	*Pedicellina Belgica.*

These throve and flourished from day to day, manifesting the highest health and vigor; the plants (including one or two Red Weeds that were introduced with the animals) looked well, and the water continued brilliantly crystalline. Within the succeeding month specimens of *Actinia mesembryanthemum*, *A. anguicoma* and *A. clavata*, a *Trochus umbilicatus*, and a *Littorina littorea* were at different times added.

Six weeks have now elapsed since the introduction of the animals. I have just carefully searched over the jar, as well as I could do it without disturbing the contents. I find every one of the species and specimens mentioned above, all in high health; with the exception of some of the Polyzoa, viz. *Crisia aculeata*, *Cellepora pumicosa*, *Cellularia ciliata*, and *Pedicellina Belgica*. These I cannot find, and I therefore conclude that they have died out; though if I chose to disturb the stones and weeds, I might possibly detect them. These trifling defalcations do in no wise interfere with the conclusion, that the experiment of manufacturing sea-water for the Aquarium has been perfectly successful. [Lime is important to many animals and had better be added.]

7. *Observations, economical and sanatory, on the employment of Chemical Light for artificial Illumination;* by Dr. E. Frankland, F.C.S., (Proc. Roy. Inst. of Gr. Britain, 1853, 319.)—There are two principal sources of artificial light, viz., electricity and the chemical force; the latter, however, has been, and still is, the only practical source of all artificial light. Although light can be thus obtained by the chemical action of substances belonging to all three kingdoms, yet closer observation demonstrates that the illuminating effect obtained from animal and mineral bodies is primarily derived from the vegetable kingdom; every plant being an apparatus for the absorption and concentration of light and heat from the solar rays, and for the retention of these forces during its passage through the subsequent stages in the formation of vegetable fuel.

Until the commencement of the present century artificial light was derived almost exclusively from the animal kingdom; but the great economy attending its immediate production from our vast stores of vegetable fuel is becoming more and more apparent, and is in fact so generally admitted, as to render more than a mere allusion to it and a glance at the following Table, unnecessary.

TABLE—showing the comparative cost of light from various sources, each equal to 20 sperm candles burning 120 grains per hour each, for 10 hours.

	s.	d.
Wax,	7	2½
Spermaceti,	6	8
Tallow	2	8
Sperm oil (Carcel's Lamp),	1	10
London gases, B, C, D, E,*	0	4¼
Manchester gas,	0	3
London gas, A,	0	2¼

We will therefore confine our attention principally to the light produced from vegetable fuel, in considering the economical and sanatory bearings of artificial light.

The production of artificial light depends upon the fact, that at certain high temperatures all matter becomes luminous. The higher the temperature the greater is the intensity of the light emitted. The heat required to render matter luminous in its three states of aggregation, differs greatly. Thus, solids are sometimes luminous at comparatively low temperatures, as phosphorus and phosphoric acids. (A jet of flame produced by the formation of these substances was exhibited, and its temperature shown to be inadequate to the ignition or even scorching of the finest cambric or gun cotton.) Usually, however, solids require a temperature of 600° or 700° F. to render them luminous in the dark, and must be heated to 1000° F. before their luminosity becomes visible in daylight. Liquids require about the same temperature. But to render gases luminous, they must be exposed to an immensely higher temperature; even the intense heat generated by the oxy-hydrogen blowpipe scarcely suffices to render the aqueous vapor produced visibly luminous, although solids, such as lime, emit light of the most dazzling splendor when they are heated in this flame. Hence, those gases and vapors only can illuminate, which produce or deposit solid or liquid matter during their combustion. This dependence of light upon the production of solid matter is strikingly seen in the case of phosphorus, which when burnt in chlorine produces a light scarcely visible; but when consumed in air or oxygen, emits light of intense brilliancy: in the former case the *vapor* of chlorid of phosphorus is produced, in the latter *solid* phosphoric acid.

Several gases and vapors possess this property of depositing solid matter during combustion, but a few of the combinations of carbon and hydrogen are the only ones capable of practical application: these latter compounds evolve during combustion only the same products as those

* London gases, A, B, C, D, E.—These are the gases furnished to consumers by five of the principal London Companies. For obvious reasons the names of the Companies are not mentioned.

generated in the respiratory process of animals, viz. carbonic acid and water. The solid particles of carbon which they deposit in the interior of the flame, and which are the source of light, are entirely consumed on arriving at its outer boundary; their use as sources of artificial light under proper regulations is therefore quite compatible with the most stringent sanatory rules.

In the usual process of gas manufacture there are generated in addition to these illuminating hydrocarbons two other classes of gaseous constituents, viz. impurities and diluents. With the exception of bisulphuret of carbon and some organic compounds containing sulphur, all the impurities are removed in the usual processes of purification, which have now been brought to great perfection; but the presence of these sulphur compounds in coal gas is very objectionable, and constitutes the chief barrier to the universal employment of gas in dwelling-houses. The attention of the manufacturer ought therefore now to be earnestly directed to the discovery of means for preventing the formation of these compounds, as it will probably be found impossible to remove them from the gas when once they have been formed.

In addition to traces of these sulphur compounds, purified coal gas contains only the following ingredients:

		Formula.
Illuminating constituents.	Olefiant gas,	C_2H_2
	Propylene?	C_3H_3
	Butylene?	C_4H_4
	Other hydrocarbons,	unknown.
Diluents.	Light carburetted hydrogen,	CH_2*
	Hydrogen,	H
	Carbonic oxyd,	CO

The light emitted during the combustion of coal gas is due entirely to the first or illuminating class of constituents, which yield an amount of light proportional to the quantity of carbon contained in a given volume; thus, propylene and butylene yield respectively 50 and 100 per cent. more light than olefiant gas, because they contain respectively 50 and 100 per cent. more carbon in a given volume.

It would not be desirable to employ a gas containing only luminiferous ingredients, even if it were possible to manufacture such a gas, because it is exceedingly difficult to consume these constituents without the production of smoke attendant on imperfect combustion. A diluting material is therefore necessary to give the flame a sufficient volume, so as to separate the particles of carbon farther asunder, and thus diminish the risk of their imperfect combustion.

All the three diluents above mentioned perform this office equally well; but if we study their behavior during combustion we shall find that in a sanatory point of view, hydrogen is greatly to be preferred.

The two objections most frequently urged against the use of gas in apartments are, first the heat which it communicates to the atmosphere, and, second, the deterioration of the air by the production of carbonic

* This gas has usually been described as possessing a certain amount of illuminating power, but a specimen of it brought from the coal strata beneath Chat Moss, Lancashire, showed that it yields no more light than hydrogen or carbonic oxyd when consumed from a *fish-tail* burner.

acid. Now, in their action upon the atmosphere in which they are consumed, the above three diluents present striking differences in these two respects.

One cubic foot of light carburetted hydrogen, at 60° F. and 30 in. barometrical pressure, consumes two cubic feet of oxygen during its combustion, and generates one cubic foot of carbonic acid, yielding a quantity of heat capable of heating 5 lbs. 14 oz. of water from 32° to 212°, or causing a rise of temperature from 60° to 80°·8 in a room containing 2,500 cubic feet of air.

One cubic foot of carbonic oxyd at the same temperature and pressure, consumes during combustion ½ a cubic foot of oxygen, generates one cubic foot of carbonic acid, and affords heat capable of raising the temperature of 1 lb. 14 oz. of water from 60° to 66°·6.

One cubic foot of hydrogen, at the same temperature and pressure, consumes ½ a cubic foot of oxygen, generates no carbonic acid, and yields heat capable of raising the temperature of 1 lb. 13 oz. of water from 32° to 212°, or that of 2,500 cubic feet of air from 60° to 66°·4.

This comparison shows the great advantage which hydrogen possesses over the other diluents, especially over light carburetted hydrogen, which is evidently a very objectionable constituent, and shows that a normal gas for illuminating purposes should consist of illuminating hydrocarbons diluted with pure hydrogen. No method is known by which a gas of exactly this composition can be manufactured, but a very close approximation has been lately made to this normal gas, by the employment of a process known as White's Hydrocarbon method of gas-making. In this process the very ingenious principle is adopted of generating the illuminating constituents in as concentrated a form as possible in one retort, and the diluents consisting principally of hydrogen free from light carburetted hydrogen in another. By this arrangement the diluents can be employed for a very remarkable and highly interesting purpose; they are conducted through the retort in which the illuminating constituents are being generated, in such a manner as rapidly to sweep out those constituents, before they have time to become decomposed by contact with the red hot interior surfaces of the retort, a mode of destruction which occurs so largely in the usual process of gas-making. This mode of treatment produces a gain in the amount of illuminating power derived from a given weight of coal, equal to from 50 to upwards of 100 per cent., whilst the increase in quantity of gas is frequently 300 per cent.

The gas thus manufactured differs principally from coal-gas made by the ordinary process, in having a large portion of the light carburetted hydrogen replaced by hydrogen; it is therefore in a sanatory point of view the best gas hitherto produced. This is seen in the following Table, which exhibits the amount of carbonic acid and heat generated per hour by various sources of light, each equal to 20 sperm candles burning at the rate of 120 grains of sperm per hour.

	Carbonic acid.	Heat.
Tallow,	10·1 cubic feet	100
Wax, Spermaceti,	8·3 " "	82
Sperm oil (Carcel's Lamp),	6·4 " "	63
London gases, B, C, D, E,	5·0 " "	47

	Carbonic acid.	Heat.
Manchester gas, - -	4·0 cubic feet	32
London gas, A, - -	3·0 " "	22
Boghead hydrocarbon gas,	2·6 " "	19
Lesmahago hydrocarbon gas,	2·5 " "	19

Notwithstanding the great economy and convenience attending the use of gas, and in a sanatory point of view, the high position which, as an illuminating agent, coal-gas of proper composition occupies, its use in dwelling houses is still extensively objected to. The objections are partly well founded and partly groundless. As is evident from the foregoing table, even the worst London gases produce, for a given amount of light, less carbonic acid and heat than either lamps or candles. But then, where gas is used, the consumer is never satisfied with a light equal in brilliancy only to that of lamps or candles, and consequently, when three or four times the amount of light is produced from a gas of bad composition, the heat and atmospheric deterioration greatly exceed the corresponding effects produced by the other means of illumination. By using a gas however of nearly the normal composition, such as the hydrocarbon gases above named, it is evident that three or four times the light may be employed, with the production of no greater heat or atmospheric deterioration, than that caused by wax candles or the best constructed oil lamps.

But there is nevertheless a real objection to the employment of gas-light in apartments, founded upon the production of sulphurous acid during its combustion: this sulphurous acid is derived from bisulphuret of carbon, and the organic sulphur compounds, which have already been referred to as incapable of removal from the gas by the present methods of purification.

The formation of sulphurous acid can readily be proved and even its amount estimated, by passing the products of combustion of a jet of gas through a small Liebig's condenser; the condensed product being heated to boiling with the addition of a few drops of nitric acid, and then treated with solution of chlorid of barium, yields a white precipitate of sulphate of baryta, if any sulphur compound be present in the gas.

These impurities, which are encountered in almost all coal-gas now used, are the principal if not the only source of the unpleasant symptoms experienced by many sensitive persons, in rooms lighted with gas. It is also owing to the sulphurous acid generated during the combustion of these impurities, that the use of gas is found to injure the bindings of books, and impair or destroy the delicate colors of tapestry. Therefore the production of gas free from these noxious sulphur compounds is at the present moment a problem of the highest importance to the gas manufacturer, and one which demands his earnest attention.

As it is nearly impossible for the consumer to procure gas free from these objectionable compounds, the only method of obviating their unpleasant and noxious effects is to remove entirely the products of combustion from the apartments in which the gas is consumed, and thus prevent them from mingling with the circumambient air. This suggestion was first made by Faraday, who, for accomplishing this object, contrived the very beautiful and effective ventilating burner exhibited in operation upon the lecture table. This apparatus, which is used at Buckingham Palace, Windsor Castle, the House of Peers, and in many

public buildings, may be truly said to have brought gas illumination to perfection; for not only are all the products of combustion conveyed at once into the open air, but nearly the whole of the heat is in like manner prevented from communicating itself to the atmosphere of the room. The only obstacles to the universal adoption of this description of burner are its expense, and the difficulty of conveying the ventilating tube safely into the nearest flue without injuring the architectural appearance of the room. The public at large will therefore still await the removal of the objectionable compounds in question, by the gas manufacturer, before they will universally adopt this otherwise delightful means of artificial illumination. E. F.

8. *Mechanical Action of Heat*; by F. A. P. Barnard.—*Prof. Dana*:—An apology is perhaps due from me to Mr. Rankine, for attributing exclusively to another, in my article on "Heated Air," sent you in November last, but published in March, a formula of thermo-dynamics which was first certainly presented by him in its most complete form, and which is as beautiful for its simplicity as it is practically useful. It may explain if not excuse my inadvertance, to remark that I but followed Mr. Joule in this respect, whose article on the "Economical Production of Mechanical Effect from Chemical Forces," republished from the Memoirs of the Lit. and Phil. Soc. of Manchester in the Lond. Edin. and Dublin Phil. Mag., for Jan. 1853, was necessarily referred to in the preparation of mine; and had been commented upon by Mr. Rankine himself in a letter to Mr. Joule, without his having noticed the error. Not having access to a complete series of the Transactions of the R. S. of Edinburgh, I had been obliged to send abroad for the volume containing Mr. Rankine's original paper, and had not received it, when my article was forwarded to you. Prof. Thompson's paper I had however read in the Lond., Edin. and Dublin Phil. Mag. for July, August and September, 1852; and also his demonstration of the formula in question in the Lond. Phil. Trans., Part I, of the same year. In the paper first mentioned, the formula is not only deducible from the expression cited by Mr. Rankine in the last number of this Journal,* viz.,

$$\frac{W}{H}=1-\varepsilon^{-\frac{1}{J}\int_{\tau_{\prime}}^{\tau''}\mu d\tau};$$

but it is actually *deduced*, by combining with the foregoing Mr. Joule's suggestion that "Carnot's Function" may vary inversely as the absolute temperatures, in expression (12) of the same article; which is, when W and H are taken as above to represent the total motive power gained, and the mechanical equivalent of the total heat expended, severally,

$$\frac{W}{H}=\frac{S-T}{\frac{1}{E}+S};$$

where S and T are the thermometric temperatures of the source of heat and of the refrigerator respectively, and E is the reciprocal of the number of degrees between the absolute and the thermometric zero. It is evident that this, when the symbols employed by me to denote

* There is a typographical error in this expression as given in the last number of this Journal.

absolute temperatures are substituted, becomes

$$\frac{W}{H}=\frac{\tau''-\tau_{\prime}}{\tau''}$$

which is the formula ascribed by me to Prof. Thompson.

A foot note, however, by the writer of the paper, had I sufficiently attended to it, would have informed me that Mr. Rankine was independently in possession of substantially the same formula as early if not earlier; for Prof. Thompson remarks that Mr. Rankine has communicated to him a formula for the ratio of power obtained to the mechanical equivalent of the heat expended, which agrees exactly with his own.

It may be remarked also, that Mr. Rankine had published his formula in the Lond. and Edin. Phil. Mag. as early as July, 1851, in a note to his letter to Poggendorff; and had reproduced it in the same journal in February and in June, 1853; so that I could hardly be unaware that he had independently originated it.

I am happy, in making this correction, to express my admiration of the ingenious theory suggested and so ably developed by Mr. Rankine, to account for the mechanical phenomena of heat. I regard the coincidence of the results deduced from premises so different as those employed by himself and Prof. Thompson, as one of the most beautiful examples of the consistency of truth which the history of science furnishes.

The necessity of a negative constant, represented by κ in the denominator of the formula as given by Mr. Rankine, for temperatures determined by the air thermometer, was pointed out by him alone. This constant, which becomes zero on supposition of the rigorous conformity of elastic fluid, at all temperatures, to "the gaseous laws," and which is always small, was neglected by me, partly because I took the formula directly from Prof. Thompson (Lond. Phil. Trans., Part I, 1852), and partly because Mr. Rankine had shown it to be too inconsiderable to be important to the purpose I had in hand.

Univ. of Alabama, July 7, 1854.

9. *Siluria, The History of the Oldest Known Rocks containing Organic Remains, with a brief sketch of the Distribution of Gold over the Earth;* by Sir R. I. MURCHISON, G. C. St. S., &c. &c. 524 pp. 8vo, with 37 plates of fossils. London, 1854. J. Murray.—A work of great value and interest.

10. *Elementary Chemistry, Theoretical and Practical;* by GEORGE FOWNES, F.R.S., late Professor of Practical Chemistry in University College, London. Edited with additions by ROBERT BRIDGES, M.D., Prof. Chem. Philad. Coll. of Pharmacy, etc. A new American from the last London edition, with numerous illustrations on wood. 555 pp. 12mo. Philadelphia, 1853. Blanchard & Lea.—This is a most excellent text-book for class instruction in chemistry, whether for schools or colleges. The death of Mr. Fownes left the recent revision of the work to the able chemists, H. Bence Jones and A. W. Hofmann of London, under whose auspices the last (4th) London edition was prepared for the press.

11. *Manual of Natural History for the use of Travellers*, being a Description of the Families of the Animal and Vegetable Kingdoms, with Remarks on the Practical Study of Geology and Meteorology; to which are appended Directions for collecting and preserving. By AR-

THUR ADAMS, M.R.C.S., F.L.S., &c., WILLIAM DALFOUR BAILIE, M.D., F.R.S.E., and CHARLES BARRON, Curator of the Royal Naval Museum at Haslar. 750 pp. 16mo. London, 1854. John Van Voorst.

12. *The Microscope and its application to Chemical Medicine;* by LOVELL BEALE, M.B. London, Prof. of Physiology and General and Morbid Anatomy in King's College, London. 302 pp. 12mo. *London*, 1854. Samuel Highley.—The value of the microscope to the medical man is beginning to be understood and appreciated. The Treatise of Prof. Beale is well calculated to instruct the student in the use of the instrument and in the methods of dissecting, preparing, and examining objects that may be of interest to him. After earlier elementary information, a chapter is devoted to the injecting of preparations; another to the modes of preparing tissues, the kidney, liver, &c.; others to examinations of the brain, nerves, nervous and serous membranes, morbid growths, etc, etc. The details are full and have special reference to the necessities of the medical man. The work is illustrated with a plate of crystalline substances, and numerous fine wood-cuts, among which are two large views of microscopes, and figures of the various tissues, cells, morbid growths or deposits, etc., described in the text.

We observe that in the remarks on the application of a goniometer to the microscope, it states that "the simplest method of measuring the angles of microscopic crystals is that of Schmidt." This Mr. Schmidt, is Prof. J. Lawrence Smith, late of the Virginia University, and now of the Louisville Medical School.

13. *Report on the Geology of the Coast Mountains and part of the Sierra Nevada*, embracing their industrial resources in Agriculture and Mining; by Dr. JOHN B. TRASK. Assembly Doc., No. 9. San Francisco, 1854.—A pamphlet of 96 pages, and not a result of a careful survey.

14. *Natural History Review.* Dublin, April, 1854. 104 pp. 8vo.—This new quarterly is announced to appear on the 1st of January, April, July and October. The April Number contains Reviews of works on Science, with notices of the contents of some prominent scientific periodicals in Zoology and Botany, and the Proceedings of Irish scientific societies. It promises to be a valuable work.

15. *Bibliographical Notices by M. Jerome Nicklès.*

Methode de Chimie par A. LAURENT, avec une notice par M. BIOT. 1 vol. in 8° de 460 pages. Paris, chez Mallet-Bachelier.

Théorie Générale des effets dynamiques de la Chaleur, par F. REECH, Ingenieur de la Marine, Directeur de l'école d'application du génie maritime.—S. Carnot published in 1824, a work entitled "*Reflexions sur la puissance motrice du feu.*" It contained some remarkable views on the manner of producing motive power with heat. The reasoning of Carnot was subjected by M. Clapeyron to mathematical analysis; both calculations and reasoning being founded on the absurdity of admitting the possibility of *creating* motive force or heat. The recent discoveries having deprived us of one of the properties considered fundamental by these authors, some writers, as Joule, Thompson, Rankine, Mayer and Clausius have applied themselves to the fuller investigation of what appeared inaccurate in the relations established by Carnot and Clapeyron.

Persuaded that in all these researches too much importance had been given to hypotheses, the logical train of reasoning of Carnot being lost

sight of, M. Reech has made the subject complete from a new point of view, establishing a formula in harmony alike with the results of M. Joule and those of M. Regnault. The work of M. Reech has excited much attention among mechanicians, and it will be studied with interest by all who are occupied with the mechanical properties of steam, warm air, etc. The volume is a quarto of 212 pages, published by Mallet-Bachelier, Paris.

The following works have also appeared at the same house:

Précis des œuvres mathematiques de Fermat et de l'arithmetique de Diophante par Brassine, Prof. à l'école d'artillerie de Toulouse. 1 vol. 8vo.—P. Fermat, inventor of the infinitesimal calculus, founder of the theory of numbers, published his collected memoirs in 1679. These remarkable works having become very rare, government made in 1843 an appropriation for their reprint; this project never having been carried out, M. Brassine has collected the principal works of Fermat, and added the correspondence between Fermat, Pascal, Digby, etc. The volume includes the statements of the propositions which enter into the six books of Diophantus, with the observations of Fermat which are very rich in fine theorems. This portion presents to teachers an excellent collection of arithmetical problems.

—— *Traité du calcul differentiel par l'abbé* Latrent. 1 vol. 8vo.—This valued work is indispensable to all who are studying the higher mathematics. It serves as an introduction to the treatises of Cauchy, Moigno, and especially the—

—— *Traité de mecanique par* M. Duhamel. 1 vol. in 8vo.—M. Duhamel, member of the French Institute, has been for a long time director of the studies of the Polytechnic School, where he still holds the professorship of mechanics with unquestioned ability. His treatise has been very successful among Professors, Engineers, the officers of artillery, and others.

—— *Uranographie ou Traité élémentaire d'astronomie par* Francœur. 1 vol. in 8vo. 6th edition.—This edition is posthumous, and is published by the son of M. Francœur. The father who was known among scientific men by his vast and varied attainments, was distinguished before the public for his rare talent in exposition. The Uranography partakes of this happy quality. Without requiring other calculations than those of arithmetic, and the elements of geometry, Francœur initiates the reader into the most delicate questions of astronomy, and even sets him to resolving important problems. The author has also in view the instruction of men of letters. Knowing that poets and orators of ancient time were well instructed in Astronomy, Physics, &c., he wished that our modern writers should be equally well taught in science, that they might not absurdly employ themselves with ideas suggested to them, which they did not understand, or like certain lawyers plead for a patent right on some chemical matter which they knew as little of as the judges. The first two parts of the book might be read with profit by all; the third part contains calculations, and is addressed to the learned in the science.

—— *Leçons de Cosmographie par* MM. Harant et Lafitte. 1 vol. in 8vo. of 188 pages.—This work has a less general end; it has been got up in view of the new programme of instruction which the government has imposed upon Colleges, Lyceums, and on candidates for ad-

mission to the polytechnic and naval schools, and the school of arts and manufactures. In spite of the narrowing restrictions, the authors have succeeded in giving a truthful sketch of history, and at the same time in delineating the successive developments of the human mind. The clear and methodical expositions which characterize the work, render it valuable to students and even to professors.

—— *Chimie photographique par* MM. Barreswill et Davanna. 1 vol. in 8vo, 287 pages.—The authors give the theory of photographic manipulations, the photographic processes on metallic plates, on paper dry and moist, upon glass prepared with collodion and with albumen; the manner of preparing the same; of employing the reagents, and of making the residues useful; the newest and most perfect methods; and finally they treat of engraving and lithography. With the aid of this book, the photographer, though little acquainted with chemistry, is enabled to purchase safely, and to employ judiciously, the best materials.

—— *Recherches cliniques sur les Eaux-Bonnes par* E. Cazenave. Pamphlet in 8vo. of 100 pages.—The author who is a physician at Eaux-Bonnes (Pyrenees) reports his observations and the results of his investigations on the diseases which belong to the vicinity of those waters. This work is especially interesting to physicians and naturalists.

—— *Des mesures propres à prévenir les collisions sur les chemins de fer, par* M. Couche, professeur a l'ecole des Mines de Paris, Pamphlet in 8vo, 48 pages. Paris, chez Carillan-Goeury.

—— *Des accidents sur les chemins de fer par* E. With. Pamphlet in 12mo. of 148 pages. Paris, Mallet-Bachelier.—These two pamphlets have been called out by the numerous railroad accidents which have occurred in France during the last year. The authors discuss with much ability the different methods that may be employed to prevent such accidents. M. Couche is chief engineer of a prominent railroad, and therefore well situated to judge of these things; this is true also of M. With.

—— *Stenarithmie ou Abréviation des calculs par* Gossart, 1 vol. in 12 de 120 pages. Paris, Mallet-Bachelier.

—— *Problème de Mathématiques et de Physique par* Duvignau. 1 vol. in 12 de 140 pages. Paris, Mallet-Bachelier.

—— *Arithmétique a l'usage des instituteurs par Finance*, 1 vol. in 12 de 236 pages. Paris, chez Mallet-Bachelier.

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ART. XXXIII.—*On the Tides at Key West, Florida, from observations made in connexion with the United States Coast Survey;* by A. D. BACHE, *Superintendent.*—Communicated by authority of the Treasury Department; revised from the Proceedings of the American Association for the Advancement of Science. (With six Plates.)

HOURLY observations of the tides were made at Fort Taylor, Key West, from the 1st of June, 1851, to the 31st of May, 1852, by Mr. J. W. Goss, of the Coast Survey and assistants. The tides ebb and flow twice in the twenty-four hours, but the diurnal inequality in height is relatively large, amounting at a mean to 0·55 foot, and reaching, in extreme cases, 0·83 foot.* The mean rise and fall of the tide being about 1·4 foot, a knowledge of the laws of the diurnal inequality by which successive high or low waters may differ is very important. The corrected establishment of Key West is 9h. 22m. The curves of Plates 1, 1 bis, 1 tris, 2, 3, 4, and 5, show the normal character of the tides at the maximum and zero of the moon's declination at the syzigies and quadratures, and at a mean of declination and six hours of the moon's age. There being two tides in the lunar day, the observations admit of discussion by the ordinary methods, while the large diurnal inequality in height of high water renders it desirable to pursue the mode which I have applied to the tides at Cat

* Throughout this paper the whole difference of the A. M. and P. M. tides is taken as the diurnal inequality.

Island (Louisiana), and Fort Morgan (Alabama.) The reductions by the ordinary methods thus become the tests of those by the other mode. The former were made under my immediate direction by Lieut. Richard Wainwright, U.S.N., and Mr. M. H. Ober, U. S. Coast Survey, and the latter by Mr. W. W. Gordon, assisted by Messrs. Mitchell, Homans, and others, of the Coast Survey.

The *half-monthly inequality* in time and height as deduced by the usual method is shown in the following Table No. 1, in which the first column contains the moon's age, the second the mean lunitidal interval corresponding, and the fifth the height.

TABLE No. 1.

Half-monthly inequality of tides at Key West from one year's observations.

Moon's age. 1.	Interval. O. 2.	Interval. T. 3.	Interval. O—T. 4.	Height. O. 5.	Height. T. 6.	Height. O—T. 7.
H. M.	H. M.	H. M.	M.	feet.	feet.	feet.
0 30	9 21	9 21	00	6·34	6·34	0·00
1 30	9 05	9 07	02	6·31	6·32	·01
2 30	8 51	8 54	03	6 25	6·26	·01
3 30	8 47	8 46	01	6 17	6·16	·01
4 30	8 50	8 55	05	6·08	6·06	·02
5 30	8 54	8 58	04	6·00	5·97	·03
6 30	9 22	9 25	03	5·94	5·94	·00
7 30	9 52	9 49	03	6·00	5·98	·02
8 30	9 59	10 00	01	6·02	6·08	·06
9 30	9 59	9 58	01	6·12	6·18	·06
10 30	9 53	9 58	05	6·22	6·27	·05
11 30	9 35	9 35	00	6·30	6·33	·03
	9 22					

The mean interval for this table is 9h. 22m., corresponding to the epoch of the moon's age of 24 minutes, showing that the transit E (of Mr. Lubbock's notation) and not F should be used in the reduction for theoretical purposes.

The comparison between the results of observation and those from the formula for the half-monthly inequality is shown in the fourth and seventh columns, the fourth referring to the interval and the seventh to the height. The difference in the mean is inappreciable, and, at a maximum, is but five minutes of interval, and six hundredths of a foot of height.

A graphic comparison is made on Plate 2. The value of the constant (A) of the formula for the interval, $\tan 2\psi = \frac{(A)\sin 2\varphi}{1+(A)\cos 2\varphi}$ is 0·325, and of E in the formula for the height $h = D + E[(A)\cos(2\psi - 2\varphi) + \cos 2\psi]$ is 0·620.

The values of the *diurnal inequality* of high and low water, both in time and height, were obtained by comparing the mean value of the interval and height for the first and second six months, with the individual values; they followed closely the law of change with the moon's declination. The inequality in height of high water at a mean is to that of low water as 79 to 61.

As the observations were only made hourly, and the inequality in the interval of high water is small, the minute changes from day to day could not be expected to show themselves. The inequalities were grouped according to the different declinations of the moon into fourteen periods, and the approximate formula, given by Mr. Lubbock, for the variation from the mean, was applied. The agreement with theory, as shown in the annexed table, is very close; G was taken as 1·15.

TABLE No. 2.

Comparison of the diurnal inequality of high water at Key West, with the formula

$$d\psi = \frac{G \tan \delta'}{1+(A) \cos 2\varphi}.$$

Moon's declination.	Diurnal inequality. Observed.	Computed.	Difference.
° ′	Minutes.	Minutes.	Minutes.
3 55	13	15	—02
7 15	25	29	—04
11 30	48	47	01
15 45	61	64	—03
18 55	74	78	—04
20 55	88	87	01
21 30	100	91	09
21 55	95	92	03
20 15	84	85	—01
17 30	83	72	11
13 55	52	56	—04
9 15	37	38	—01
5 15	25	21	04
2 55	07	11	—04
			—23
			+29
			52

The inequalities of time of high water were also arranged according to the moon's age, but the agreement of the observation with the formula is not as close as in the former case, as must be the case from the small number of observations, and the variation of the inequality following chiefly the law of the declination. The law of change is still evident in the grouping, and the plus and minus quantities balance nearly.

The discussion of the diurnal inequality in height will be resumed in referring to the diurnal wave, after noticing the decomposition of the curve of observation into a semi-diurnal and diurnal curve.

Decomposition of the curves of observation.

As in the discussion presented for the Cat island tides, the curves of observation at Key West were decomposed into two—one representing the semi-diurnal and the other the diurnal tide. The interval (E), which was in the former case assumed to be constant, was here treated as variable. The observed ordinates being referred to the mean of high and low water of the day as

a zero, were tabulated, and the maximum ordinates S and D of the semi-diurnal and diurnal curves of sines found, taking (E) generally at its mean value. From these the ordinates of each curve for the several hours were obtained, and thence the ordinates of the compound curves. These were compared with observation, and E was next made to vary until the value was found, which gave the sum of the differences of computation and observation, without regard to sign, the smallest.

It was next intended, treating this as a first approximation, to take a different zero-point for the semi-diurnal curve, but the labor necessary has prevented this idea from being followed up thus far, and the agreement of the computed and observed curves is quite satisfactory in the cases in which E is not varying too rapidly for safe deductions. The labor and uncertainty of deducing E from the observations in the manner just referred to is very considerable, and, after one full comparison made in this way, the values will be deduced from theory, and applied to the curves.

The approximate compound curve was next projected on a diagram of suitable scale, and the outline cut from the paper so as to apply it to the curve of observation, and thus to find its best position in reference to that curve, and to determine the times of high water. The work referred to in the paragraphs preceding the last is mechanical, but this latter requires much judgment, and has been executed by Mr. W. W. Gordon. Supposing that some discrepancies observed might result from a sort of personal equation in making these comparisons, a second person was engaged to repeat them for verification, and the result showed that the comparison could be depended upon in individual cases to within about five minutes in time in the position of the maximum ordinates.

Semi-diurnal Tides.

The times of high water from the semi-diurnal curves being taken from the diagrams, are subject to an error, which Mr. Gordon estimates as from four to five minutes. This, however, does not appear in the final results, which agree as well with theory as those for the heights, not subject to any such error of estimate.

The lunitidal intervals and heights found were tabulated according to the moon's age, as in the following tables, which contain the result for the first and second six months of the year and for the whole year. The fourth and seventh columns contain, respectively, the differences in the interval and height drawn from the curves, and from the formula for the half-monthly inequality referred to in the previous part of this paper:

TABLE No. 3.—*First six months.*

Moon's age.	Interval. O.	Interval. C.	Interval. Difference. O—C.	Height. O.	Height. C.	Height. Difference. O—C.
H. M.	H. M.	H. M.	M.	feet.	feet.	feet.
0 30	8 53	8 54	1	0·75	0·75	0·00
1 30	8 38	8 42	4	·73	·72	·01
2 30	8 29	8 30	1	·64	·66	·02
3 30	8 24	8 24	0	·60	·58	·02
4 30	8 28	8 27	1	·48	·49	·01
5 30	8 42	8 40	2	·45	·43	·02
6 30	9 06	9 03	3	·42	·42	·00
7 30	9 23	9 23	0	·44	·46	·02
8 30	9 31	9 30	1	·53	·54	·01
9 30	9 28	9 28	0	·61	·62	·01
10 30	9 20	9 19	1	·68	·69	·01
11 30	9 09	9 07	2	·75	·74	·01
	8 57					

TABLE No. 4.—*Second six months.*

Moon's age.	Interval. O.	Interval. C.	Interval. O—C.	Height. O.	Height. C.	Height. O—C.
H. M.	H. M.	H. M.	M.	feet.	feet.	feet.
0 30	8 53	8 52	1	0·78	0·77	0·01
1 30	8 42	8 39	3	·72	·75	·03
2 30	8 27	8 28	1	·69	·68	·01
3 30	8 22	8 21	1	·59	·59	·00
4 30	8 22	8 23	1	·49	·49	·00
5 30	8 34	8 38	4	·46	·42	·04
6 30	9 04	9 03	1	·40	·40	·00
7 30	9 22	9 23	1	·44	·45	·01
8 30	9 31	9 31	0	·53	·54	·01
9 30	9 32	9 28	4	·62	·64	·02
10 30	9 20	9 19	1	·69	·72	·03
11 30	9 04	9 06	2	·71	·77	·06
	8 56					

TABLE No. 5.—*The whole year.*

Moon's age.	Interval. O.	Interval. C.	Interval. O—C.	Height. O.	Height. C.	Height. O—C.
H. M.	H. M.	H. M.	M.	feet.	feet.	feet.
0 30	8 53	8 53	0	0·76	0·76	0·00
1 30	8 40	8 40	0	·73	·74	·01
2 30	8 28	8 29	1	·67	·67	·00
3 30	8 22	8 22	0	·59	·59	·00
4 30	8 25	8 25	0	·49	·50	·01
5 30	8 38	8 39	1	·45	·43	·02
6 30	9 05	9 03	2	·41	·41	·00
7 30	9 22	9 23	1	·45	·46	·01
8 30	9 31	9 31	0	·53	·54	·01
9 30	9 30	9 28	2	·62	·63	·01
10 30	9 20	9 19	1	·69	·71	·02
11 30	9 06	9 07	1	·73	·75	·02
	8 57					

Curves showing the result of these comparisons are given in Plate 2. The greatest difference for the whole year between the two sets of results is but one minute of time for the interval, and ·02 foot in height.

The results are in apparent time, the substitution of which for mean time was, however, appreciable in but a slight degree.

There are several small corrections suggested by the hypothesis which has been adopted, but the small value of the residuals renders the following of them up unnecessary. To the last computations we have reached no greater accuracy than is presented by these residuals, and could not safely base any conclusions on less quantities.

The ordinates used in the heights are the maximum ordinates of the component curves, and not those of high water of the compound curve; but it is easily shown that when the value of (E,) when most nearly constant, is, as at Key West, between about nine and nine a half hours, this difference is inappreciable.

The solar day having been used in this decomposition instead of the lunar, the curves are at a mean twenty-five minutes behind their true place, and the mean lunitidal interval differs twenty-five minutes from the truth; adding this quantity, it agrees, as it should do, with the former determination.

For the reason just assigned, these numbers would require correction before using them to determine the constants. This, when made, gives the result as before stated.

Diurnal tides.

The maximum ordinates found for the diurnal tides from the decomposition of the curves of observation were grouped according to the declinations of the moon, by magnitude without regard to sign, as shown in the first and second column of Table No. 5.

The maximum ordinates may, in this case, be reduced to high water ordinates by a very simple process, and thus a comparison be established between this mode of reduction and the ordinary one. For (E) = 9h. 30m., the high water ordinate is 0·79, the maximum, provided, as in the case at Key West, the time of high water may be taken as that of the semi-diurnal wave.

The following table shows the moon's declination, the corresponding mean maximum ordinate, twice the high water ordinate deduced from this, (which is the diurnal inequality from our mode of reduction,) the diurnal inequality as usually obtained, and the difference.

TABLE No. 6.

Sine twice moon's declination.	Maximum ordinate.	Twice high water ordinate, (C.)	Diurnal inequality, (C').	Difference, (C'—C).
	feet.	feet.	feet.	feet.
11	0·12	0·20	0·19	—·01
21	·17	·27	·28	·01
35	·28	·44	·44	·00
48	·38	·59	·60	·01
59	·46	·71	·70	—·01
66	·51	·80	·79	—·01
69	·54	·84	·83	—·01
Mean		·55	·55	

The results are represented in Plate No. 3.

The statement made above in relation to the high water ordinates is not true for those of low water, as the consideration of the formula $y = C. \cos 2t + D \cos (t - E)$ will show, making $E > 9$ hours. The reverse is the case if $E < 9$ hours, the statement applying then to the inequality of low water and not of high. At Key West, while the high water inequality in height is thus readily found from the maximum ordinate, the low water presents a less accordant result; while at Cedar Keys just the reverse occurs, as should be the case.

It is plain, also, that changes in the coefficients C, D, and in E, will cause the inequalities in times and heights to vary, as well as those of high and low water, losing all correspondence with each other, as is also well shown in the annexed diagram. Mr. Gordon suggests that in the value of (E) will be found the full explanation of the peculiarities of the Petropaulofsk tides described by the Rev. Mr. Whewell.

In diagram No. 1, Plate No. 5, E is assumed 9 hours and $S = D$, and the inequality of high and low water in interval and height correspond to each other. The same is the case for $E = 15$ hours. In No. 2, E is 12 hours, and $S = D$. The inequality in interval of high water is 0h., of low water 4h., when that in height of high water is 2 feet, and of low water 0 feet. For E, 18 hours and $S = D$, these inequalities would be reversed, that of interval of high water being 4h., and of low water 0h., while for height the inequality of high water is 0 feet, and of low water 2 feet.

Using the high water ordinates, determined as before stated, instead of the diurnal inequality in height, from which it has been shown not to differ sensibly, the numbers were compared with those of Mr. Lubbock's formula:

$$dh = B\left[(A)\sin 2\delta \cos(\psi - \varphi) + \sin 2\delta \cos \psi\right];$$

Neglecting the variations of $\cos(\psi - \varphi)$, $\cos \psi$, the coefficients B and (A) B were found by least squares for the separate six months and for the year, agreeing sensibly in the partial and total determinations. From two years' results, $B = 0{\cdot}56$ and $(A)\,B = 0{\cdot}16$. The value of (A) thus obtained is, as it should be, the same as deduced from the half-monthly inequality.

The sum of the squares of the difference of the numbers from the formula, and from the computed high water ordinates, is for the year but 0·0087 foot; corrected for the moon's parallax, but 0·0078 foot.

The individual results are given in the annexed table, in which the first column contains the moon's age, the second the difference between the computed high water ordinates and the corresponding quantities from the formula for the variations of the

diurnal inequality in height, corrected for parallax, and the third the same, as uncorrected for parallax.

TABLE No. 7.

Moon's age.	Diurnal inequality height; observation—theory.		Moon's age.	Diurnal inequality height; observation—theory.	
	Corrected.	Uncorrected.		Corrected.	Uncorrected.
H. M.			H. M.		
0 30	005	010	6 30	−080	−085
1 30	005	005	7 30	−090	−090
2 30	−035	−030	8 30	−065	−065
3 30	−035	−035	9 30	−020	−020
4 30	−075	−080	10 30	−030	−030
5 30	−070	−070	11 30	+005	010

The residuals are very small, but follow the law of the half-monthly inequality, as was found, also, from the corresponding results of the Cat Island observations.

The discussion of the value of E, which is in progress, I hope to present at a future meeting of the Association.

Changes of mean level.

The mean level of the water at Key West was seen from the observations to undergo remarkable changes from one period of the year to another. A comparison of the reductions for the first and second six months shows that the high water of neap tides of the first period rises actually to a higher level than the high water of spring tides of the second. The mean level of the high water for the first six months exceeds that for the second by 0·48 foot. The form of the half-monthly inequality is perfectly regular in each six months. The guage had remained undisturbed; and in seeking for the explanation, it was observed that the mean level of the water varied very materially in the two periods, there being a change which appeared to go through its variations in the course of the year.

The annexed table shows the heights of high water at the several ages of the moon in the first and second six months, referred to the same zero.

TABLE No. 8.

Moon's age.	Height of high water.	
	First six months.	Second six months
H. M.	feet.	feet.
0 30	6·63	6·05
1 30	·59	·04
2 30	·48	·02
3 30	·38	5·97
4 30	·31	·86
5 30	·26	·75
6 30	·17	·72
7 30	·28	·72
8 30	·26	·79
9 30	·34	·90
10 30	·43	6·01
11 30	·54	·06
	6·39	5·91

I hardly supposed that the numbers representing these changes of level would furnish evidence of the two interesting tides of long period pointed out by Mr. Airy, (Tides and Waves, Encyc. Metrop., p. 355;) they do so, however, and in the case of the moon's action, where the number of averages which can be brought to bear upon a single result is considerable, and the observations run through various parts of the year, the results bear carrying to numerical comparison with the formula. These tides, as far as I am aware, have not been developed from observation, though certain general analogies pointed to their existence. Dividing the numbers showing the daily level of the water into groups of nearly fourteen days, each corresponding to the moon's declination from the maximum to the maximum again, and taking the mean of each set corresponding to the same declination, we obtain a series which is the average of twenty-six numbers in which the irregularities of the depressing and elevating action of the winds will be eliminated, and in which the sun's action will be nearly the same. This series presents a tolerable regularity increasing to a maximum at zero of declination, as shown in Plate No. 6, curve No. 1.

Taking the mean level of the water for each fourteen days, and dividing the results into two groups corresponding to the same declination of the sun, north and south, we have a series of numbers which, though less regular than the others, also rise towards the zero of declination, as shown in Plate 6, curve No. 2.

The results of the first series of computations bear very well a comparison with the formula given by Mr. Airy:

$$(1{\cdot}34 \times \sin^2 \mu + 0{\cdot}61 \times \sin^2 \sigma)\,(\cos 2\lambda + c,)$$

in which μ and σ are the declinations of the moon and sun respectively, and λ is the latitude of the place, requiring $C = O$. Those of the second present greater discrepancies and require $C = \frac{1}{3}$, contradicting the former. Though the weight of authority is that in favor of the criterion of the wave theory $C = O$, the result is inconclusive. In either case the whole number involved is less than the tenth of a foot, the theoretical lunar tide being 0·095, and the measured 0·098, while the theoretical solar tide for $C = O$ is 0·046, and the measured is 0·077.

It may be necessary to remark that for places of low latitudes the increase of numbers in the formula corresponds to a fall of the tides.

The regularity of the winds of this region, the trade winds, taken in connexion with the form of the harbor, point also to their action as a source of explanation of this change of level. The meteorological tables kept while the tidal observations were made, furnish means for a complete discussion, which is in progress. I may remark, now, that winds tending to elevate the water in the harbor prevail for six months, from March to August inclusive, and those tending to depress it for the other six months, from

September to February inclusive. The subject is one in which it is difficult to come to numerical results, because the variations in the force of the wind and the duration both enter into the effect, and distant action sometimes causes local effects. The whole rise and fall is nearly three-quarters of a foot.

The mean level of the water deduced from the mean of high and low water in each month is shown in the annexed table.

TABLE No. 9.

Month.	Height in feet	Month.	Height in feet
June, - - - -	5·60	December, - - - -	5·31
July, - - - -	·78	January, - - - -	·11
August, - - - -	·63	February, - - - -	·15
September, - - -	·93	March, - - - -	·26
October, - - - -	·90	April, - - - - -	·26
November, - - -	·73	May, - - - - -	·32
	5·76		5·24

Art. XXXIV.—*On the Geographical Distribution of Crustacea;* by James D. Dana.*

In volume xvi, of this Journal, the author presented a chart of oceanic isothermal (or rather isocrymal) lines, for the illustration of marine zoological geography, prepared more especially with reference to the geographical distribution of Crustacea, and taken from his Report on the Crustacea of the Exploring Expedition around the world under Capt. Wilkes. The following is a brief abstract of the remainder of the Chapter on the Distribution of Crustacea; the Tables which occupy near 30 pages are omitted besides other details.

The lines on the chart, it may be here repeated, are lines of equal winter (or coldest month) temperature for the water, the "cold" lines being adopted because the distribution of species away from the Tropics is limited by cold temperature. The temperatures corresponding to the lines are 74°, 68° (limiting temperature of coral reefs), 62°, 56°, 50°, 44°, 35° F. Between the lines of 68° F. north and south of the equator lies the *Torrid Zone* of oceanic water temperature; from the line of 68° to that of 35°, the *Temperate Zone;* beyond the line of 35°, the *Frigid Zone.* These Zones are divided by the lines into Regions or Sub-zones as follows.

I. TORRID ZONE.	1. Torrid Region or Sub-zone,		74° and above.
	2. Subtorrid	"	68° to 74°
II. TEMPERATE ZONE,	1. Warm Temperate	"	62° to 68°
	2. Temperate	"	56° to 62°
	3. Subtemperate	"	50° to 56°
	4. Cold Temperate	"	44° to 50°
	5. Subfrigid	"	35° to 44°
III. FRIGID ZONE.			

* From the Author's Report on Crustacea, (2 vols. 4to, 680 and 1620 pages).

The reader is referred to the former paper and map for other details, where the Zoological Provinces in these zones are laid down, and explained.

The Tables are in two series. The first contains for each genus of Crustacea the number of species *according to present knowledge* in each temperature Region or Sub-zone.* The second, the number for each genus in each *Geographical Province.*

We proceed with a summary of the results presented in the first series of the Tables.

I. BRACHYURA.

	a. Torrid.	*b.* Sub-torrid.	Total of Torrid zone.	*c.* Warm Temperate.	*d.* Temp'ate.	*e.* Sub-temperate.	*f.* Cold Temperate.	*g.* Sub-frigid.	Total of temperate zone.	*h.* Frigid.
Maioidea, - - - -	82	57	122	35	27	21	16	14	92	3 (2)
Cancroidea - - - -	157	112	229	22	25	23	25	8	69	3 (3)
Grapsoidea, - - - -	72	88	131	21	14	27	10	9	63	
Leucosoidea, - - -	35	33	48	11	8	5	3	2	24	
Corystoidea, - - -	2	8	5	2	4	2	6	6	16	1?
	348	298	535	91	78	78	60	39	264	

This table contains the number of species of the orders of Brachyura, according to present knowledge, in each Region and Zone.

The following general facts or conclusions may be deduced from the Tables of the Brachyura.

I. The line of division, separating the Torrid and Temperate zones of ocean temperature, following the isocryme of 68° or the outer limit of coral reef seas, marks a grand boundary in organic life, well exemplified in Crustacean species. Out of the five hundred and thirty-five species of the Torrid and Subtorrid Regions (the Torrid zone,) there are over one hundred now known to be common to the two. But of the two hundred and sixty-four in the Temperate Regions, only thirty-four occur in the Torrid zone. A large number of genera, containing more than a single known species, are confined wholly to the Torrid zone: such are Micippa (5 species), Menæthius (9), Huenia (4), Parthenope (3), Atergatis (17), Carpilius (13), all the Chlorodinæ, including forty-nine species, nearly all the Eriphinæ, including eighteen species, Charybdis (15). At the same time, the species of the Torrid and Subtorrid Regions are in many cases equally numerous. Of species of Charybdis, eleven species occur in each of these Regions; of the Carpilii, eleven are reported from the Subtorrid and but five from the Torrid; of the Menæthii, five are found in the Torrid Region, and six in the Subtorrid, only two being common to both. These proportions may be much varied by future in-

* Since the ocean's waters decrease in temperature as we descend in depth, there will be some error in the tables from the cold water species thus passing into regions nearer the equator. But this error will diminish the number of species regarded as peculiar to the colder regions, and if eliminated, the following conclusions would be still more strongly sustained.

vestigations. Still it cannot fail to be evident from a survey of the tables, that the line between the Torrid and Temperate zones is a natural zoological limit.

II. The Torrid species of Brachyura (Torrid and Subtorrid Regions) greatly preponderate over those of the Temperate zone, the proportion being above two to one. This fact is the subject of remarks by Edwards. but with different conclusions from those which we would deduce.

III. The Frigid zone, as far as known, includes *one* species peculiar to it, the *Chionœcetes opilio.* And *Stenorhynchus phalangium*, *Hyas araneus*, *Portunus pusillus*, *Carcinus mænas*, and *Cancer pagurus*, are all that are known to extend into it from the Temperate zone. Perhaps the *Cancer chirogonus* from Kamtschatka (*Telmessus chirogonus* of White) should be added. This may be in part evidence of the little exploration hitherto made in the Frigid Seas. Yet, after the investigations of Beechey, Fabricius, Kröyer, Rathke, and others, we may be assured that the number of species is exceedingly small.

IV. Within the Temperate zone, the species are most numerous in the Warm Temperate, Temperate, and Subtemperate Regions; beyond this, the number diminishes, being a *quarter less* in the Cold Temperate than in the Subtemperate, and *half less* in the Subfrigid. Moreover, in the last-mentioned region, seventeen out of the thirty-nine species, or nearly one-half, occur in warmer temperate latitudes, only twenty species being confined to the Region.

V. In the Torrid zone, the species of the torrid region, amounting to three hundred and forty-eight, exceed in number those of the Subtorrid by only forty-five, although the Subtorrid region is not one-third as great, both as to surface and extent of coast line.

VI. Passing now from these general considerations respecting the Brachyura as a class to the several orders, we may look at their ratios among these orders and their subdivisions, for the several regions, in order to discover what is the relation of the species to temperature, and whether the cold or warm-water species are the higher or lower in grade, or whether the torrid or the temperate zone can claim species of the highest perfection or magnitude among the Brachyura.

The following table gives the ratio which the number of species of the several orders in the Temperate and Frigid zones, bears to that of the Torrid zone.

1. Maioidea,	1 : 1·3
2. Cancroidea,	1 : 3·3
3. Grapsoidea,	1 : 2·1
4. Leucosoidea,	1 : 2·0
5. Corystoidea,	1 : 0·3

It hence appears that the Maioidea and Corystoidea are proportionally much more abundant in the colder seas than the Cancroidea, Grapsoidea, or Leucosoidea.

If we examine into the subdivisions of the Maioidea and Cancroidea, we shall find the difference between the two groups in distribution more strikingly brought out. We shall find, moreover, that both groups may be divided into a warm-water and cold-water section, as below.

I. MAIOIDEA.

1. COLD WATER OR TEMPERATE ZONE SECTION.

	Torrid species.	Temperate species.
1. Inachidæ,	1	10
2. Maiidæ, subfamilies, *Libininæ*, *Maiinæ*, *Pisinæ*, *Othoninæ*,	15	35
3. Eurypodidæ,	0	7
4. Leptopodidæ,	1	8
	17	60

2. WARM WATER OR TORRID ZONE SECTION.

	Torrid.	Temperate.
1. Maiidæ, subfamilies *Micippinæ*, *Chorininæ*, *Pyrinæ*,	16	3
2. Mithracidæ,	11	6
3. Tychidæ,	4	0
4. Periceridæ,	43	14
5. Parthenopinea,	28	8
6. Oncininea,	2	0
	104	31

II. CANCROIDEA.

1. TEMPERATE ZONE SECTION.

	Torrid.	Temperate.
Cancridæ,	0	11
Platyonychidæ,	2	7
Portunidæ, subfamily *Portuninæ*,	0	15
Cyclinea,	0	1
	2	34

2. TORRID ZONE SECTION.

	Torrid.	Temperate.
Xanthidæ,	129	16
Eriphidæ,	44	12
Portunidæ, excluding the *Portuninæ*,	52	7
Podophthalmidæ,	2	0
	227	35

We have here two singular facts brought out.

First, that the cold-water section of the Cancroidea embraces those species that approach most nearly to the Corystoidea, and which we have elsewhere shown to be the *lowest* in grade of the Cancrinea. All have the lax character of the outer maxillipeds, which is a mark of degradation in the Corystoids; and the Cyclinea are still nearer that group. Many of the species moreover have the hind legs a swimming pair, another mark of degradation. The Corystoidea, as before shown, are two-thirds cold-water species.

Second, that the cold-water section of the Maioidea contains the species that are *highest* in grade, and largest in size. It is headed by the Macrocheira of Northern Japan, the king of all crabs, whose body is seventeen inches in length and a foot broad;

with extended legs, it sometimes covers a breadth of eleven feet, and the anterior legs or arms are *four feet* long!* The species of the other genera are mostly among the larger of the Maioids, and have no mark of inferiority. Such are the species of *Maia*, *Pisa*, *Libinia*, *Eurypodius*, etc.

But among the species of the warmer section, we find the Oncininea and Parthenopinea, both manifestly inferior in grade, the former approaching even the Anomoura, and the latter forming the passage of the Maioids to the Cancroids, as has been explained. We observe also the Pericerid æ and the Tychidæ, all very small species, excepting a few Pericerae: the Menæthii, Tiariniæ, and Acanthonyces, are examples of the group. In addition, there are the Mithracidæ, which although attaining a large size show their inferiority in their shorter epistome, shorter body, which is sometimes even transverse, and their spoon-shaped fingers. In the last character, the Chlorodinæ among the Cancroids, similarly show their inferiority to the Xanthidæ. That this kind of finger is such a mark of inferiority is apparent from its diminishing in many species as the adult size of the animal is attained, the tendency being towards producing the acuminated finger found in the highest grades.

We are hence sustained in the conclusion that the Maioids of the Temperate zone are generally those that are highest in grade. It also shows the congeniality of cold waters to the Maioids, that the only Brachyuran peculiar to the Frigid zone is of this group. We refer to the Chionœcetes opilio.

VII. The Brachyura, therefore, although most numerous in the Torrid zone, do not reach in this zone their highest perfection. On the contrary, the Temperate zone or colder waters are the habitat of the highest species. Hence, as the Maioidea stand first among all Crustacea, the highest development of the class Crustacea takes place, not in the Torrid zone, the most profuse in life, but beyond the tropics and coral-reef seas, in the middle Temperate Regions.†

VIII. The prevalence also of the inferior Corystoids in the colder waters does not invalidate this conclusion, as the fact respecting the Maioids is wholly an independent one; for these last, by attaining their highest perfection in these coldest waters, determine the principle as regards themselves, the highest grade of Crustacea. Lower grades occur also in the colder waters, and the laws governing their distribution demand separate study and consideration.

IX. Passing a step below the Maioids, we come to the Cancroids, and these, with the exception of the lower Corystoid species, and only *one-eighth* of the rest, are Torrid zone species.

* De Haan's Fauna Japon., Crust. p. 101.

† On the coasts of Britain, the Cancroids (excluding the swimming species,) are only half as numerous as the Maioids.

X. If the Torrid zone is the proper region for the full development of the Cancroid type, and its heat is needed for this end, it is natural that species of Cancroids like the *Portuninæ*, *Platyonychidæ*, and *Cancridæ*, found in the less genial waters of the Temperate zone, should bear some mark of inferiority;—and it is a fact that they have such marks in their structure. This inferiority is not seen in their smaller size,—for a larger size under certain conditions, may equally evince a lower grade,—but in the inferior concentration of the life-system, exhibited either in the lax outer maxillipeds, the elongation of the antennæ and abdomen, or in the smaller size or swimming character of the posterior legs.

For a like reason also, the species of Corystoidea, a grade still lower, naturally occur in the cold and ungenial region they frequent.

We hence perceive, that the degradation among the Maioids takes place when the species become warm-water species, and the degradation among the Cancroids, in the reverse manner, when the species become cold-water species; for the reason that the colder waters are the proper habitat for the Maioid type, and the warmer for the Cancroid type.

XI. In the tables of the Maioidea and Cancroidea of the Temperate and Torrid zones, page 317, the species are included by families and subfamiles, and consequently the peculiarities of the genera are not shown. In the families or subfamilies referred to the cold-water section, there is only one warm-water genus, viz., *Doclea*, of the subfamily *Libininæ;* it contains four Torrid and one Temperate zone species.

Among those referred to the warm-water section, there are the following cold-water genera:—

			Species in Torrid zone.	Species in Temperate zone.
Parthenopinea,	genus	Eurynome,	0	2
"	"	Eurynolambrus,	0	1
Xanthidæ,	"	Paraxanthus,	0	2
Ozinæ,	"	Ozius,	2	3

The species of Cancrinea of the Torrid zone section, which reach farthest into the Temperate zone, are those of the following genera:—*Xantho*, which has eight Temperate zone species out of twenty-eight in all; *Panopeus*, which in the same way has four out of ten; *Pilumnus*, which has seven out of twenty-two; and *Lupa*, which has four out of ten. The cold Temperate Region is the highest for each of these genera, excepting Lupa and Pilumnus, a species of each of these latter genera extending just within the limits of the Subfrigid Region, on the coast of Massachusetts.

XII. The Grapsoidea, if divided between the Torrid zone and Temperate zone, according to families or subfamilies, will fall within the Torrid zone, excepting a single family of the Pinno-

theridæ, which contains eight species in the Torrid zone and fifteen in the Temperate. Considering the genera, however, we find that several among the Grapsidæ may be called cold-water genera, or are about equally divided between the Torrid and Temperate zones. They are as follows:

	Torrid species.	Temp'te species.
Pseudograpsus,	1	2
Heterograpsus,	0	1
Brachynotus,	0	1
Planes,	2	2
Hemigrapsus,	4	5
Cyrtograpsus,	0	1
Chasmagnathus,	2	2

Five out of twelve species of Grapsus also reach into the colder seas. Further particulars will be gathered from the tables.

XIII. The Leucosoids include as cold-water genera the following:

	Torrid.	Temperate.
Genus Ebalia,	0	8
" Ilia,	0	1

The other genera are mainly confined to the Torrid zone; out of the species they contain, sixty-seven in all, forty-eight are of this zone. *Hepatus*, however, contains as many cold-water as warm-water species, and the same is true of *Dorippe*, although but one of the species of the latter is exclusively Temperate.

XIV. The tropics afford not only a larger number of species of Brachyura than the temperate zone, but also a much greater proportion of individuals of the several species. Crustacean life, of this tribe, is far the most prolific in the warm waters of the globe. Species are very abundant about coral islands, far exceeding what may be found in other regions.

XV. The actual mass of Brachyura appears also to be the largest in the tropics, although there are genera, as Macrocheira and Cancer, which have their largest species in the colder waters, and which exceed in size any other Brachyura. The genera Atergatis, Carpilius, Xantho, Menippe, Zozymus, Eriphia, Thalamita, Charybdis, Calappa, besides others of the Torrid zone, contain many large species, which are of very common occurrence; while the cold-water genera of Maioids appear to be much less prolific in species, and the other genera, though abounding in individuals, as Cancer and Lupa, are still but few in number. Any very exact comparison, however, of the two zones in this particular cannot be made without more data than have yet been collected.

II. ANOUMOURA.

XVI. The Anoumoura are nearly equally divided between the torrid and temperate zones, there being hardly *one-tenth* more torrid than cold-water species. Only fifteen species out of two

hundred and twenty-five are common to the torrid and temperate zones.

Yet it is seen from the table, that if we except the Galatheidea, Lithodea, and part of the Paguridea, the species hardly extend beyond the warmer half of the temperate zone. There are but six known frigid species, and these are of the two last-mentioned groups.

XVII. The torrid zone and temperate zone sections of the Anomoura, are as follows; the frigid zone species being here added to the temperate.

1. TEMPERATE ZONE SECTION.

	Torrid zone.	Temperate zone.
Dromidæ, G. *Latreillia*,	0	3
Homola,	0	2
Bellidea,	0	2
Raninidea, G. *Notopus*,	0	1
Lyreidus,	0	1
Hippidea, G. *Albunhippa*,	0	2
Lithodea,	0	10
Porcellanidea,	27	20
Paguridæ, G. *Paguristes*,	3	6
Bernhardus,	3	29 { 1 torrid and 4 frigid.
Ægleidea,	0	2
Galatheidea, G. *Munida*,	0	2
Grimothea,	0	1
Galathea,	5	4

2. TORRID ZONE SECTION.

	Torrid zone.	Temperate zone.
Dromidæ, G. *Dynomene*,	1	0
Dromia,	8	2 (1 torrid).
Cymopolidæ, G. *Cymopolia*,	1	1
Caphyra,	2	0
Raninidea, G. *Raninoides*,	1	0
Ranina,	1	0
Ranilia,	1	0
Cosmonotus,	1	0
Hippidea, G. *Albunæa*,	3	3 (2 torrid).
Remipes,	5	1 (1 torrid).
Hippa,	2	2 (1 torrid).
Paguridæ, G. *Diogenes*,	5	2 (2 torrid).
Pagurus,	14	7 (1 torrid).
Calcinus,	6	0
Aniculus,	1	0
Clibanarius,	19	4
Cancellus,	1?	0?
Cenobitidæ,	10	1

The Dromidea and Paguridea have *one-third* to *one-fourth* more torrid than cold-water species.

The Raninidea and Hippidea are mainly tropical. The two extra-tropical species of Raninidea occur only in the warmer of the temperate regions, and the species of Hippidea in the temperate zone (eight out of the whole number fourteen) have among them four that occur also in the tropics.

The Lithodea belong to the coldest temperate regions, abounding especially in the subfrigid region. The Galatheidea are mainly of the temperate zone; there are five known torrid species, and seven temperate, the latter pertaining to the colder seas.

The genus *Porcellana* has but two-thirds as many species in the temperate as in the torrid zone. Yet the subtemperate region contains but one less than the subtorrid, and some of the largest species of the genus occur here; while, on the contrary, the torrid-zone species are quite small. Although, therefore, Porcellana may rank as a torrid zone genus, if we consider the relative number of species in the two zones, it is more properly a temperate zone genus.

The Paguridea range through both the tropics and temperate zone, even passing into the frigid zone. *Bernhardus* is mainly a cold-water genus, while Pagurus, Calcinus, and Clibanarius are mostly torrid genera. *Pagurus* has seven out of twenty-one species in the temperate zone. But it is in the torrid zone where the species of the largest size occur; the extra-torrid species belong almost exclusively to the Mediterranean. The species are exceedingly prolific in the tropics, far exceeding what occurs as regards any Paguridea in the temperate zone.

XVIII. It was found in the Brachyura, that the highest species among the Maioids, and the highest of Crustacea occur in the *extra-tropical* regions; and that as we descend to the Cancroids, the species become mainly *tropical;* moreover, as we descend among the Cancroids (the type of which is tropical), there is in general a return to the less genial colder waters, as exemplified in the true Cancers or Cancridæ and the Corystoidea, these last being mainly cold-water species. By these steps we find the more degraded forms among the Brachyura occurring in both the colder and warmer waters. We cannot therefore expect that the Anomoura, which are properly Brachyura of a still lower grade, should be arranged according to rank in one zone in preference to the other. And it is a fact that the genera of higher species occur about equally in the two zones. Latreillia, but a single step below the Inachidæ, is found in the warmer temperate regions; and Dromia, a little lower, has three-fourths of its species in the tropics. Homola, again, has been found only in the temperate zone.

Among the Paguridea, the Bernhardi or cold-water species are probably the superior in rank; and the Lithodea, which are a grade higher still, are from the neighborhood of the frigid zone.

The Hippidea, which we have considered as in the Corystoid series, but below the Corystoidea, are mostly from *warmer* waters.

The most bulky forms among the Anomoura are found in the genera Lithodes, Ranina, and Dromia. The common *Ranina dentata* has a length of five inches in the Japan Seas, while in

the warm East Indies (at the Moluccas), as De Haan states, four inches is the greatest length.

III. MACROURA.

XIX. The Macroura, according to the table, [see Report,] are nearly equally divided between the torrid and extra-torrid zones, the former including one hundred and forty-seven species, and the latter one hundred and fifty-three species.

In the table we have not included the fresh-water Astacidæ, as we are treating only of marine species. Yet in a comparison of numbers between the zones, these should be brought in. They are about thirty-six in number, and all, excepting perhaps one, belong to the temperate zone as regards the temperature of the waters they frequent. With this addition, the numbers become 147 for the torrid zone, and 189 for the extra-torrid. Sixteen of the cold-water species are common to both the torrid and temperate zones, and twenty-nine occur in the *frigid* zone, twenty-seven being peculiar to this zone. This is strikingly in contrast with the Brachyura, of which two-thirds are torrid species, and only five or six are known to extend into the cold zone, of which but *one* as far is known, is confined to it.

XX. The Thalassinidea are mainly extra-torrid species.

The Astacidea are divided between the warm and cold seas; the Palinuridæ and Scyllaridæ being mostly of the former, and the Astacidæ almost exclusively of the latter.

The Caridea spread largely over both zones; but extensive groups are extra-torrid, and some genera contain many frigid species.

The Penæidea are mainly of the torrid zone.

The exact ratios may be gathered from the tables.

XXI. The geographical relations of the subordinate groups are shown in the following table.

1. TEMPERATE AND FRIGID ZONE SECTION.

	Species in the Torrid zone.	Species in the Temperate and Frigid zones.
Thalassinidea,	6	17
Astacidea,	24	50
Astacidæ,	1	46
Scyllaridæ, G. *Arctus*,	0	1
Palinuridæ, G. *Palinurus*,	2	3
Caridea.		
Crangonidæ,	2	25
Atyidæ, *G. Ephyra*,	0	2
Palæmonidæ.		
Alpheinæ, G. *Betæus*,	1	4
Alope,	0	1
Athanas,	0	1
Hippolyte,	8	37 (19 frigid).
Pandalinæ, G. *Pandalus*,	0	4 (2 frigid).
Palæmoninæ, G. *Cryphiops*,	0	1
Pasiphæidæ, G. *Pasiphæa*,	0	8 (1 frigid).
Penæidea, G. *Eucopia*,	0	1 (frigid).

2. TORRID ZONE SECTION.

	Species in the Torrid zone.	Species in the Temperate and Frigid zones.
Astacidea.		
Scyllaridæ, except *Arctus*,	10	2
Palinuridæ, G. *Panulirus*,	12	1
Caridea.		
Atyinæ,	8	1
Palæmonidæ.		
Alpheinæ, G. *Alpheus*,	31	7
Palæmoninæ, G. *Pontonia*,	4	2
Œdipus,	3	0
Harpilius,	1	0
Anchistia,	3	0
Palæmonella,	2	0
Palæmon,	32	19 (1 frigid).
Hymenocera,	1	0
Oplophorinæ,	8	1
Penæidea,	19	12

XXII. Considering the Scyllaridæ and Palinuridæ as the Macroura highest in grade, this division of the Podophthalmia appears at first to have its superior developments in the tropics. But it may still be questioned whether this is altogether true. The Palinuridæ include two genera, one *Palinurus*, mainly a cold-water genus, the other *Panulirus*, a warm-water or Torrid zone genus; and is the Torrid zone genus the superior in rank, as should be the case, if the tropics are the most congenial to the highest Macroural developments? *Palinurus* has the outer antennæ nearly in contact at base, and the flagella of the inner antennæ are very short; *Panulirus*, the warm-water genus, has the outer antennæ remote at base, and the flagella of the inner antennæ very long. The genera are thus characterized by marks analogous to those that distinguish the higher and lower species among the Brachyura, or that exhibit the superiority of the Brachyura as a class over the Macroura; and if such evidence is here to be regarded, the cold-water genus, Palinurus, is the higher in rank. Moreover, the aspect of the Palinuri, the harder shell and more compact body, strike the eye at once as indicating their higher character. In size, they are not at all inferior; they even exceed the Panuliri in bulk if not in length. Among the Palinuri, one species is afforded by the warm seas of the West Indies; but it is not half the size lineally, of the *Lalandii* of the Cape of Good Hope, or the *vulgaris* of the Mediterranean, both gigantic species, sometimes a foot and a half in length independent of the antennæ.

The Astacidæ, the remaining family in the tribe Astacidea, is confined almost wholly to the colder waters, and the species are numerous.

Among the Caridea, the Crangonidæ certainly have the precedence. The fact that the first pair of legs have perfect hands, while the other legs are vergiform, shows a relation to the Brach-

yura, which is evidence of superiority. These Cr the highest of the Caridea, are almost exclusiv species.

In the family Palæmonidæ, some genera have tl furnished with stout hands, while in others the stout chelate pair. The former, for the reason while speaking of the Crangonidæ, and elsewh plained, are superior in rank. It is among these superior grade, the Alpheinæ, that we find the boreal species. The genus Hippolyte alone contai cold-water species, nineteen of which are of th and there are only eight torrid species.

On the contrary, among the Palæmoninæ, the there are forty-six torrid to twenty-two extra-torri only one of the latter is boreal. Species of Alphe in the tropics about coral-reefs; but the largest genus, two or three inches long, occur beyond the

The Penæidea, the lowest of the tribes of Macro tropical. Yet, the very lowest species (like the low occur partly in the colder waters, or even in the F

XXIII. Comparing the torrid and temperate croura, we are led to conclude, that the latter are numerous in individuals, and the most bulky in r ing the Panuliri, Scyllari, and some Palæmons, t cies are small, and moreover, they are not partic about coral-reefs. The species of the torrid ge Œdipus, Harpilius, Anchistia, Palæmonella, Hy Atya, are all quite small, the greater part not exc and a quarter in length; moreover, the tropical small species, as stated above. The Penæidea a species. Contrast these particulars with the facts of the Temperate zone. Palinurus, Astacus, Ne phrops, Homarus, Arctus, Crangon and the relat polyte, Pandalus, Cryphiops, contain species most and the adult Homari and Palinuri are not exce by any other Macroura.

The Thalassinidea, which belong almost exc temperate regions are smallest in the warmer pa perate zone, and larger in the middle and colder Sound species (subfrigid region) of Callianassa (least four and a half inches long, the *C. uncinat* inches, and the *Thalassina scorpionides* of Ch The facts respecting this subtribe, added to th above, strengthen much the conclusion, that the era have the largest species; for all the species ar and a half in length.

IV. ANOMOBRANCHIATA.

XXIV. The Mysidea, to which the Penæidea are related, are, to a considerable extent, cold-water species, although many are found also in the tropics. There are among them twenty torrid species and seventeen extra-torrid species.

In the Squilloidea we have an example of an inferior grade in a large lax body, with a small head and long abdomen; and they remind us of overgrown larval forms, or species vegetatively enlarged beyond the normal or most efficient size. In this particular they have some analogies with the earlier forms of life. They are found mostly within the tropics. Twenty-four of the Squillidæ are torrid zone species, and only seven pertain exclusively to the Temperate zone. Of the Erichthidæ, twenty-one out of twenty-two species are reported from the Torrid zone. The Amphionidea, a related group, include seventeen Torrid zone species and two of the Temperate zone.

(*To be continued.*)

Art. XXXV.—*Notes on Map Projections;** by Lieut. E. B. Hunt, Corps of Engineers, U. S. A.

MAP PROJECTIONS CLASSIFIED AND DEFINED.

That department of descriptive geometry, or analysis, which treats of map and chart projections, has to deal solely with the terrestrial spheroid, and especially with the representation of the parallels and meridians subdividing its surface. As all localities, both on land and sea, are most readily and generally determined by latitude and longitude observations, so it is the most available and universal method, in constructing maps, to refer all positions to meridians and parallels as coördinate lines.

If we conceive the earth's surface reticulated by a complete framework of parallels and meridians, it is then the specific and uniform object of all modes of projection to represent these lines on a plane surface, in the most advantageous manner. But, as the spheroid is incapable of direct development on a plane, it only remains to present, in projection, the *best* approximation to similitude in form, relation, and proportional area in the parts of the earth's surface to be represented.

Ptolemy, Lambert, Euler, Lagrange, De l'Isle, Monge, La Croix, Puissant, Henry, Gauss, and others, have treated of projections in more or less detail, and some of them by methods of

* Extracted with modifications from the Coast Survey Report for 1853, in which is also included the formulæ for polyconic projections, with full tables and a description. The Tables suffice for the entire United States on either a large or small scale.

the highest grasp and compass.* This general to the following modes of projections, (all tecl some quite incorrectly so called,) each of which and most of which possess advantages for some maps or charts. This classified synopsis will serv precisely the relative value and precise character projection.

CLASS I.—Perspective projections on planes,	Orthographic. Globular, or equidistant. Stereographic. Gnomonic, or central.
CLASS II.—Developed perspective projections,	By a tangent cylinder. By a secant cylinder. By a tangent cone. By a secant cone.
CLASS III.—Projections by developing elements,	Cassini's. Flamstead's. Bonne's, or the modified Flamste Polyconic, (U. S. Coast Survey.)
CLASS IV.—Projections conformed to some arbitrary condition,	The flat chart, with equal latitude The flat chart, with latitudes = rad De Lorgna's. Ptolemy's modified conic. Mercator's.

CLASS I.

All simple perspective projections are made b eye at some particular point, and the plane of pr resentation to be pierced by all the rays, or rays tween the eye and all points of the parallels and r curves composed of all these piercing points of r perspective projection. A projection is practicabl ble position of the eye or plane, (except when th plane,) but only a few among this infinite numbe are convenient or eligible for construction.

In the *orthographic* projection the eye is assum distance, and the projecting rays are parallel lin plane of projection is perpendicular at any point d method circles are projected into ellipses, and th the projected hemisphere are very much crowded.

In the *globular* or *equidistant* projection, or Hire, the eye is placed at a distance from the cen = Radius + sine $45^\circ = (1+\sqrt{\frac{1}{2}})$ radius. The p

* Reference on this subject may be made with advantage to t treatises: Puissant, "Traité de Topographie," 1805; Henry, " jection des Cartes," 1810; "Memorial du Depot de la Guerre," Croix, "Precis Astronomique," in "Pinkerton's Geography;" Ba La Croix, vol. i, "Memorial Topographique et Militaire," Fra Geodesie;" Gehler, "Physickalisches Worterbuch;" Mayer's " 1828; "Littrow's Astronomy," 1812; Narrien's "Astronomy a Jackson's "Chartography," in "Manual of Geographical Science,

tion passes through the centre perpendicular to the central ray. This projection obviates the orthographic contraction or crowding and the stereographic exaggeration in the outer rim of a projected hemisphere.

In the *stereographic* projection, the eye is taken on the surface of the earth at the pole of the great circle used as a plane of projection. Circles are stereographically projected into circles. An increasing exaggeration of parts from the centre outwards is its great defect.

In the *gnomonic* or central projection, the projection is on a tangent plane—the eye is taken at the centre of the sphere. Great circles are gnomonically projected into straight lines, and all small circles into curves of the second order or conic sections. The entire hemisphere cannot thus be projected, and the portions become rapidly exaggerated in receding from the tangent points.

CLASS II.

Instead of projecting directly on planes, an intermediate cylinder or cone is employed in this class to receive the projection, which is then developed or rolled out on a tangent plane. The cylinder and cone being the only surfaces which can be developed by simple rolling out, and without elementary resolution, this class always requires the auxiliary use of one of these surfaces, which may be assumed, subject to several different conditions.

The projection on a *tangent cylinder* for development is made by placing the eye at the centre of the earth, and projecting the parallels and meridians on a cylinder tangent around the equator. On development, the parallels and meridians are found projected into perpendicular straight lines.

A *secant cylinder* may be so determined that the entire area of the spherical zone projected shall be exactly equivalent to its projection. These methods are limited in their advantageous application to a moderate equatorial belt.

In projecting perspectively on a *tangent cone* for development, the eye is assumed at the earth's centre, and the cone is taken tangent around the middle parallel of the zone to be projected. On developing this cone, the meridians appear as straight lines radiating from its vertex, and the parallels as circular arcs concentric around this point, the middle parallel being in its true length.

A *secant cone* may be taken which will give two parallels of correct length in the development, and much reduce the distortion of the extreme belts. This method of Ptolemy was revived by Mercator, and was used by De L'Isle in his map of Russia. Murdoch proposed to make the area of the conic frustum used equal to that of the projected spherical zone—a good condition, though inconvenient in construction. De L'Isle proposed to use a cone, through the limiting parallels. Euler proposed and de-

termined the cone which equalizes the errors and distortion on the central and the two limiting parallels. The use of two conic frustums—one for the north and one for the south half—has also been attempted, and advocated.

CLASS III.

The class of projections in which portions of the spherical surface are developed by being resolved into their differential elements, which are successively developed, is characterized by a peculiar elegance, and is of the highest importance. By this means, any portion of a spherical or spheroidal surface may be reconstructed on a plane with the most perfect attainable preservation of the relations and dimensions of its parts. This class of projections is far the best for representing limited areas, and can even be extended with advantage in some forms to *mappemondes*, or maps of the entire earth's surface.

Cassini's projection is made by first developing the central meridian of the area for projection into a straight line. A series of prime verticals or great circles perpendicular to their central meridian is passed at elementary distances along the meridian arc, all of which circles intersect in the spheric poles of the central meridian. These divide the surface into elementary rectangular isosceles triangles, or sectors, basing on the meridian elements. When the meridian is developed, these elementary triangles are conceived to be carried with it, and then to be severally developed into plane triangular elements. The elementary opening out between these developed areas may be neglected for some distance from the central meridian. Accordingly, a series of perpendicular straight lines through the graduations of the developed central meridian is taken as a substitute for parallels, and may be used as far as the opening out between elements can be neglected. Cassini's Map of France is thus projected; but, as its inaccuracy on the extreme east and west sheets amounts to 150 toises in 40,000, the use of this projection is not to be recommended for areas thus extensive. Du Séjour has developed the theory and formulæ of this method. As parallels of latitude do not enter, the latitudes of places must be derived indirectly, except on the central meridian.

Flamstead's projection is based on a resolution of the earth's surface into elementary zones or rings by parallels of latitude taken at successive elementary distances laid off along the central meridian of the area to be projected. Having developed this centre meridian on a straight line of the plane of projection, a series of perpendiculars is conceived to be erected at the elementary distances along this line. On or between these perpendiculars the elementary zones are conceived to be developed in their correct relations to each other and to the central meridian. Each

zone being of uniform width, occupies a constant breadth along its entire developed length, and consequently the area of the plane projection is exactly equal to that of the spheroidal surface thus developed. This demonstration applies directly to an analogous plane development of the surface of all supposable surfaces of revolution, be the generating meridian curve what it may, and even though the generatrix be one of double curvature. The meridians of the developed spheroid are traced through the same points of the parallels in which they before intersected them. They all cut the parallels obliquely, and are concave towards the central meridian. Thus, while each quadrilateral between parallels and meridians contains the same area and points after developments as before, the form of configuration is considerably distorted in receding from the central meridian, and the obliquity of intersections between parallels and meridians grows to be highly unnatural.

Bonne's, or the *modified Flamstead's* projection, to a great extent obviates this defect. It is the same as Flamstead's, except that the elementary zones, instead of being developed on right lines, are rolled out on concentric circular arcs described from the vertex of the cone tangent along the central parallel for their common centre. The great importance and wide use of this method induce a more detailed treatment of it under a subjoined heading.

The *polyconic projection*, being that for which the Coast Survey tables are prepared, will be specially explained further on in its proper place.

CLASS IV.

In addition to the perspective projections, the developed perspective, and the elementary development projections, there is a class in which some extraneous, arbitrary mathematical condition is imposed, giving rise to constrained or distorted delineations. The assumed condition is usually due to some practical consideration.

The *flat-chart* projection, with *equal latitude* degrees, is a rude method once much in use for charts. Two sets of equidistant perpendicular lines, composing a series of equal squares, were arbitrarily assumed as parallels and meridians to which all localities were referred by latitudes and longitudes. Hence resulted a gross distortion of figures, areas, and directions.

Another *flat-chart* projection was sometimes used, in which equidistant straight lines served as meridians; and for parallels a second set of straight perpendiculars at distances from the equator equal to those of the respective terrestrial parallels for which they stand. This is a radial projection on the circumscribing cylinder tangent along the equator, the radii of parallels being the only

projecting lines. Hence resulted a very distorted picture, but one in which each quadrilateral contains an area equal to and corresponding with its spherical correlative—a direct result of the relation between the sphere and circumscribing cylinder. This was the sole recommendation of the method.

De Lorgna's projection is chiefly employed as a polar projection of a hemisphere, for which use it is well adapted. A circle is determined equivalent in area to the hemisphere to be projected. Radii drawn to the graduations of its circumference represent meridians. A radius, graduated into ninety equal parts, is sometimes used as the latitude scale; but the chords of the polar distance of the parallels should be always employed. Hence results equality of areas between the projected and resultant quadrilaterals in general. Outlines are traced by latitudes and longitudes, as usual. For projecting a polar hemisphere, this method is most excellent, as rectangular intersection is combined with conservation both of figure and area.

Ptolemy's modified conic projection is made by using the concentric parallels of the pure conic development, and tracing curved or elliptical meridians across these in place of radial lines. By turning the convexities of these curves from the central line, and by skillful choice of curves, much of the distortion due to the extension of extreme parallels in development is obviated. This projection has been much used for maps of Asia, Africa, and America.

Mercator's projection is truly invaluable for navigators in laying long courses when out of sight of land, as these courses are always straight lines on the chart. Meridians are represented by equidistant parallel straight lines, and the parallels by a perpendicular set of parallel straight lines, whose distances from each other increase from the equator towards the poles in precisely the same ratio as the corresponding longitude degrees diminish. This projection results from the development of a cylinder tangent along the equator; the meridians being projected on their tangent elements, and the parallels being assumed as circles of right cross section at distances from the equator equal to the meridian arc of latitude divided by the cosine of the latitude—the earth's compression being neglected. By this means the relation of length between the latitude and longitude measurements on the chart is preserved uniformly the same as it is on the earth's surface. Tables of the increasing degrees have been computed, and are in general use for laying down parallels. Distances and areas are increasingly exaggerated and distorted as this projection is pushed towards the pole, making the scale very variable from point to point of an extended chart; but as the navigator computes his distance run, this variable scale is not by any means so serious a defect as to offset the invaluable facility with which

Mercator's principle enables him to run directly from one point to another. For the polar portion of the earth in which this projection totally fails, a central projection can be used to some distance. A projection on Mercator's principle might be made relative to the prime meridian instead of the equator, its prime verticals serving as the equidistant parallels, (as in Cassini's) and the circles parallel to the prime meridian being projected by the table of increasing degrees. This would require the investigation of the formulæ for conversion of coördinates in this case. The parallels and meridians of the earth might then be constructed by points. Another mode would be to make a radial and concentric circular projection around the pole, in which the length of the latitude degree should be deduced from the same condition as in Mercator's method, the divergence of meridians being duly considered. The amount of distortion in Mercator's projection wholly unfits it for land maps; and the variation of its scale in different parts would give rise to endless inconvenience were it applied to any other purpose than that of nautical off-shore charts, in which direction is so much more important than area or distance.

BONNE'S OR THE MODIFIED FLAMSTEAD'S PROJECTION.

This method of projection is that which has been almost universally applied to the detailed topographical maps based on the trigonometrical surveys of the several States of Europe. It was originated by Bonne, was thoroughly investigated by Henry and Puissant in connexion with the map of France, and tables for France were computed by Plessis.

In constructing a map on this projection, a central meridian and a central parallel are first assumed. A cone tangent along the central parallel is assumed, the central meridian is developed on that element of this cone which is tangent to it, and the cone is then developed on a tangent plane. The parallel falls into an arc with its centre at the vertex, and the meridian into a graduated right line. Concentric circles are conceived to be traced through points of this meridian taken at elementary distances along its length. The zones of the sphere lying between the parallels through these points are next conceived to be developed each between its corresponding arcs. Thus, all the parallel zones of the sphere are rolled out on a plane in their true relations to each other and to the central meridian, each having in projection the same width, length, and relation to its neighboring zones, as on the spheroidal surface. As there are no openings between consecutive developed elements, the total area must in this case, and in all like developments of surfaces of revolution, remain wholly unaltered by the development. Each meridian of the projection is so traced as to cut each parallel in the same point in which it intersected it on the sphere.

If the case in hand be that involving the greatest extension of the method, or that of the projection of the entire spheroidal surface, a prime or central meridian must first be chosen, one half of which gives the central straight line of the development, and the other half cuts the zones apart, and becomes the outer boundary of the total developed figure. Next, the latitude of the governing parallel must be assumed; thus fixing the centre of all the concentric circles of development. Having then drawn a straight line and graduated it from 90° north latitude to 90° south latitude, and having fixed the vertex or centre of development on it, concentric arcs are traced from this centre through each graduation. On each parallel the longitude graduations are then laid off, and the meridians are traced through the corresponding points. There results from this process an oblong kidney-shaped figure, which represents the entire earth's surface, and the boundary line of which is the double developed lower half of the meridian first assumed. If the vertex of the governing cone be removed to an infinite distance, the equator becomes the governing parallel, the parallels all fall into straight lines, and Flamstead's projection results. The kidney-shaped figure becomes an elongated oval, with the half meridian for one axis, and the whole equator for the other. A somewhat similar figure is obtained by placing the vertex at the pole, and reducing the tangent cone to a plane. An indented cusp at one end, and a rounding out at the other, will give an approximate pear-shape. Ptolemy's modified conic method reaches its full geometrical result in these forms, derived from the condition of *preserving areas* in tracing meridian curves.

Bonne's method is rarely applied to areas exceeding the limits of a State, but is invaluable for topographical maps of this description. The projection is made at once for the whole territory of the map, and the rectangular system of sheets laid out on the projection. Each sheet is numbered, and the coördinates of the corners are determined, so that the coördinates of intersection between parallels and meridians falling on each sheet are referred to its neat lines as axes.

This projection preserves in all cases the areas developed without any change. The meridians intersect the central parallel at right angles; and along this, as along the central meridian, the map is strictly correct. For moderate areas, the intersections approach tolerably to being rectangular. All distances along parallels are correct; but distances along the meridians are increased in projection in the same ratio as the cosine of the angle between the radius of the parallel and the tangent to the meridian at the point of intersection is diminished. Thus, in a full earth projection, the bounding meridian is elongated to about twice its original length. While each quadrilateral of projection preserves

its area unchanged, its two diagonals become unequal; one increasing and the other diminishing in receding towards the corners of the map, the greatest inequality being towards the east and west polar corners. Though great circles between stations on the earth are generally projected into curves, the amount of deviation for moderate limits is very slight on a Bonne projection. The scale is nearly uniform over the entire projection, being accurate along the parallels and along their radii, but being too great along one diagonal of the quadrilaterals, and too small along the other. In an area of 120° longitude and 70° latitude, a distance of 7,000 miles is in error but $\frac{1}{55}$th. This projection has thus many excellent qualities for topographical maps; and its defects of oblique intersections, of unequal diagonals, and of scales varying with the point of the compass, are not very serious in a limited area, as in the map of France, or that of England and Wales. A special set of tables for each central parallel is required in this method; and the extent of these is so vast as to make impracticable the conception of a universal set of tables. The French tables of Plessis are based on the parallel of 50gr. or 45°, and are available for any area centered on this line, except that the old compression was used in computing them. But to construct tables for Bonne's projection for use in the disconnected local maps of our country was impracticable, as no central parallel could be assumed for them all. Were a general topographical map of the United States to be made, a central parallel might be assumed for that purpose; but even in this case the question should be carefully weighed, whether the Bonne projection would be as desirable as the rectangular polyconic.

THE POLYCONIC PROJECTION, ITS PROPERTIES AND VARIETIES.

The operations of the coast survey being limited to a narrow belt along the seaboard, and not being intended to furnish a map of the country in regular uniform sheets, it is preferred to make an independent projection for each plane-table and hydrographic sheet, by means of its own central meridian. These sheets embrace areas so limited as to exhibit in projection no sensible distortion of figure, and they individually agree with nature far more perfectly than they would if arranged as parts of a rectangular series projected on Bonne's method. In fact, each sheet is projected strictly as a local map, and its connexion with the adjoining sheets is established solely by the points of triangulation. In reductions, including several sheets, the plotting of points is the first step, and the change of scale is then made by corresponding squares. By the aid of the coast-survey tables, a *rectangular polyconic* projection can at once be made for each locality or subdivision of the United States, or for the United States as a whole.

The name *rectangular polyconic* projection is applied to the method in which each parallel of the spheroid is developed symmetrically from an assumed central meridian by means of the cone tangent along its circumference. Supposing each element thus developed relative to the common central meridian, it is evident that a projection results in which all intersections of parallels and meridians take place at right-angles.

Let the most general case, or that of the entire spheroid, be first assumed, the development being made, for instance, relative to the meridian of Washington. Starting at the north pole, the tangent cone there has then its limiting form, or it coincides with the tangent plane. Taking then the elementary parallels successively southward, the vertex of the moving tangent cone recedes along the prolonged earth's axis, giving to the developed parallels a receding centre and an increasing radius as the latitude diminishes. At latitude 45° the terrestrial and development radii become equal. At the equator the vertex recedes to an infinite distance north, or the cone becomes a cylinder, and the equator falls in a straight line perpendicular to the meridian. On passing to the south the vertex approaches from an infinite distance south, the parallels change their concavity southward, while the curvature, increasing in an inverse order, becomes infinite at the pole, or the polar parallel falls in a point. There results from this process a biaxial figure, with four equal quadrants, the short axis being the rectified Washington meridian, (180° in length) and the long axis being the entire rectified equator, or about twice the length of the shorter one. A re-entering cusp marks the bounding curve at each pole, and the meridian, 180° from Washington, which circumscribes each half of the figure, is elongated on each side to more than twice its original length by the development. Over the entire area of this projection all parallels and meridians intersect at right angles, and the diagonals of each projected quadrilateral are everywhere nearly equal to each other. The scale on N. and S. lines near the border is somewhat enlarged, but is very correct on E. and W. lines, while along both diagonals it is somewhat enlarged, though nearly equally so on each. On the whole there results from this method much less of local distortion than from Bonne's projection. Equality between the spheroidal and developed areas is not preserved, but the departure from equivalency is not great in amount.

As rectangular intersections and preservation of areas are not both attainable at once, it becomes a question of preference between them in each case. It is also a question whether it might not in some cases be advantageous still further to sacrifice the preservation of areas so as to make the same scale applicable in all azimuths at each point. This would require the longitude degrees to increase from the centre meridian outwards in the

same ratio as the corresponding projected meridional degrees. This condition would determine a new polyconic projection, whose scale, from point to point, (an element which in Bonne's, and the simple polyconic projection, is a function both of the central meridian distance, and azimuth) would become a function of the central meridian distance only, and would increase alike in all directions on receding from this line. Such a projection would reduce distortion of local configuration, to an absolute minimum, and the areas in projection would be proportional to the squares of their local graphic degrees. This would enable us to take strict account of those irregularities of scale which now lurk in disguise. But it would be a great labor to prepare the tables requisite for its ready use, and there would be some valid objections to its results. In a large topographical map thus projected, the scale of each sheet could be derived and engraved on its plate, making the sheet quite homogeneous on that scale, and perfect in the preservation of its configuration. Were a topographical map of the United States to be undertaken on a liberal scale, this projection might be found superior to any other, as in each sheet areas, dimensions, relations, and rectangular intersections, would be well preserved according to its own scale, giving it the greatest local perfection, while it would also combine correctly in its proper place. It should be stated that this projection is novel and untried.

The method of projection in common use in the Coast Survey office for small areas, such as those of plane-table and hydrographic sheets, may be called the *equidistant polyconic.* This ought to be regarded rather as a convenient graphic approximation, admissible within certain limits, than as a distinct projection, though it is capable of being extended to the largest areas, and with results quite peculiar to itself. In constructing such a projection the central meridian and a central parallel are chosen, and they are constructed just as in the rectangular polyconic method. The top or bottom parallel, and a sufficient number of intermediate ones to determine the meridians with proper correctness, are constructed by the tables, and the meridians are drawn. Then starting from the central parallel, the distance to the next parallel is taken from the central meridian and laid off on each other meridian. A parallel is traced through the points thus found. Each parallel is constructed by laying off equal distances on the meridians in like manner, and the tabular auxiliary parallels are, all except the central one, erased. In fact, as only the points of intersection are required, the auxiliary parallels should not be actually drawn. From this process of construction results a projection in which equal meridian distances are everywhere intercepted between the same parallels.

If we conceive this projection extended to include the entire earth, a singular result appears. Taking the equator as the central parallel, all the parallels become concave towards this line; for the distance between parallels measured along the curved meridians being constructed equal to that along the central straight meridian, it is evident that the parallels must converge in receding from the central meridian. The parallel of 90°, or the polar point, becomes extended into a curve, whose length approaches that of the developed equator. It will be seen that each parallel falls nearer the equator than it would in Flamstead's projection, being, indeed, tangent on the equatorial side of the Flamstead perpendicular. Thus, in this method the projected area is less than that of the spheroidal surface. If an equidistant polyconic projection be made on the same central meridian and parallel as a Bonne projection, its area will in like manner be less for each rectangle and for the aggregate; hence this projection, where extended to a great surface, always gives its area too small. It also makes its meridian arcs unduly short, and the extreme parallels much too long; giving a grotesqueness to the polar regions bordering on that of a Mercator projection. The scale becomes in some parts excessively dependent on azimuth; the distortion in the polar corners is very great; the intersections are far from rectangular, and they are so conditioned as not to be readily computed. From these defects, so gross in the developed spheroid, and still great even in a map of the United States, it is clear that the *polyconic-equidistant* projection ought by no means to be extended beyond the most moderate limits. A square degree, on a scale of $\frac{1}{10000}$, may be taken as a limit, beyond which no convenience of graphic construction should induce the use of this approximation. Beyond this limit the *rectangular-polyconic* method should be employed, at least in all Coast Survey projections.

The polyconic method of projection has been developed in the Coast Survey office, and tables, prepared for facilitating its use, were there computed, and are now first published in the Report for 1853.

GRAPHIC CONSTRUCTION OF POLYCONIC PROJECTIONS—COAST SURVEY METHODS.

Having fixed the limits to be covered by the projection, the central meridian is represented by a straight line, as nearly as practicable, through the centre of the sheet. From an assumed starting-point on this line are laid off the successive meridian arcs, as taken from the tables.

Rectangular polyconic method.

Through each point of division on the central meridian, given by these tabular arcs, erect a perpendicular to it by means of a

well-tested right-angled ruler, with twenty-four inch legs, and a hard pencil; or first carefully construct a single accurate perpendicular by sweeping intersecting arcs with the beam-compass, and then draw on each side a parallel to the central meridian, on which lay off the meridian distance from the perpendicular, and draw the parallel lines through the three equidistant points thus obtained for each. Take from the tables for each required point of intersection between parallels and meridians, its appropriate length of arc of parallel, from which subtract the corresponding x. Lay off this difference from the central meridian each way on its proper perpendicular, and erect, towards the pole, at the point so formed, a perpendicular equal to the corresponding value of y in the tables—its extremity is the point of intersection required. Through all the corresponding points of intersection trace the parallels and the meridians. Erase the auxiliary lines, and write on the margin the numerical latitude and longitude.

The following mode is more rapid and better checked: Lay off first the longest arcs of parallel, and then take the length of a single subdivision of the parallel in a pair of hair-spring dividers, and step it off on the perpendicular to the right and left of the central meridian, being careful not to prick the paper. Having adjusted the dividers so as to bring the extreme points thus obtained to a perfect agreement, prick lightly the subdivision points. Take from the tables the successive values of x for each point, and when these are sensible on the scale used, lay them off back towards the central meridian from the points before obtained, and erect, at the last points, perpendiculars equal to their respective values of y. As x is always small, and for some entire projections quite insensible, this method is much more convenient than that of using all the while long distances; but the check of a total measurement on each parallel is quite indispensable, as an insensible error in taking the subdivision distances grows, by repetition, to be very evident in the check measurement.

Equidistant polyconic method—(Inadmissible in projections covering more than one square degree.)

Proceed as before to graduate the central meridian, and to construct a central parallel. Construct the points of meridian intersection with the top and bottom parallels, and as many intermediate parallels as are requisite closely to determine the meridians. Through these points then draw the meridians. Starting now from the central parallel, lay off on each meridian the distance to the required parallel equal to that on the central meridian, and trace the parallels through these points. Proceed in like manner to construct the others, using always the central parallel as a base, and the totals measured from it along the central meridian in laying off.

This method requires much less recourse to the tables than the other, and is sufficiently accurate, within a square degree, on a scale of $\frac{1}{10000}$. The x and y may often be neglected as insensible in small projections; but no value of x, which is at all appreciable on the scale used, should be neglected. The y, for the auxiliary parallels, affects the meridian less rapidly, but its palpably sensible values should always be used.

The following quantities are sensible, yet only barely sensible, on the scales affixed:

12	metres on a	scale of	$\frac{1}{80000}$.
10	"	"	$\frac{1}{60000}$.
8	"	"	$\frac{1}{40000}$.
6 or 5	"	"	$\frac{1}{30000}$.
3	"	"	$\frac{1}{20000}$.
$1\frac{1}{2}$ or 2	"	"	$\frac{1}{10000}$.

These quantities are quite overshadowed in large sheets by the expansion and shrinkage of drawing-paper from day to day. In both methods the x and y should always be used for good projections when they would be sensible on the scale in use. And it is peculiarly essential to accurate projections that the hygrometric condition of the paper be kept as uniform as possible during all the time that measured distances are being laid down. It is often better to mark simply the intersection points by a small cross +, and to omit the remainder of the parallels and meridians. For plotted points this is also the best indication, if the cross lines are stopped on each side of the point, just far enough off to leave the dot distinct.

For drawing parallels and meridian curves, a long, slender, flexible ruler of straight-grained cedar, or other compact wood, is employed. Its cross section is three-sixteenths ($\frac{3}{16}$) of an inch by two sixteenths ($\frac{2}{16}$) of an inch. A specially designed steel ruler might be found preferable. There is a small groove on the top of the ruler, and its ruling edge is slightly beveled. Leaden, paper-covered, beak weights, of about four pounds weight each, are used to hold the ruler in place from point to point. These are so shaped as not to incommode the hand in ruling, and each has a hooked beak, ending in a knife-edge, turned downwards, which, resting in the ruler groove, throws the main bearing of the weight on the ruler, while its small end rests on the paper. The beak weights in use are five (5) inches long, two and one-eighth ($2\frac{1}{8}$) wide, and two and one-eighth ($2\frac{1}{8}$) deep, the beak being five-eighths ($\frac{5}{8}$) of an inch long, and turned down one-fourth ($\frac{1}{4}$) inch. The mass of lead is nearer the beak end. Having placed the ruler approximately, it is so adjusted under a beak weight to the first point that the curve will be ruled exactly through it. It is then adjusted under a second weight to the

next point, and then bent to the next in like manner, and so on until the entire curve is completed. Before ruling this the eye should criticise it carefully, as a check on graphic errors. For fine projections the hardest pencils are best; and in inking, the lines should be drawn as delicately as clearness permits.

When no metre scale is at hand, the tabulated distances can be converted into yards by using the conversion tables, or by the constants of relation between units; or, when the greatest accuracy is not important, a metre scale can readily be constructed from a yard or foot scale by proportionality. Thus, rule two parallel scales, one of yards and one of five-sixths ($\frac{5}{6}$) yards, and draw a third parallel, whose distance outside the yard-scale is $\frac{3.3685}{6}$th of that between the yard and five-sixth yard scales. Through the similar graduations draw straight lines; these will give a metre scale by their intersections. If space permits, a point may be substituted for the five-sixths ($\frac{5}{6}$) yard scale. The projection once constructed, may be used independent of the unit of the tables.

Art. XXXVI.—*On the Educational Uses of Museums;* by Edward Forbes, F.R.S., &c.*

The third Session of the Government School of Science applied to Mining and the Arts commences to-day. The Director and my Colleagues have assigned to me this year the duty of opening the courses. I shall avail myself of this opportunity to offer some remarks upon the leading and characteristic features of the Institution, considered as an educational Museum, and to make some observations upon the instructional uses to which Museums may be advantageously applied.

The school of applied sciences here established is the only instance in Britain of an organized instructional institution arising out of a Museum, and being maintained in strict connection and relation with its origin. This is not an accident, but an event contemplated from the commencement of the Geological Survey. It is an experiment on a considerable scale with a greater purpose,—for, with a limited though rapidly improving machinery, it is intended to advance educational aims that have a vital importance in their bearing on the future prospects of this country. It is an endeavor by a State-mechanism to cast the seeds of science over the broad fields of British industry,—not indiscriminately, but especially in those places where there is a good soil thirsting for their germination. We who are appointed to be

* Introductory Lecture at the Metropolitan School of Science, etc.; Museum of Practical Geology. London, 1853.

cultivators have a responsible duty and a noble task. We have firm faith in the dignity our work, and in the certainty of good results arising from it. This must be our reward; and with it we are content, as long as we can, to labor patiently and earnestly to the best of our endeavors,—hopeful of the approbation and coöperation not only of our fellow-laborers in science, but also of all intelligent and patriotic Britons.

The results so far of the teaching here have been in the main highly satisfactory. With the close of last session terminated the two years curriculum of the students who entered the Government School of Mines in 1851. Since their studies are now completed, I may speak of the men in the language not of compliment, for of that there is no necessity, but of unmixed praise. I can say this not only for myself but for all my colleagues; and we have the delightful satisfaction of anticipating a distinguished scientific and practical career for those who were lately our pupils, and whom now we number among esteemed friends. Their services are sure to be appreciated and anxiously sought for; and already we have had the pleasure of congratulating some of them on the obtainment of highly-valuable and honorable posts, for which they had become qualified within these walls.

With equal satisfaction we can refer to the department of our lectures devoted to the instruction of working men. The artisans of London have eagerly and admirably responded to the opportunity so freely offered to them by Government in this Institution. They have crowded to our theatre and attended our courses with unmistakable earnestness and intelligence. To address the audience, composed exclusively of working men, assembled on these benches on Monday evenings has been a privilege and a pleasure to all of us.

The peculiar advantages which we have held out to officers of the public service, especially to officers of the army and navy, have not been neglected by the class for whom they were intended. At the same time, it was expected that more use would have been made of them. It is difficult, doubtless, for gentlemen whose duties command so much of their time, to make days and hours suit their convenience. Those who entered our classes, have been among the most diligent of students. The majority have come to us from the East India Company's service, chiefly officers of engineers and medical officers. Much might be done for the advancement of science by naval and military men when on service abroad, and much is being done by them every day as the transactions of learned societies can amply testify. An occasional course of study in one or more of the sciences taught here would enable many a soldier and sailor to occupy vacant hours with pleasure and advantage, and possibly with benefit to the general advancement of knowledge. During the time when I had the

honor of assisting on board one of Her Majesty's surveying ships, I witnessed the happiness and profit that resulted from the pleasure taken by a corps of naval officers in scientific pursuits.

It was supposed that opportunities for scientific instruction such as are here afforded would have been appreciated by intelligent persons among the middle and higher ranks, having time at command. With the exception of a chosen few, the anticipation has proved fallacious. Possibly the occult science of table-turning, which in these days seems to occupy the place filled by astrology in days of yore, has too seriously occupied their thoughts to permit of chemical, physical, geological, or biological studies. In London there are several institutions of high character, that offer, at reasonable cost, scientific instruction to the so-called "educated" classes; yet if the numbers of all, young and old, who avail themselves of the chances that are placed within their reach were to be summed up, scanty indeed would the proportion appear who appreciate, as compared with the vast majority who neglect, the opportunity. Need we wonder then at the success of popular follies and absurdities among persons to whom, if we applied the epithet "unenlightened," we should give mortal offense? There is, indeed, no stronger argument in favor of the State taking the initiative in scientific instruction of the kind given here, than the fact, that the classes of the people who cannot afford to pay high fees, or come to learn during the hours of the day, are anxious and thankful for it; whilst those who ought to support deserving institutions of private foundation have yet to be imbued with a taste for natural knowledge, before they will do that which should be at once a duty and a pleasure.

This year our resources, though still too limited, have been considerably extended, and an important and indispensable want supplied, through the institution of a lectureship on Applied Mechanics. It is with feelings of exultation that I venture to allude to the manner in which this new post has been filled. The accession to our corps of so eminent a philosopher as Professor Willis is an honor deeply appreciated by all of us. In him we feel that we have acquired a new source of strength, whose value cannot be too highly reckoned. We feel, too, that in the world of science, and in the world of mechanical industry, the approbation of this appointment is universal.

In the presence of Dr. Hofmann, who though appointed to the lectureship on Chemistry and charge of the Laboratory, since the conclusion of last session, has sat with us and served amongst us for some time, I will not—I need not—enter on any eulogium of his distinguished merits. To have secured the services of one of the most eminent of European chemists, for the post until lately so ably filled by Dr. Lyon Playfair, is as great a satisfaction to ourselves, as it will be a guarantee of good work to the public.

His predecessor has left us for a post of heavy responsibility and inestimable importance,—one on the conduct of which the success of government institutions for scientific education will in a great measure depend. He has left us with our warmest wishes for his success, and our firmest confidence in his ability, energy, earnestness, and truthfulness. But though no longer holding a professorial post here, we retain the benefit of his advice and counsel, since he still remains connected with our institution, and sits with us as a member of our Educational Committee.

We commence the session—so far as the class of students of most consequence, viz., the matriculated class, is concerned—under peculiarly favorable auspices. The number of entries is greater at this early stage of the courses than during either of the former years. Considering how difficult it is in our country for any establishment on a new plan to make way, this evidence of progress may be taken as a fair subject for congratulation.

The object of the Museum in which we are now assembled is mainly the illustration of the mineral constitution and products of the British islands, and to some extent, of the British colonies. This purpose, whether we consider the great benefit derived from mineral wealth by our nation at large, the vast capital invested in the search after and application of mineral resources, or the light thrown upon science under its nobler and less profitable forms, cannot but be esteemed a worthy one. To carry it out effectively would require more than double the space here assigned to it, and powers of speedy and comprehensive action such as are not usually conferred upon the managers of State institutions. The purpose of the place in some of its branches is more or less fully presented, but in others is barely sketched or rather indicated. The applications of mineral products to the various useful and ornamental arts are so numerous, that, except in a few principal instances, it would be folly to attempt their illustration within our confined boundaries. Consequently, in a purely industrial direction our display is sketchy and partial. That a collection fully and judiciously illustrating the arts that spring from the world of minerals, treated with equal regard to their present extension and past history, to their excellencies, capabilities, and defects, would be in the highest degree instructive and beneficial, if employed in the illustration of well-devised courses of instruction, there cannot be a doubt. If ever such a collection be formed, this institution may fairly claim the credit of its paternity.

In one of its departments this Museum aims at more amplitude; and even proceeding at our present somewhat tardy pace—inevitably so, as we are situated,—must in the end attain, or at least nearly approach, completeness. I allude to that devoted to the illustration of the geological structure of the British Islands. You are aware that we are here an establishment in intimate connex-

tion with, and many of its officers of, the Geological Survey of the United Kingdom. Perhaps the most distinctive feature of this Museum is, that it is the visible evidence of the bearings and progress of that survey. When the officers of the Royal Engineers have performed their duty as topographers, and given to the public the admirable and invaluable maps issued by the Ordnance, then the members of the corps of Government geologists go over the ground anew with the distinct and important office of delineating its mineral structure. On electrotype casts of the original Ordnance plates, the new lines traced by the officers in charge of the geology are laid down, and the new map so constituted is issued to the public at an expense corresponding to the cost of fresh work and coloring. My colleagues in the field to whom these labors are assigned are engaged in a laborious task, requiring judgment, skill, training, and high scientific acquirements. Theirs, no more than ours, in this museum and school, are not mere duties of routine, office, clerkship, or limited hours. There is no off-duty; the head must work whilst the eyes are open if our task is to be well and thoroughly done.

Whilst the collections here displayed are mainly confined to the mineral products of the British islands, there is one department in this building, represented at present by three or four wall-cases, that I cannot refer to without the deepest interest, insignificant though it may now seem. I allude to that of Colonial Geology. The idea of it is to exhibit the mineral products of each of our colonies separately, the evidences of their geological constitution, and the indications of their mineral wealth. Such a display would be more than a curious and interesting illustration of the products of those countries for the benefit of persons at home. It would be a source of instruction of the most vivid kind to all thoughtful men intending to emigrate,—and most emigrants are thoughtful, at least before they go. Over and over again, when working at the arrangement of the cabinets in our galleries, I have been addressed by intelligent persons of this class who have come here in the hope of meeting with a collection of the kind I have mentioned, and of passing some time in the study of it. With feelings akin to shame I have shown them our shabby though not worthless display, and endeavored to make it the text of conversation and advice. Surely it would be worthy of a great empire like ours to possess, in the metropolis of all its world-strewn states, some sufficient illustration of their structure and productions. I speak not merely of their mineral productions, which are all that we can aim at here, but of their works of art and industry, their natural productions of all kinds, and illustrations of their history and of their ethnology. It is true that many of these are embodied in general collections, and form an essential part of systematically arranged cabinets. But what we

require is to see them distinctly grouped with regard to their geography; so that, for example, the emigrant proceeding to Australia might come and learn before he departed, and the officer ordered on duty to India or the West Indies might acquire an acquaintance with the structure and products of those countries that would enable him when there to occupy his spare time in research useful to himself and beneficial to his country. All that is required for carrying out such a collection is space. Contributors anxious and able to assist would be found in numbers. Those who have derived some benefit and knowledge from their studies in the Museum before leaving, would when abroad add judiciously and gratefully to its contents. Indeed there are at present extensive and valuable collections of colonial specimens lying useless, packed in boxes, that might be had for the asking, provided it could be shown that there was a proper place in which to arrange them for the public benefit.

Museums, of themselves alone, are powerless to educate. But they can instruct the educated, and excite a desire for knowledge in the ignorant. The laborer who spends his holiday in a walk through the British Museum, cannot fail to come away with a strong and reverential sense of the extent of knowledge possessed by his fellow-men. It is not the objects themselves that he sees there and wonders at, that make the impression, so much as the order and evident science which he cannot but recognize in the manner in which they are grouped and arranged. He learns that there is a meaning and value in every object however insignificant, and that there is a way of looking at things common and rare distinct from the regarding of them as useless, useful, or curious,—the three terms of classification in favor with the ignorant. He goes home and thinks over it; and when a holiday in summer or a Sunday's afternoon in spring tempts him with his wife and little ones to walk into the fields, he finds that he has acquired a new interest in the stones, in the flowers, in the creatures of all kinds that throng around him. He can look at them with an inquiring pleasure, and talk of them to his children with a tale about things like them that he had seen ranged in order in the Museum. He has gained a new sense,—a thirst for natural knowledge, one promising to quench the thirst for beer and vicious excitement that tortured him of old. If his intellectual capacity be limited and ordinary, he will become a better citizen and happier man; if, in his brain there be dormant power, it may waken up to make him a Watt, a Stephenson, or a Miller.

It is not the ignorant only who may benefit in the way just indicated. The so-called educated are as likely to gain by a visit to a Museum, where their least cultivated faculties, those of observation, may be healthily stimulated and brought into action. The great defect of our systems of education is the neglect

in *educating* the observing powers,—a very distinct matter, be it noted, from scientific or industrial *instruction*. It is necessary to say this, since the confounding of the two is evident in many of the documents that have been published of late on these very important subjects. Many persons seem to fancy that the elements that should constitute a sound and manly education are antagonistic,—that the cultivation of taste through purely literary studies and of reasoning through logic and mathematics, one or both, is opposed to the training in the equally important matter of observation through those sciences that are descriptive and experimental. Surely this is an error; partizanship of the one or other method or rather department of mental training, to the exclusion of the rest, is a narrow-minded and cramping view from whatsoever point it be taken. Equal development and strengthening of all are required for the constitution of the complete mind, and it is full time that we should begin to do now what we ought to have done long ago. Through the teaching of some of the sections of natural history and chemistry,—the former for observations of forms, the latter of phenomena,—I cannot but think the end in view might be gained, even keeping out of sight altogether, if the teacher holds it best to do so, what are called practical applications. For this branch of education museums are the best text-books; but, in order that they should be effectively studied, require to be explained by competent teachers. Herein at present lies the main difficulty concerning the introduction of the science of observation into courses of ordinary education. A grade of teachers who should be able and willing to carry science into schools for youth has hardly yet appeared. Hitherto there have been few opportunities for their normal instruction. Now, in a great measure, this defect may be considered as removed; and in the metropolitan schools of science and art connected with the Board of Trade there are ample opportunities afforded for the acquirement of scientific knowledge in the required direction by persons who purpose to become educators.

In their instructional aspect, considered apart from their educational applications, the value of Museums must in a great measure depend on the perfection of their arrangement and the leading ideas regulating the classification of their contents. The educated youth ought, in a well-arranged museum, to be able to instruct himself in the studies of which its contents are illustrations, with facility and advantage. On the officers in charge of the institution there consequently falls a serious responsibility. It is not sufficient that they should be well versed in the department of science, antiquities, or art committed to their charge. They may be prodigies of learning, and yet utterly unfitted for their posts. They must be men mindful of the main end and purpose in view, and of the best way of communicating knowledge according to

its kind, not merely to those who are already men of science, historians, or connoisseurs, but equally to those who as yet ignorant desire to learn, or in whom it is desirable that a thirst for learning should be incited. Unfortunately museums and public collections of all kinds are too often regarded by their curators in their scientific aspect only,—as subservient to the advancement of knowledge through the medium of men of science or learning, and consequently as principally intended for the use of very few persons. This is not the main purpose for which the public money is spent on museums, though one of the very highest of their uses, and in the end of national consequence, since the surest measure of national advancement is the increase and diffusion of scientific and literary pursuits of a high grade. One of the signs of a spread of sound knowledge and intellectual tastes in a country is the abundant production of purely monographic works by its philosophers, and the evidence of their appreciation by the general mass of readers, as indicated by the facility with which they find publishers.

Very few museums present much of an industrial aspect, valuable, interesting, and popular as any arrangement or display of their contents under this point of view must evidently be. The noble invention of the Great Exhibition, a glory to the end of time around the name of one of the most enlightened princes, proved to all men the high and national interest inherent in industrial collections. It is indeed strange that amongst a people so essentially industrial in their habits, occupations, and modes of thought as the English nation, no great and comprehensive collections illustrative of their agriculture, manufactures, machinery, and sources of trade should have been formed long ago. This defect in our institutions is, however, rapidly in the course of being removed; and I need not dwell upon the value of a kind of museum, of which all sensible men now understand the importance.

It has long been a subject of discussion, in what manner and to what extent can instruction by means of lectures and public teaching be advantageously associated with public collections. There are those who are opposed to such a course, holding that museums should stand on their own exclusive merits, and be mainly places of personal study and consultation. This, however, is the contemplation of them under their scientific aspect only; and though it may fairly be maintained, that a great central collection, such as the British Museum, may be rendered most serviceable by this course of action, holding that magnificent establishment as a general index for science, and, as it were, Encyclopedia of reference,—I feel convinced, after a long and earnest consideration of the question for many years, that unless connected with systems of public teaching, museums in most instances are

of little use to the people. The most useful museums are those which are made accessory to professorial instruction, and there are many such in the country, but almost all confined to purposes of professional education, and not adapted for or open to the general public. The museums of our Universities and Colleges are, for the most part, utilized in this way, but the advantages derived from them are confined to a very limited class of persons. In this Institution, an endeavor has been made to render its contents subservient to the cause of education and instruction; and the course which is here taken may be imitated with advantage in the provinces, where there are not unfrequently collections of considerable extent turned to small account for the benefit of the residents, a large proportion of whom in many instances are ignorant of their very existence. Yet it is to the development of the provincial museums, that I believe we must look in the future for the extension of intellectual pursuits throughout the land, and therefore I venture to say a few words respecting what they are and what they should be.

When a naturalist goes from one country into another, his first inquiry is for local collections. He is anxious to see authentic and full cabinets of the productions of the region he is visiting. He wishes, moreover, if possible, to study them apart—not mingled up with general or miscellaneous collections,—and distinctly arranged with special reference to the region they illustrate. For all that concerns the whole world or the general affinities of objects he seeks the greatest national collections, such as the British Museum, the Jardin des Plantes, the Royal Museums at Berlin and Vienna. But that which relates to the particular country he is exploring, he expects to find either in a special department of the national museum, or in some separate establishment, the purpose of which is, in a scientific sense, patriotic and limited. So also with the students of history and antiquities; they are often disappointed, and in the end find what they require here and there, bit by bit, in the cabinets of private individuals. In like manner, when the inquirer goes from one province to another, from one county to another, he seeks first for local collections. In almost every town of any size or consequence he finds a public museum, but how often does he find any part of that museum devoted to the illustration of the productions of the district? The very feature which of all others would give interest and value to the collection, which would render it most useful for teaching purposes, has in most instances been omitted, or so treated as to be altogether useless.

Unfortunately not a few country museums are little better than raree-shows. They contain an incongruous accumulation of things curious or supposed to be curious, heaped together in disorderly piles, or neatly spread out with ingenious disregard of their relations. The only label attached to nine specimens out of ten is,

"Presented by Mr. or Mrs. So-and-so;" the object of the presentation having been either to cherish a glow of generous self-satisfaction in the bosom of the donor, or to get rid—under the semblance of doing a good action—of rubbish that had once been prized, but latterly had stood in the way. Curiosities from the South Seas, relics worthless in themselves, deriving their interest from association with persons or localities, a few badly stuffed quadrupeds, rather more birds, a stuffed snake, a skinned alligator, part of an Egyptian mummy, Indian gods, a case or two of shells, the bivalves usually single and the univalves decorticated, a sea urchin without its spines, a few common corals, the fruit of a double cocoa-nut, some mixed antiquities, partly local, partly Etruscan, partly Roman and Egyptian, and a case of minerals and miscellaneous fossils,—such is the inventory and about the scientific order of their contents. I have a vivid remembrance of going through the Cheetham collection at Manchester, and hearing the explanation of its contents by one of the boys on the foundation, when I was of small size myself. The peculiar classification that mystified sightseers thirty years ago is in too many instances still maintained.

There are, however, admirable exceptions to this censure. There are local collections arranged with skill and judgment in several of our county towns, and which at a glance tell us of the neighborhood and activity of a few guiding and enlightened men of science. It would be invidious to cite examples, and yet the principles, in each case distinct, adopted in the arrangement of those of Ipswich and Belfast ought especially to be noticed. In the former, thanks to the advice and activity of Professor Henslow, the specimens of various kinds, whether antiquarian, natural history, or industrial, are so arranged as to convey distinct notions of principles, practice, or history. In the Belfast Museum the eminent naturalists and antiquarians who have given celebrity to their town have made its contents at a glance explanatory of the geology, zoology, botany, and ancient history of the locality and neighboring province. The museums of Manchester, York, Scarborough and Newcastle might be cited as highly commendable likewise, thanks to the science and ability of the eminent men connected with them, or who have taken an interest in their formation. It so happens, however, that the value and excellence of almost every provincial museum depend upon the energy and earnestness of one, two, or three individuals, after whose death or retirement there invariably comes a period of decline and decay. Now this should not be, and would not be were the facilities for scientific and literary instruction in the provinces greater than they are. In very few instances do we find the collections freely open to the public. In most cases they are unassisted by local or corporate funds, and dependent entirely upon the subscriptions of private individuals. Indeed, any attempt to favor the establish-

ment of public museums and libraries through the application of local funds is opposed with a horrible vigor more worthy of a corporation among the Cannibal Islands than within the British Empire. The governing bodies of too many of our towns include no small proportion of advocates of unintellectual darkness. It is not the interest of the public but that of the publican which sways, when a councillor wiser than the rest proposes in vain to inform his fellow-citizens through the agency of free museums, libraries and gardens. This may seem a harsh and possibly a rash censure, but I speak deliberately and with knowledge of examples. And yet, alas, the direful sway of distilleries and breweries may be excused, when we learn that in some, be it hoped few instances, the proposition to establish public libraries by means of a small local rating has been opposed by the members of local so-called philosophical institutions, on the plea that having got what they wanted in this way for themselves they did not choose to pay a tax for the extension of these advantages to their less fortunate fellow-citizens.

In every museum of natural history, and probably in those devoted to other objects, there gradually, often rapidly, accumulates a store of duplicates that if displayed in the collection render it more difficult to be studied than if they were away altogether, occupying as they do valuable space and impeding the understanding of the relations and sequence of the objects classified. If, as is sometimes the case, they are rejected from the collection and stowed away in boxes or cellars, they are still in the way; for cellarage and storage—as we know here, from the want of them, to our detriment,—are indispensable for the proper conducting of the arrangements of museums. Yet out of these duplicates, more or less perfect sets of specimens might be made up, of very high value for purposes of instruction. A well-organized system of mutual interchange and assistance would be one of the most efficient means of making museums generally valuable aids to education. Much money, when money is at the command of curators or committees, is spent in purchasing what might be obtained for asking or through exchange. Some objects of great scientific interest, but equally costly, might be purchased by one establishment only, and made fully as useful, instead of being bought in duplicate by two or more contiguous institutions. The larger institutions might supply the smaller; and out of the national stores, numerous examples—to them almost worthless, but to provincial establishments highly valuable—might be contributed with facility and greatly to the public benefit.

It is in this way, viz. by the contribution of authenticated and instructive specimens, that the museums supported by the State can most legitimately assist those established from local resources in the provinces: the scientific arrangements of the latter might also be faciltated through the aid of the officers attached to Gov-

erment institutions. Money grants would do in many cases more harm than good, destructive as they are of a spirit of self-reliance, and apt to induce a looseness of expenditure and habits of extravagance.

At the same time, every shilling granted judiciously by the State for purposes of education and instruction, for the promotion of schools, libraries, and museums, is a seed that will in the end generate a rich crop of good citizens. Out of sound knowledge spring charity, loyalty, and patriotism—the love of our neighbors, the love of just authority, and the love of our country's good. In proportion as these virtues flourish, the weeds of idleness, viciousness, and crime perish. Out of sound knowledge will arise in time civilization and peace. At present it is folly and self-conceit in nations to claim to be civilized, otherwise than as contrasted with savage barbarity. The admiration of physical prowess, the honoring of tinsel and pomp, the glorification of martial renown, are yet far too deeply inrooted in the spirit of the most cultivated nations to permit of the noble epithet "civilized," being appended to their names. The nobility of industry in all its grades,—first soul work, the labor of genius—then head-work, the labor of talent,—then hand-work, the honest labor of the body striving in the cause of peace—must be honored by state and people, before either can with truthfulness claim to be civilized. We are at best as yet but enlightened barbarians. Think how all Europe and half Asia have stood for months, and are even now standing, on the verge of foul and barbarous war; how Christian nations have girded on their armor, and, with mutual distrust and well-grounded suspicion, have stood with hand on sword-hilt ready to guard or to strike; think of what is worse, of the crime and ignorance that fester in the byways of Christian cities, and then boast of civilization if you can. The arts, the sciences, taste, literature, skill, and industry seem to have thriven among us in spite of ourselves—to have come among mankind like good spirits, and by main force to have established themselves on earth. They struggle with us and conquer us for our welfare, but are not yet our rulers. Sent from Heaven, aided by the few, not by the many, they have made firm their footing. If the monarchs and presidents of the states of the earth knew wherein the best interest of themselves and their people lay, it is in these intellectual invaders they would confide. The cost of armaments and the keep of criminals would cease in time unproductively to drain their treasuries. But ambition and strife are sturdy demons yet, and the educator, who dreams of their enchainment, and anticipates the speedy approach of a peaceful millenium, has but a limited acquaintance with the condition of mankind, and the hearts of its governors.

I cannot help hoping that the time will come when every British town even of moderate size will be able to boast of possessing

public institutions for the education and instruction of its adults as well as its youthful and childish population,—when it shall have a well-organized museum, wherein collections of natural bodies shall be displayed, not with regard to show or curiosity, but according to their illustration of the analogies and affinities of organized and unorganized objects, so that the visitor may at a glance learn something of the laws of nature,—wherein the products of the surrounding district, animate and inanimate, shall be scientifically marshalled and their industrial applications carefully and suggestively illustrated,—wherein the memorials of the history of the neighboring province and the races that have peopled it shall be reverently assembled and learnedly yet popularly explained; when each town shall have a library the property of the public and freely open to the well-conducted reader of every class; when its public walks and parks (too many as yet existing only in prospect) shall be made instructors in botany and agriculture; when it shall have a gallery of its own, possibly not boasting of the most famous pictures or statutes, but nevertheless showing good examples of sound art, examples of the history and purpose of design, and, above all, the best specimens to be procured of works of genius by its own natives who have deservedly risen to fame. When that good time comes, true-hearted citizens will decorate their streets and squares with statues and memorials of the wise and worthy men and women who have adorned their province, not merely of kings, statesmen, or warriors, but of philosophers, poets, men of science, physicians, philanthropists and great workmen. How often in travelling through our beautiful country do we not feel ashamed of its towns and cities, when we seek for their ornaments and the records of their true glories and find none? How ugly is the comparison that forces itself upon our minds between the conduct of our countrymen in this respect and that of the citizens of continental towns? A traveller need not go far through the streets of most foreign cities without seeing statues or trophies of honor, serving at once as decorations and as grateful records of the illustrious men they have produced,—reminding the old of a glorious past, and inciting by example the young to add to the fame of their native soil.

My picture may seem a dream; but I have faith sufficient in England and Englishmen to believe that in the course of time it will come to pass. Had the foresight of the present crossed the imagination of an ancient Briton, he might have hoped for its realization in another world, scarcely in this. But a simple belief in the probability of State and people advancing in intellectual aims and true civilization, and working them out through the length and breadth of the land, is essentially too wholesome and compatible with the progress of Christianized human nature, not to find an embodiment in a coming reality.

Art. XXXVII.—*On the Cause of the Aurora Borealis;* by Prof. A. de la Rive.*

When in June 1836, I published in the Bibliothèque Universelle a note on the origin of hail and atmospheric electricity, I already foresaw that the same cause would explain the aurora borealis, and the irregular and diurnal variations of the magnetic needle. As I had not then seen an aurora, I withheld at that time this application of the principles. Since then I have witnessed two fine auroras, and the appearances observed, especially during that of November 17, 1848, have confirmed my view of the nature of the phenomena, while they also accord with the observations of others, especially with those of Hansteen, Bravais and Lottin, and also with the many interesting details in Humboldt's Cosmos. My subsequent electrical experiments throw additional light on the origin of the aurora.

This last statement indicates that I regard the cause as electrical. This view has often been presented before, and was brought forward by Arago at the time of Œrsted's discovery. Yet no one, to my knowledge, has explained the mode of action and production of the electricity, or the attendant phenomena resulting from this cause.

Without going into any historical details, I will briefly describe the Aurora Borealis itself and its effects, and then pass to my own theory, the accordance of which with facts I shall endeavor to point out.

1. *Description of the Aurora and its accompanying effects.*

I cite the following details principally from the Cosmos. They are derived mostly from the descriptions of Hansteen, Bravais, Lottin, and other travellers, who have been in favorable places for observing the aurora. The learned author of Cosmos has grouped the facts with great skill, presenting in an admirable manner the prominent points, and seems with scientific tact to reach towards the true theory of the phenomena which he describes.

An aurora borealis is always preceded by the formation in the horizon of a kind of nebulous veil, which rises slowly to a height of four to six or eight, or even ten degrees about the magnetic meridian; the sky though before pure, becomes darkened, and over this obscure segment, whose color varies from brown to violet, the stars are seen as through a thick haze. An arc of light, first white, and afterwards yellow, borders the dark segment. Sometimes this luminous arc is agitated for hours by a

* Mem. Soc. de Phys. et Hist. Nat. de Genève, xiii, and Bib. Univ., xxiv, 337, Dec. 1853.

sort of effervescence, and a constant change of form, before it rises into the rays or columns of light which mount to the zenith. The more intense the emission of the polar light, the brighter are the colors that appear, which from violet and bluish white pass by intermediate shades to green and purplish red—just as electric sparks are colored only when the tension is strong, and the explosion violent. Sometimes the columns of light proceeding from the luminous arc are mixed with blackish or smoky columns; sometimes they rise simultaneously from different points of the horizon; or they may unite in a sea of flames of indescribable magnificence, the form and brilliancy of which are in incessant change. The motion gives greater visibility to the phenomenon. Around the spot in the heavens towards which the dipping needle points, the rays appear to cluster and form a corona. Rarely the aurora continues till the corona is on all sides complete, and when this happens, it announces that the end of the exhibition is near at hand. The rays then become feebler, shorter, and less bright in their colors. Soon, only large nebulous motionless spots, of a pale or ashy tint, are seen over the celestial vault; and finally, traces of the dark segment in the northern horizon, where the appearances began, alone remain.

The connection between the polar light and a certain kind of cloud is recognized by all observers, who affirm, that *the polar light sends forth its brightest columns when the upper regions of the air contain masses of cirro-stratus clouds of great tenuity, which tend to form a corona around the light.* Sometimes the clouds are grouped and arranged like the auroral columns; and in this case they appear to disturb the magnetic needle. After a brilliant aurora, the trains of clouds in the morning have sometimes been found to indicate the positions of as many luminous columns during the night.

The absolute height of the aurora has been variously estimated. For a long time it was supposed that it might be ascertained by the observations of distant observers on the corona: but it is now well known that the corona is only an effect of perspective, due to the apparent convergence of rays which are parallel to the dipping needle; so that each sees his own corona, as each his own rainbow. Moreover the aspect of the phenomenon depends on the position of the observer. The seat of the aurora is in the upper regions of the atmosphere; but sometimes it appears to be produced within less elevated regions, where clouds are formed. Such observations as those of Capt. Franklin appear to establish the latter conclusion, who saw an aurora which lighted up the under surface of the clouds, whilst Mr. Kendall, two to three miles distant, saw no light whatever, although awake and constantly observing the sky. Captain Parry also as-

serts his seeing an aurora depicted on the flank of a mountain: and it is said that a luminous arc has been seen on the surface even of the sea, around the magnetic pole.

Mairan and Dalton believed the aurora borealis to be cosmical, and not atmospheric. But Biot, who had an opportunity of observing the aurora at the Shetland Isles in 1817, proved it to be an atmospheric phenomenon, from finding that it did not partake of the movement of the stars from west to east, and consequently moved with the earth's rotation. Since then, nearly all observers have come to the same conclusion; and in particular MM. Lottin and Bravais, who have observed more than 143 auroras, and given detailed descriptions of them.

It is therefore quite certain that the aurora is not extra-atmospheric. To the evidence from its appearances, we may also add the crackling noise sometimes affirmed to be heard by the inhabitants in the far north, and the sulphurous odor which also has been observed. And, in fine, if the phenomenon is wholly beyond our planet, why should it be located about the polar regions? M. de Tessan, in the voyage of the Venus around the world, saw a fine aurora australis, which he describes with care. It was 14° in height, and the centre of the arc was in the magnetic meridian. He heard no sounds connected with it, which he attributes to its distance: but he mentions that M. Verdier, a French naval officer, on the night of Oct. 13th, 1819, while on the coast of New Holland, heard distinctly a kind of crepitation, during a brilliant aurora. All the details mentioned by M. de Tessan prove the exactness of the observations.

As concomitant effects of the aurora, we have mentioned the crackling sound, and the sulphurous odor. M. Matteucci has also observed during the appearance of a late aurora, satisfactory evidence of positive electricity in the air. But of all the phenomena, those which are of most invariable occurrence are the magnetic. The magnetic needle undergoes perturbations, either to the west or east, and usually the latter. These disturbances vary in intensity, but never fail of taking place; and they are at times manifested in places where no aurora is seen. This coincidence of magnetic disturbance with the aurora, shown by Arago to be without exception, from many years of observation, enabled this philosopher to tell, while in the basement of the Paris Observatory, when there was an aurora in our hemisphere. M. Matteucci has observed this magnetic influence under a new form. During the aurora of Nov. 17, 1848, the armatures of soft iron used with the Electric Telegraph between Florence and Pisa remained attached to their electro-magnets as if strongly magnetised, although the apparatus was not in action, and the batteries out of use.

M. de Tessan cites an observation made in 1818, by M. Baral, another French naval officer, on the same coasts of New Holland, who found that he had been making a wrong course from following his needle; there had been no storm, and the compass had not been touched. But on the evening of the same day, there was a brilliant aurora, and to this he attributes the deviation—a conclusion which could not have been dictated by theory, since at the time (in 1818) the relations between electricity and magnetism were not known.

The intimate connection between the aurora and terrestrial magnetism, has led Humboldt to designate as a *magnetic storm* a succession of disturbances of equilibrium in the magnetic forces of the earth. The presence of this storm is indicated by the oscillations of the magnetic needle, and afterwards, by the aurora, of which the oscillations are precursors, and which also put an end to the storm, just as the lightning in an ordinary electric storm announces that the equilibrium, before disturbed, is again established in the normal distribution of the electricity. Humboldt finds proof, amounting to experimental certainty, in the discovery of Faraday, who produced light by the action of magnetic forces alone, that the earth, by virtue of its magnetism, has the property of emitting light quite distinct from that which is afforded by the sun.

While recognising the truth of the analogy which Humboldt here traces out, we should recollect, that it is not of itself, but because it produces electric currents, that magnetism gives out light; the light is purely electrical in origin. Magnetism produces luminous phenomena only because it can disengage electricity, and it is probably in this point of view that Humboldt says in a general way, that it is a source of light.

It is hence in electricity, and in the influence which this agent in a state of motion, and magnetism, mutually exert, that we must look for the cause of the aurora borealis. This is the view which I would sustain, and to the force of my demonstration, I propose to bring some direct experiments, as well as the results of numerous observations through past years.

2. *Proposed Theory.*

The atmosphere in its normal state is constantly charged with a considerable quantity of positive electricity, which increases as we ascend, starting at the earth's surface where it is zero.

I will not inquire into the origin of this electricity: what is certain is that its production is connected with the action of the sun, since its intensity is subject to diurnal variations. It may be a question whether the sun acts directly, either through its light or its heat, on the constituents of our atmosphere, and so produces the electricity; or whether it is an indirect effect of the

solar rays causing evaporation from the waters of the seas, or the vegetation of the land. It is probable that both causes act: yet I am inclined to regard the first as most general and most constant. But this is of little importance here: the fact of the constant charge of positive electricity in the atmosphere, and of negative electricity in the earth, is abundantly proved, and this is sufficient for our explanations.

This constant production of the two electricities must necessarily be attended by a recomposition or neutralisation; otherwise the contrary electric states would acquire an infinite tension, which is contrary to observation. This recomposition or neutralisation takes place in two ways, one irregular and accidental, the other normal and constant.

The first method is exhibited under various forms. Generally it is the simple humidity of the air, or the fall of rain or snow, which causes the neutralisation. At other times, it is the thunder-bolt, which exhibits in an energetic manner the tendency to union in the two accumulated electricities, one in the air, the other in the ground. The winds in certain cases, by mixing the air from the earth's surface which is negative like the earth, with the positive air of a region more elevated, leads to a neutralisation of the two electricities, causing either storms or an exhibition of heat lightning. In winter, the air being constantly more saturated with moisture, the direct neutralisation is effected through the aqueous vapors and there are therefore fewer great disturbances and consequently fewer storms; and at the same time, as Arago has remarked, considering the number of storms, the lightning strikes the earth more frequently in winter than in summer.

In general, the influence of the hygrometric state of the air on the manifestations of atmospheric electricity is almost as great as that of the cause itself which produces this electricity; for this influence makes itself felt both in the production of the accidental phenomena just enumerated, and in the indications of the electrometer by which we ascertain the normal electric state of the air for the hours of the day, and days of the year. Hence it is difficult to deduce from these observations even the intensity of the atmospheric electricity for any given moment, seeing that it is impossible to separate this original intensity from the degree more or less decided which the electric registers may manifest.

Let us now pass to the second mode of neutralisation of the two electricities, which I regard as normal and regular.

The positive electricity, with which the upper beds of the atmosphere are charged, will traverse them freely, because of their high state of rarefaction. But in the polar region, where the intense cold constantly condenses the aqueous vapors, it finds a

portion of the atmosphere saturated with humidity, giving rise to mists; and by this means it may easily pass to the earth and combine with the negative electricity with which the earth itself is charged. It consequently results that there are constant currents of positive electricity rising from different points of the earth's surface into the upper regions of the atmosphere, which pass towards the poles, and then return beneath the earth's surface towards each of the points whence they have started. The currents of the northern hemisphere should go to the north pole, and those of the southern, to the south pole. In the equatorial regions, the position of the sun will determine the dividing line between the two systems. We may add that the experiments made with the electric telegraph have demonstrated that the terrestrial globe is an almost perfect conductor of electricity, compensating by its mass, for what it wants in the conductibility of the materials which constitute it. Thus the existence of the currents, whose course I have traced, rests on well established principles, with a foundation of simple experiment.

But more than this: their existence is demonstrated by facts long studied and established,—those pertaining to the diurnal variation of the magnetic needle.

I do not examine here into the origin of the earth's magnetism, a subject to which I shall have occasion to return in another work; for the present, I only say that I do not regard the disturbing causes of the direction of the magnetic needle as of the same nature with those which determine this direction. I content myself now with regarding the earth as a large magnet having its two poles; and I study only the causes that modify the direction which, in this quality of a magnet, it tends to impress on the magnetic needle. These causes are the electric currents, whose existence I have just shown; they well explain the diurnal variations. These variations, in fact, consist in this, that in our hemisphere, the north pole of the needle moves to the west, during the morning until $1\frac{1}{2}^{h}$ P. M., and then returns to the east during the rest of the day, to remain stationary during the night. But this deviation is precisely that which should be occasioned by currents passing along the surface of the globe from the north pole to the equator, augmenting in intensity with the heat of the day and diminishing as it decreases. The diurnal variation is at its maximum (13′ to 16′) in those months in which the sun is longest above the horizon, May, June, July, August. It is at its minimum (4′ to 5′) during the winter months. The variation is greater as we pass from the equator towards the pole; but it is evident, that if the currents, proceeding from different points of the earth's surface heated by the sun, rise in the atmosphere to redescend at the polar regions, and thus traversing the globe, reach their point of departure, the nearer the needle to the mag-

netic pole, the greater the number of currents that will act upon it: near the equator, it will not be subject to any influence from the currents which are formed beyond the region around the needle. In winter these differences are less sensible, because the currents from the equatorial regions are the only ones whose effects will be very decided, on account of the little difference of temperature which exists in this season between the earth's surface and the upper regions of the atmosphere in the temperate and especially the polar zone.

Finally, according to our theory, the same effects should be manifested in the southern hemisphere, only that all is reversed; and this is fully established by the various results of recent observers, including those of Colonel Sabine and a large number of travellers.

I should however acknowledge that there are some anomalies, either in the hours or in the direction of diurnal variation, at certain places, especially at St. Helena and the Cape of Good Hope, anomalies which it is difficult to explain by the theory proposed. But I am convinced that when further examined, they will be found to be due to local and accidental causes, such as the vicinity of the sea, which influences very notably the diurnal variations of temperature, and especially their amplitude and the hours of the maximum and minimum of heat. The question whether there are not places of no variation, proposed by Arago, is of little importance in this connection. The points of the earth's surface without diurnal variation, will be those where the two currents originate, and whence they proceed from the right and left towards the two poles: they are situated in the equatorial regions, but they cannot well be laid down, as their position will vary with the sun, the temperature, the winds, and other disturbing causes.

But I do not dwell on this point, as my object is not to treat of the diurnal motions of the needle. My end is simply to prove from the diurnal variations, the existence of the terrestrial currents. In continuation, we may obtain another proof still more direct, although less general, of the presence of these currents, by making use of the telegraph wires for collecting them. This I have done in England, as has also Mr. Barlow; and M. Baumgartner has performed similar experiments in Germany. In these trials, the currents have in all cases been detected by means of the galvanometer. M. Baumgartner, having introduced a very sensitive galvanometer into the circuit formed by the telegraph wire between Vienna and Prague, which has a length of about 61 miles, obtained the following results when the two extremities of the wire were buried in the earth.

1. The magnetic needle never stood at zero, but was more or less deviated.

2. The deviations were of two kinds, some of large extent, even 50°, others small, varying from 1° to 8°;—the former not common, and changing in direction and intensity, so that no law can be discovered; the latter on the contrary subject to a simple law, and being very regular when the air is dry and the sky serene, but with many anomalies when the weather is cold and rainy.

Mr. Barlow has made numerous observations, and obtained results demonstrating the exactness of the principle which I have laid down. Four main lines starting from Derby, were used in his experiments, two running towards the north and northeast, and two towards the south and southwest. The direction of the currents perceived on the first two lines, was always contrary to that of the currents on the two others, as ought to be the case, on the theory proposed. But the most remarkable fact, is the perfect concordance which these observations have proved to exist between the movement of the needle of the galvanometer placed in the circuit of the telegraph wire and the diurnal variations of the magnetic needle. The diurnal movement of the needle of the galvanometer is subject to disturbances in intensity more or less continued, during storms, and also when the aurora borealis is visible; and so also is this true of the compass needle. There is this difference, that the currents acting on the latter, circulating beneath the earth's surface, should not be subject to disturbances like those which happen to the telegraph wires through the influence of the electrical condition of the atmosphere about them.

The existence then of electric currents circulating beneath the earth's surface appears to us to be well demonstrated; and once proved, it leads necessarily to the conclusion that it is a consequence of the normal and regular reëstablishment of the electric equilibrium between the earth and its atmosphere, which is broken essentially in tropical regions; whilst the electric discharges, more or less intense, which take place between the earth and the air are the accidental and variable means for the reëstablishment of this equilibrium. We may now see how the explanation of the phenomena of the aurora both north and south, flows necessarily from the formation of these electric currents circulating from the equator to the two poles in the upper regions of the atmosphere, and from the two poles to the equator along or beneath the surface of the globe.

As we have said above, the positive electricity with which the atmosphere is charged, especially in the upper regions, is carried towards the two poles either by the greater conductibility of the upper and most rarified strata of the atmosphere, or by the currents of air in the upper regions which move from the equator to the two poles. It is consequently through the vapors which are

constantly condensed in the forming mists in the polar regions that the positive electricity should find its passage into the earth, and also therefore its discharge. This discharge when possessing a certain degree of intensity should be luminous, especially if, as is almost always the case near the poles and sometimes in the upper regions of the atmosphere, it encounters in its course icy particles of extreme minuteness, which form the haze as well as the more elevated clouds.

The formation of lunar halos which generally precede the appearance of an aurora, and the fall of rain or snow which also is often a prelude to it, are a proof of the presence in the atmosphere of these fine needles of ice, and of the part they play in the phenomenon before us.

This attenuated mist, rendered luminous by the transmission of electricity, ought to appear under a regular form, like an illuminated surface of greater or less extent, and more or less broken. It should spread outward from the poles, forming as a first appearance the auroral bank like a veil in the north. The tenuity of this veil is such that the stars may be seen through it, as has been remarked by all observers. MM. Bixio and Barral, in the balloon ascension which they recently made, suddenly found themselves,—although the sky was quite serene and the atmosphere without a cloud—in the midst of a veil or mist, which was perfectly transparent, consisting of a multitude of small icy needles so fine that they were hardly visible. Such are the needles which become luminous by the passage of the electricity, which determine the formation of halos as has been rigorously demonstrated, and produce by condensation the aqueous vapors in their passage through the air towards the earth, the fall of snow or rain, or sometimes under peculiar circumstances, hail.

Now if we inquire what should pass in the portion of the luminous mist nearest to the earth's surface, we shall conclude that the vicinity of the magnetic pole must exert a decided influence on this electrised matter,—for it is in fact a true mobile conductor traversed by an electric current.

In order to obtain a correct idea of this action, I have endeavored to imitate artificially the process of nature, and with this view, I contrived the following experiment.

Into a glass globe, 30 to 40 centimeters in diameter, I introduced through one of its two opposite tubulures, a piece of soft iron wire, about 2 centimeters in diameter, making it to terminate at the inner end very near the centre of the globe, while the other end was exposed out of the globe. The wire was covered through its whole length, excepting its extremities, by a very thick insulating bed formed first of shell-lac, then with a glass tube covered itself with shell-lac, then with a second tube of glass, and finally with a bed of carefully applied wax. The

insulating layer in all was a centimeter thick, giving 4 centimeters for the thickness of the bar thus covered. Within the globe, a ring of copper surrounded the bar and its insulating bed, at the part most distant from the tubulure. This ring was arranged to be put in communication with a source of electricity exterior to that of the bar by means of a metallic wire insulated with care, which passed through the tubulure and ended without in a hook. A stopcock attached to the other tubulure of the globe, was arranged for obtaining a vacuum. When the air within is sufficiently rarified, the hook is connected with the conductor of an electric machine, and the outer extremity of the bar of iron with the soil; by this means the electricity forms within the globe a luminous sheaf, more or less irregular, which passes from the ring, and terminates at the inner extremity of the soft iron. But immediately on placing the outer extremity of the soft iron on the pole of an electro-magnet, the electric light takes a wholly different aspect. Instead of proceeding indifferently from different points of the upper surface of the cylinder of iron, it proceeds from all points in the circumference of this surface, so as to form around it a continuous luminous ring. This is not all: this ring has a movement of rotation around the magnetized cylinder, sometimes in one direction and sometimes in the other, according to the direction of the electric current, and the nature of the magnetisation. Finally, jets of brilliant light are seen to proceed from this luminous circumference, which are distinct from the rest of the mass of light. When the magnetization ceases, the luminous phenomena return to the condition familiar in the experiment, known under the name of the *Electric Egg*.

There is some advantage in using for the experiment here described Armstrong's hydro-electric machine, in which the boiler is made to communicate with the hook which is united by a metallic connection to the ring of copper within the globe, whilst the conductor which receives the vapor is put in connection with the bar of soft iron. Thus we have in the globe an electric current of great intensity which may be changed in direction, by inverting the connections.

3. *Agreement of the theory with the facts.*

We have remarked that all observers agree now in regarding the aurora as an atmospheric phenomenon, and we have cited facts in support of this view. One more fact may be alluded to here which places it beyond doubt; it is from the observations on the aurora borealis published in the History of the Voyage of Captain Franklin. Lieutenant Hood and Dr. Richardson were 55 miles miles apart for the purpose of making simultaneous observations, in order to ascertain the parallax of the phenomenon and consequently its height. The results from three trials place

it alike at a height of 6 to 7 miles. On the 2nd of April, at the most northerly station a brilliant arc was seen 10° above the horizon; at the other station, it was not visible. The 6th of August the aurora was at the zenith at one station, and 9° in height at the other. On the 7th of April it was again in the zenith at the first station, and 9° to 11° in height at the second.

Again, Hansteen, and after him, MM. Lottin and Bravais, were led to believe as a consequence of their observations, that the arc of the aurora is a luminous ring whose different parts are sensibly equidistant from the earth, and which is centered around the magnetic pole so as to cut at a right angle all the magnetic meridians which converge towards this pole. Such a ring is the auroral arch and its *apparent* summit is necessarily in the magnetic meridian of the place. M. Bravais also observes that the arc seems to have a kind of movement of rotation from the west to the east passing by the south. From this description the phenomenon is quite similar to the result of the experiment described above, and the direction of the rotation in the luminous ring is precisely that which ought to take place according to the laws governing the mutual action of currents, if it be the positive electricity which passes from the atmosphere to the surface of the earth, thence to penetrate about the north magnetic pole, reunite with the negative electricity, and thus constitute the current.

The diameter of the luminous ring will be greater, as the magnetic pole is more distant from the earth's surface, since this pole ought to be found in the intersection of the plane of the ring with the axis of the terrestrial globe.

It hence results that each observer sees the summit of the auroral arc in his own magnetic meridian; and hence only those on the same magnetic meridian see the same summit, and can take simultaneous observations for ascertaining the height.

If the summit of the arc pass the zenith of the observer, he is surrounded on all sides by the matter of the aurora, or the auroral influences which proceed from the earth, and then, if at all, the crackling sound which has been alluded to should be heard. If it does not reach the zenith, the observer is then outside of the region; and the aurora is more or less distant according to its altitude. The noise may be produced by the action of a powerful magnetic pole on luminous electric jets very near this pole, as I have proved by experiment; I have succeeded in producing a similar sound by bringing a piece of iron, strongly magnetised, to the luminous arch formed between the poles of a voltaic battery.

As to the sulphurous odor, it proceeds like that which accompanies lightning, from the conversion of the oxygen of the air into ozone by electric discharges.

The light of the aurora is not polarized, as was remarked by Biot in 1817, from his observations at the Shetland Islands. This

negative result is confirmed by Mr. Macquorn Rankine, who has shown that this absence of polarisation is not due to the feebleness of the light, since this same light viewed after reflection from water is found to be polarised by this reflection. The most careful study and experiment have found no trace of polarisation in electric light, whether the discharges be made in the air or in a vacuum. This is a new proof of the identity of these two kinds of phenomena.

Finally, we discover in the resemblance between auroral appearances and certain clouds, as well as the disturbances of the magnetic needle, a further important confirmation of our theory.

The observations of Dr. Richardson already mentioned, which show that the aurora exists at moderate elevations, also indicate that it is often connected with the formation of different kinds of *cirro-stratus* clouds. Lieutenant Hood, in speaking of the luminous bands or columns of the aurora, says that he is convinced that they are carried by the wind, because they retain exactly their relative situation, which is not the case when the luminous matter moves in the air by its own direct action. Finally, the coëxistence of the aurora with small ice needles in the atmosphere, such as exist in elevated clouds, is shown by Captain Richardson, who having seen at a temperature near $-32°$ C. ($-35°$ F.) an aurora whose superior arc was near the zenith, remarked that although the sky appeared perfectly serene during the phenomenon, there fell a fine snow hardly perceptible to the eye, though easily observed as it fell on the hand and melted. The same fact had been previously observed in full sunshine, the rays of the sun rendering the floating particles of ice visible.

Observers are agreed with regard to the existence of a stratum or dark segment, which rests in the northern horizon, and appears to be the source of the auroral display. The numerous observations of M. Struve at Dorpat, and those of M. Argelander at Abo confirm this appearance. It is like a veil, which although permitting the light to pass gives the sky a more somber aspect; moreover it is bordered by a luminous arc. The existence of such a dark segment is confirmed by an observation of Gisler, who says that in Sweden, upon the high mountains, the traveller is sometimes suddenly enveloped in a very transparent mist of a grayish white color, verging towards green, which rises from the soil, and is changed into the aurora borealis.

The cirro-cumulus and the mists become luminous when they are traversed by electric discharges sufficiently energetic, provided daylight does not efface the feeble light. They may sometimes be detected in the day: thus Arago establishes most incontestably that Dr. H. Usher was not deceived in a notice published in volume II. of the Memoirs of the Irish Academy, where he describes an aurora seen at mid-day on the 24th of May, 1788.

This observer, during the day after a night in which he had witnessed a brilliant aurora, having observed an oscillation of the stars as seen with his lens, perceived in the sky rays of a white quivering light which rose from all points in the horizon towards the pole of the dipping needle, where they formed a light and whitish corona like that which the most brilliant aurora presents at night. Arago, on consulting old records at the observatory, found that there were considerable magnetic disturbances that day in the magnetic needle kept for showing the diurnal variation, thus proving beyond question that the phenomenon observed by Dr. Usher was a veritable day aurora.

I find also in the account of the voyage of the Venus by M. de Tessan, that M. Cornulier, an intelligent officer in the French Navy, often observed on the coast of New Holland a particular direction in the cirrus clouds during the day, from which he was enabled always to announce a fine aurora australis at night. M. Cornulier, like M. Verdier, was convinced, from a study of the arrangement of the cirrus clouds, that in those regions, auroras occur during nearly every day, and that the variation is only as to their brightness; they are often hid from view by clouds and storms. This remark agrees with the observations made under the direction of Captain Lefroy in Canada, at 13 different stations, and with others, collected by the Smithsonian Institution. It results from all these observations, that the aurora was seen on almost all clear nights, when the moon was not too bright, although not at all the stations. This is especially true during the months when the nights are longest. From October to March, there is scarcely a night without a visible aurora; and they are most brilliant in the month of February. The tables show that auroras were seen during 261 nights in 1850, and 207 in 1851. It is also remarkable and natural, that the auroras should have been seen most frequently in the stations nearest the magnetic pole.

Recurring to the coëxistence of icy particles in the air with the auroras, we find striking proof on this point in the Canada observations. The tables give with exactness the weather before and after the auroras. The aurora was almost always preceded by a fall of rain or snow; it also often happened that a fall of one or the other succeeded the aurora. The appearance of lunar halos, a common prelude to auroras, is a proof of the presence in the atmosphere of these icy particles which make up the network illuminated by the electric current.

But the most important proof of the electrical origin of the aurora is that derived from its action on the magnetic needle. The observations by Arago at the observatory of Paris,* by Fors-

* Ann. de Ch. et de Phys., x. 120; xxx, 423; xxxvi, 398; xxxix, 369; xlii, 351; xlv, 403.

conclusions:—

1. During the day preceding the night on which an aurora appears, the declination of the magnetic needle to the west is always augmented 10, 20 or 30 minutes, or more.

2. On the contrary, at the middle, and at the end of the exhibition, the needle deviates from its normal state to the east.

3. Finally, the needle often undergoes irregular perturbations during an aurora, amounting to several minutes.

It happens ordinarily that the maximum deviation of the needle during the day preceding the night of the aurora, is at noon, or half an hour after noon; and the deviation due to the disturbance may be 5 to 30 minutes or more, beyond that of the days before or following. Sometimes the maximum western deviation is at other hours in the morning, and it is probable that in such cases there is an aurora during the day. Arago cites several cases of this kind. Thus, on the 17th of August, 1828, the declination from 8½h. A. M. till noon was 5′ above the mean of the month for the same hours; and on the same day, at 10h. P. M., Messrs. Coldstream and Foggo perceived feeble traces of an aurora which was probably the end of a day aurora. During the evening the needle was in its ordinary position.

The magnetic observations made in the regions near the pole confirm the influence on the needle. Thus at Reykinwik (64° 8′ 15″ N.) MM. Lottin and Bravais, having made numerous observations on the diurnal variation of the needle parallel with similar observations at Paris and Cherbourg, were struck with the almost continual disturbance of the needle. They at first attributed it to some movement in the earth: but afterwards, remarking the concordance of their observations with those of M. de Löwenörn made in 1786, 50 years before, they satisfied themselves that the effect was due to auroras invisible to them because of the continued presence of the sun above the horizon. M. Ginge, a Danish Missionary, made observations in 1786, 1787, continued through the 24 hours, which showed that the western declination was ordinarily strongest from 9 to 10 in the evening, and least at 9 to 10 in the morning, a fact which he attributed without hesitation to the aurora. This conclusion is confirmed by the very numerous and excellent observations of MM. Lottin and Bravais.

We thus see, that for a long period observations near the pole have shown that auroras must be more frequent than was supposed, and this is confirmed by the facts observed in Canada and the United States.

We therefore conclude, that the production of auroras, northern and southern, is the normal mode of neutralising the positive electricity of the atmosphere with the negative of the earth.

This neutralisation should not take place in a man
or regular. It is evident that the variations in tl ducting capabilities of the atmosphere will be att tions in the facility of this neutralisation.

These differences will be evinced by the devia ances of the magnetic needle, which will be s distances from the poles, as in the temperate zone often observed. The western deviation which in itudes usually precedes an aurora, indicates a larg of electricity, due to a powerful condensation (polar regions, which by facilitating the reunion (tricities, augments the intensity of the terrestrial in our hemisphere from the equator to the north, a ly carries the needle more to the west. When th visible, the current becomes less strong, because of the aurora is proof of the resistance (probably gelation of the particles of water suspended in t stitutes the mist) which the reunion of the two counters;* the needle will then retrograde to the takes place.

In the higher latitudes, the disturbances of the tinual, because the slightest differences in the electric discharges that take place in the polar re there perceived. As to the observations of MM nörn and Lottin, that the maximum deviation of place from 8 to 10 o'clock in the evening, and t 9 to 10 in the morning, they were made only dur in summer, and they prove only that at this sea the greatest amount of condensation of moistur should be the case, at times just preceding and fo ting of the sun, and the least 7 or 8 hours afte the observations of Lieutenant Hood, made in Captain Franklin, between the 1st of February May, the greatest declination took place at 8 and morning, and the least at an hour after noon. ' the times of the maxima and minima are widely high latitudes, where there are great differences the day, and also in temperature, and therefore con disturbances of the air.

It is a singular fact, sometimes noticed, that wh in the midst of an aurora, so to speak, the action o be null. This was remarked by Mr. Forster, at yond 65° N., the latitude of Forts Franklin and E Dr. Richardson had on the contrary observed t needle. In fact, a needle in the interior of the

* It is clear that the mist when first formed should be a when, afterwards, it consists only of icy particles.

the aurora about the magnetic pole, is no longer under the influence of the currents which circulate around it and not above or below, and it ought therefore to experience only a variable and irregular action.

I have said that the aurora was probably of daily occurrence, and varied only in intensity. These differences in intensity are the reason for its being not always perceptible, and also for its less frequency remote from the magnetic poles. As to the differences of number for each month, they are attributable to two causes—but especially to the unequal length of the nights, for there should be fewer in the shorter nights. Thus in May, June and July the fewest are seen, because the days are the longest, while in the nine others, and especially in March, September and October, they are most numerous. This preëminence of these three months above others of still shorter days, can be due only to this, that the auroras are most frequent at the times of the equinoxes, and especially the autumnal equinox. This is readily understood if we consider that the vernal equinox is the time when the sun transfers to the northern hemisphere its powerful influence either direct or indirect in the development of electricity; and that the autumnal should be followed with a large condensation of the vapors accumulated in the atmosphere during the months of summer—a condensation which, as already explained, facilitates the neutralisation of the two electricities, developed in large quantities during the summer, and augments consequently the intensity of the discharge at the pole.

It has been pretended that in the appearances of the aurora borealis there are secular variations; in other words, that there are epochs comprising a certain number of years during which auroras are particularly frequent, and others in which they are rare. This opinion does not appear to me to be based on documents sufficiently exact to be admitted. There may be a difference in different years, as there is a difference in temperature and humidity. But this is far from making out a periodicity in auroras: to establish such a periodicity, there ought to be the collected observations of a century, from observers at least as good, and as favorably situated with reference to the magnetic poles, as those now engaged: and this we have not. We need not therefore dwell longer on this point, only remarking that if really such a periodicity exists, it might be connected with the change in the magnetic poles, which are the centers of the aurora, and which according to the surface about them would more or less facilitate the electric circulation: for it is evident that the naked soil would afford more ready circulation than a surface covered with a great thickness of ice. But, I repeat it, the fact of the periodicity is far from proved.

Recapitulation.—1. All observations agree in demonstrating that the aurora borealis is a phenomenon taking place in our atmosphere, and that it consists in the production of a luminous ring whose center is the magnetic pole, and having a diameter more or less large.

2. Experiment demonstrates that in causing in highly rarified air the reunion of the two electricities near the pole of an artificial magnet, a small ring of light is produced similar to that which constitutes the aurora, and having a like movement of rotation.

3. The aurora is consequently due to electric discharges taking place in the upper regions between the positive electricity of the atmosphere and the negative electricity of the earth—the electricities being separated by the direct or indirect action of the sun, principally in the equatorial regions.

4. As these electric discharges take place constantly, though with varying intensity, depending on the state of the atmosphere, the aurora should be a daily phenomenon, more or less intense, and consequently visible at greater or less distances, and only when the night is clear—which accords precisely with observation.

5. The phenomena that attend the aurora, such as the presence and form of the cirro-stratus clouds, and especially the disturbances of the magnetic needle, are of a kind to demonstrate the truth of the electric origin attributed by the author to the aurora—an hypothesis with which these phenomena correspond even in their minutest details.

6. The aurora australis, according to the few observations on it which have been made, presents exactly the same phenomena as the aurora borealis, and is explained in the same manner.

Art. XXXVIII.—*Notice of three ponderous masses of Meteoric Iron at Tuczon, Sonora;* by Charles Upham Shepard, M.D.

The first intimation concerning the locality here noticed, was afforded in 1851, at the Meeting of the American Association for the Advancement of Science, on the occasion of my describing before the Chemical and Natural History section of that body, the meteoric stone of Deal, New Jersey. Dr. J. L. Le Conte being present, and having just returned from California through the province of Sonora, stated to the meeting, that "while passing through the village of Tuczon, a frontier town of Sonora, near the Gila, in the month of February previous, he observed two large pieces of meteoric iron, which were used by the blacksmiths of the town for the purposes of anvils. He was unable to procure any specimens from these bodies; but was guided to a cañon

between two mountain ridges in the immediate vicinity from which both pieces had been taken, where the masses of meteorites were so abundant as to have given name to the cañon. He had not before heard any account of this remarkable circumstance, and had considered it an interesting subject for observation."*

Nothing farther was brought to light respecting this very remarkable locality, until the present season, on the return of Lieut. John G. Parke, of the United States Topographical Engineers, from his scientific explorations in Sonora, when he had the goodness to address me a letter of inquiry respecting them, attended with about an ounce weight for my examination, which he had procured in person from one of the masses.

Lieut. Parke observed in his letter that "the Alcaide and Commandante would not consent to our removing the masses, even had we possessed the means; but by dint of two hours hard labor, we managed to chip off a few fragments, which I hope may serve the purposes of analysis."

I immediately set myself to the best examination of the subject, which the limited supply of materials and the facilities at hand, permitted.

The fragments were small; the largest piece not weighing above one-quarter of an ounce, and that somewhat battered by the process employed for its separation. Still, it showed the natural outside of the meteor. It was destitute of a well marked crust, and much coated with oxyd of iron, evincing in common with the other fragments, that this iron is prone to undergo a rapid oxydation on exposure to the weather.

The fresh surfaces presented the color and lustre of white cast-iron; though it is not brittle, or granular in its fracture. A close examination of a fresh surfaçe, produced by the cold chisel, reveals frequent white spots, of the size of a pin's head and smaller, scattered in every direction, and without any very perceptible order. These spots seem to be owing to the presence of an earthy powder, which adheres closely to the iron, and indeed seems partially imbedded therein. When such a surface is highly polished on the burnishing wheel, the spots disappear; but are renewed again on the application of acids, in the etching process. They then come into view, rather more circumscribed in their areas than before; but of a very determinate figure, being mostly rounded or oval, sometimes with angular indentations in their borders. They are never rhomboidal or rectangular in their outline, after the manner of the much larger earthy grains, or crystals, in the Atacama iron, which render the latter porphyritic, when cut into slabs. The Tuczon iron on the contrary, when thus polished and etched, is amygdaloidal only; and to discern this character thoroughly, requires the aid of a microscope.

* Proceedings of the American Association for the Advancement of Science, Sixth Meeting, p. 188.

The acids act very tardily on the iron, and require to be aided by heat, before the action will fairly commence. No decided crystalline structure is developed in the process; though the fragments experimented on, being small, and considerably altered in molecular texture by the force applied in their separation from the parent mass, it would not be safe from this trial perhaps, to conclude against a crystalline structure in the main portion of the iron.

Sp. gr. = 6·66, which corresponds very nearly with that of the Atacama iron, as determined by Turner, whose trial specimen no doubt included the earthy constituent of that iron.

No sulphur was detected in those fragments that were acted upon by acids; but here again, it would not be strange if this very common element of meteorites should hereafter be detected, when a larger portion of the mass comes to be examined.

The most striking phenomenon that presented itself during my examination of the Tuczon iron, was the following. A white, insoluble powder came into view throughout the liquid, as the solution of the iron proceeded in nitro-hydrochloric acid; and at the conclusion of the process a considerable precipitate of this powder was obtained, among which were little ovoidal grains of a milk-white mineral and occasionally also, others of the same figure that were perfectly limpid, like hyalite; while others still were milky on one side and limpid on the other,—thus evincing that the milky and the limpid mineral, was one and the same species. Indeed I was led to regard the mealy powder also, as partaking of the same nature; and such was the general resemblance of the whole, to the mineral I have called chladnite, in the meteoric stone of Bishopsville, South Carolina, that I am led to refer this earthy substance to that species: an opinion which is the more probably correct, as I found the acid solution to contain decided traces of magnesia, an earth which it is most likely proceeded from the partial decomposition of portions of the pulverulent, earthy ingredient of the meteoric iron, and not from the iron-alloy. Should this view of the unknown substance prove correct, it will be the first instance in which it has been found in an iron-mass. We shall then have (astropetrologically considered) a second species of meteoric iron, with an earthy admixture; the first being the previously known *peridotic iron*, and the second, that here pointed out, viz., the *chladnitic iron.*

The nitro-hydrochloric solution of the iron, afforded with ammonia in excess, a deep blue liquid, indicative of nickel in a very decided proportion, to the iron.

It remains only to state a few additional particulars concerning these iron-masses, derived from a later letter of Lieut. Parke, which he kindly permits me to annex to this notice.

"The three masses were found in a cañada of the Santa Rita Mountain, abont 25 or 30 miles to the south of Tuczon. Two of them were shown to us by the Commandante; both being

used as anvils. One lies within the Presidio, and is of a very peculiar form, it being annular, and somewhat like a seal-ring of huge proportions. Its exterior diameter is about three and a half feet; its interior about two. It weighs nearly 1200 lbs. The other piece is in front of the Alcalde's house. It weighs about 1000 pounds, and has an elongated prismatic form, serving well the purposes of an anvil. It is partially buried in the soil, but having two feet of its length projecting above the ground. The third piece I did not see; but was told that it was much smaller than either of the others. By permission of the authorities, our blacksmith undertook to cut off some specimens, in which, however, he almost entirely failed—the metal being so tough and hard. It yields to the hammer, and has a clear ring, not unlike that of bell-metal. The surfaces were rounded, and rusted,—closely resembling a mass of refined cast-iron that had been exposed to the action of the weather for a long period. The surfaces that have received the blows of the hammer, where used as an anvil, are quite polished.

"To obtain these specimens would be attended with no little difficulty, owing to the remoteness of the locality, and the broken-down condition of animals when reaching this point."

The route of transportation recommended by Lieut. Parke, is that, via. Fort Yuma, distant 275 miles from the locality, on the California side; and from thence by water, to the head of the Gulf of California. Measures are already on foot for the removal of one or more of the masses, to this part of the country, which it is greatly to be desired will be crowned with success.*

Art. XXXIX.—*Reëxamination of American Minerals:* Part IV—*Boltonite; Iodid of Silver; Copiapite; Owenite; Xenotime; Lanthanite; Manganese Alum; Apophyllite; Schreibersite; Protosulphuret of Iron; Cuban;* by J. Lawrence Smith, M.D., Prof. Chem. Med. Depart. University of Louisville.†

37. *Boltonite, identical with Chrysolite.*

Boltonite was first described as a new species by Professor C. U. Shepard. He made the specific gravity from 2·8 to 2·9. It was subsequently examined by Professor Silliman, Jr., who found

* The above Sonora meteoric irons were described and illustrated with figures in a paper by Dr. J. Lawrence Smith, presented to the American Association at its meeting at Washington in April last—a paper which was to have appeared in our last number, but is still delayed. The masses were seen by officers of the late Boundary Commission, and figures are published in Bartlett's Personal Narrative (8vo, 1854), ii, 298.—Eds.

† The absence of Mr. G. J. Brush from America, who was associated with me in the first three parts of these reëxaminations, makes it necessary for me to continue them alone and the absence of his valuable assistance is a matter to be regretted by all who take any interest in the subject.—J. L. S.

3·008 as its specific gravity, with a hardness of from 5 to 6: his analysis gave for its constituents:*

Silica,	46·062
Alumina,	5·667
Magnesia,	38·149
Protoxyd of iron,	8·632
Lime,	1·516

With this knowledge of the mineral, I undertook its examination, on specimens in the gangue furnished me by Prof. Shepard. Examination of different portions separated mechanically from the gangue, made it very evident that the mineral was more or less mixed with other substances which had escaped observation, for no two analyses agreed; and it was soon discovered that it was impossible (from the specimens in my possession at least) to separate Boltonite in a state of purity without the aid of other means than had been adopted.

Boltonite, as is well known, occurs at Bolton, Mass., disseminated in irregular masses and grains in a white limestone. If a piece of the mineral in its gangue be placed in cold dilute hydrochloric acid, the limestone is readily dissolved, and a mass left, which is seen to consist of asbestus, dolomite, a little mica, small crystals of magnetic iron, and a greenish or yellowish green mineral; if the acid be now heated, the dolomite will be entirely dissolved with a little of the last mentioned mineral.

In order to obtain the Boltonite as pure as possible for analysis, the following method was adopted. Pieces were separated by the hammer as thoroughly as possible from all other substances; these were subsequently placed in dilute hydrochloric acid, and boiled for some time; the acid being washed away and the substance dried, it was crushed in a mortar to fragments from the twentieth to the tenth of an inch in diameter; these were again introduced into dilute acid and heated for a short while; the acid was thoroughly washed away, and the mineral dried. The small fragments (now like coarse gravel) were placed on a piece of glazed paper, the hand laid flat upon it and the mineral rubbed so as to grind the particles against each other for the purpose of ridding their surfaces of a little cohering silica arising from its partial decomposition; with a small gauze sieve the finer particles are separated, and from that remaining in the sieve we are enabled with the aid of a glass without any difficulty to pick out the pure Boltonite. This method requires a little patience, but no extraordinary care, and however unpromising the original specimens may have been, there is no difficulty in obtaining a material, the results of whose analysis is constant. From a larger selection of specimens than that used, there doubtless could be obtained pieces perfectly pure of some size. After being satisfied with this method of obtaining the pure mineral, three different portions were pre-

* This Journal, vol. viii, 2d ser., p. 391.

pared and examined, the first two being of the greenish variety, and the third of the yellow variety, which color is doubtless due to a peroxydation of a minute quantity of the protoxyd of iron entering into the constitution of the mineral. Mr. L. Saemann in a communication made to the American Association some time since, attributed this change to magnetic iron undergoing decomposition; but this, however, does not appear to me to be the case, for the reasons that crystallized magnetic iron is a mineral difficult of decomposition, and the color is not in fissures as would be the case if the peroxyd arose from a substance foreign to the composition of the mineral, but enters into its most intimate structure.

The hardness of Boltonite is found to be, as already stated, between 5 and 6. The specific gravity was taken on three specimens; Nos. 1 and 2, on a gramme each of fine particles; No. 3 on a piece of ·150 gramme, all possible precautions being used to arrive at correct results:

No. 1, 3·270 No. 2, 3·208 No. 3, 3·328

No. 3 is to be regarded as by far the most reliable, as in taking the sp. gr. of fine grains it is almost impossible to detach the last particles of air, and consequently the sp. gr. they indicate is below the true number.

The analyses of three portions gave—

	No. 1.	No. 2.	No. 3.
Silica,	42·56	41·95	42·41
Magnesia,	51·77	51·64	50·06
Protox. iron,	2·35	3·20	3·59 (with Alumina)
Alumina,	0·10	0·25	
Loss by heat,	2·22	1·58	not estimated.
	99·00	98·62	

Nos. 1 and 2 were the greenish variety, No. 3 the yellowish. The oxygen ratio of the silica and protoxyds are—

	No. 1.	No. 2.	No. 3.
Silica,	22·11	21·77	22·03
Magnesia,	20·35	20·30	19·67
Protoxyd of iron,	·52	·71	·75

This being as one to one within a small fraction, the formula therefore is $(\dot{M}g\,\dot{F}e)^3\,\dddot{S}i$, or of the general form $\dot{R}^3\,\dddot{S}i$, which of course proves it to be *chrysolite*, a fact sustained in every respect by its physical characters.

38. *Iodid of Silver.*

In this reëxamination of American minerals it was not originally designed to include those of South America: but my recent examination of the minerals obtained by Lieut. Gilliss of the U. S. Chili Expedition, has afforded an opportunity of analyzing certain minerals that it was well to investigate, and among these

were one or two fine specimens of iodid of silver. A reexamin-
ation of this mineral is especially interesting, from the fact that its composition is still in doubt, owing to the discrepancy between the original analysis of Vauquelin on the mineral from Zacatecas in Mexico, and that of Domeyko on the mineral from Chanarcillo in Chili.

	Vauquelin.		Domeyko.	
Iodine,	22·6	$Ag^2 I$	46·89	$Ag I$
Silver,	77·4		54·25	

The constitution of the native Chlorids and Bromids of Silver would lead to the supposition that Domeyko's analysis was the correct one, and this is strengthened by its resemblance to the artificial iodid of silver.

The specific gravity was found to be 5·366, being a little lower than that given by M. Domeyko. The analysis of an exceedingly pure specimen gave me—

	1.	2.
Iodine,	52·934	53·109
Silver,	46·521	46·380
Chlorine,	*trace*	*trace*
Copper,	*trace*	*trace*
	99·455	99·489

clearly showing its constitution to be

$$Ag\,I = \text{Iodine } 53{\cdot}85,\ \text{Silver } 46{\cdot}15 = 100,$$

leaving no doubt of its perfect analogy to the natural chlorid and bromid of silver. The other properties of this mineral are not mentioned, as they are all fully stated in all works on mineralogy.

39. *Copiapite.*

This mineral was also furnished me by Lieut. Gilliss, it having been brought from Chili. It consists of most beautiful silky fibres or fibrous masses of a pearly lustre. Its color is white with a very slight tinge of yellow. From the specimens in my possession there was no difficulty in picking out a portion in a state of great purity. Its specific gravity is 1·84. Examined under the microscope its form appears to be a hexagonal prism. Cold water has but little action on it, merely causing the crystals to separate and the mass to swell out to a very much increased bulk. If the water be boiled, decomposition ensues with a deposition of the oxyd of iron and the formation of a soluble sulphate. On analysis it afforded

	1.	2.
Sulphuric acid,	30·25	30·42
Peroxyd of iron,	31·75	30·98
Water,	38·20	not estimated.
Undissolved,	0·54	
	100·74	

The analyses correspond to the formula

$$\overline{\mathrm{Fe}}\,\dddot{\mathrm{S}}^{2} + 11\dot{\mathrm{H}}.$$

This is the same formula as that obtained by Rose, with an additional half atom of water, his formula being

$$2\overline{\mathrm{F}}\mathrm{e}\,\dddot{\overline{\mathrm{S}}}^{2}+21\dot{\overline{\mathrm{H}}}.$$

Protoxyd of iron was looked for but none found.

40. *Owenite,* identical with Thuringite—with an announcement of a new locality.*

Owenite was first described by Dr. F. A. Genth as a distinct species, who gave a minute and accurate analysis in the Am. Journ. of Science, vol. xvi, 2d series, p. 167. It was found on both sides of the Potomac river near Harper's Ferry. The physical characters being already fully and accurately given, it is needless to repeat them here, merely remarking that its specific gravity as taken by me is 3·191. It is readily soluble in hydrochloric acid; notwithstanding, analysis No. 2 was made by fusion with carbonate of soda. Results of analyses as follows:

	1.	2.	Genth.
Silica,	23·58	23·52	23·21
Peroxyd of iron,	14·33		13·89
Alumina,	16·85	16·08	15·59
Protoxyd of iron,	33·20	32·18	34·58
Protoxyd of manganese,	0·09		*trace*
Magnesia,	1·52	1·68	1·26
Lime,	——		0·36
Soda.	0·46		0·41
Potash,	*trace.*		0·08
Water,	10·45	10·48	10·59
	100·50		99·97

After this examination it was rendered strongly probable that Owenite and Thuringite were similar if not identical minerals; yet, in the analysis of Thuringite by Rammelsberg, alumina is not mentioned as one of its constituents. This view was sustained by the apparently perfect accordance in the physical characters of the two minerals, coupled with the fact that the amount of silica and water in the two, as already examined, was the same, and also the sum of the oxyds of iron and alumina in the Owenite were equal to the sum of the oxyds of iron in the Thuringite. To settle the question, it became necessary to reëxamine Thuringite, of which I obtained a specimen from Mr. Markoe, coming from the original locality; it was slightly altered by the action of the air, but this could interfere only with the correct estimate of the protoxyd of iron. Its specific gravity was 3·186, and its composition,

* The identity of these two minerals has already been announced by me in a letter to one of the editors of this Journal (Am. Jour., xvii, 131), but no details were then given.

Silica,	22·05
Peroxyd of iron,	17·66
Alumina,	16·40
Protoxyd of iron,	30·78
Magnesia,	0·89
Soda, Potash,	0·14
Water,	11·44
	99·36

The peroxyd of iron is a little higher, and the protoxyd a little lower than in the analysis of Owenite, but this arose from the partial decomposition of the specimen. The correct analysis of Thuringite is that first given, and the formula deduced by Mr. Genth from it is to be looked upon as the correct one, namely—

$$2\dot{R}^3\ddot{S}i + \ddot{\overline{R}}^3\ddot{S}i + 6\dot{H},$$

corresponding to the oxygen ratio for

$$\dot{R}, \ddot{\overline{R}}, \ddot{S}i, \dot{H}, \quad 1 : 1·5 : 1·5 : 1.$$

In looking over some minerals placed in my hands by Mr. Markoe, I have found a specimen of Thuringite coming from the Hot Springs of Arkansas. Its identity is made out without the slightest difficulty, as all its physical characters correspond most perfectly with the Thuringite, its sp. gr. being 3·184 and composition—

Silica,	23·70
Peroxyd of iron,	12·13
Alumina,	16·54
Protoxyd of iron,	33·14
Magnesia,	1·85
Manganese,	1·16
Soda, Potash,	0·32
Water,	10·90
	99·74

An interesting fact connected with this mineral as shown by this investigation is, that although not crystalline, or at least very obscurely so, yet coming as it does from three localities so widely separated as Thuringia, the Potomac, and Arkansas, it is nevertheless found quite unmixed with any other mineral, as the analyses indicate.

41. *Xenotime of Georgia.*

In examining a few years ago some of the residue of gold washings from Clarksville, Georgia, in the possession of Prof. Gibbes of Charleston, I observed some small octahedral crystals associated with zircon, titaniferous iron and kyanite. Two or three of the most perfect were selected, and having no goniometer at hand, they were sent to Mr. Teschemacher, who referred them, after a partial examination, to Zircon. Prof. Gibbes subsequently examined their form, and pronounced them Xenotime, (Am. Jour. Science, 2nd Ser., xiii, 143). Since then, from ma-

terial that had been placed in my hands by that gentleman, nearly a gramme of the substance has been procured, and upon that the following examination has been made.

Some of the crystals are exceedingly short prisms surmounted by four-sided pyramids, but most of them are without the prism, the summits coming together forming a flattened octahedron. The measurements made were: over the pyramidal edge 123° 10′, over basal 81° 30′, face of pyramid on prism 131° 40′. The above measurements can be made with perfect accuracy; not so the faces of the prisms on each other, and as far as I could make it out, I am inclined to think that they are not square prisms, but rhombic prisms of 93°. Its hardness is 4·5, sp. gr. 4·54, and the physical characters those given for Xenotime.

It was decomposed by fusion with carbonate of soda and silica, and analysed with the following results:

Phosphoric acid,	32·45
Yttria,	54·13
Oxyd of Cerium, (with a little $\dot{L}a$ and $\dot{D}$)	11·03
Oxyd of iron,	2·06
Silica,	0·89
	100·56

This analysis will be seen to differ from that of the Xenotime of Hitteroe, Sweden, by Berzelius, in that a portion of the yttria is replaced by the oxyd of cerium; the formula represented by the analysis is, however, the same, namely,

$$(\dot{Y}, \dot{C}a)^3 \ddot{\bar{P}}$$

Great care was taken in the separation of the oxyd of cerium, which after being peroxydized by heat, yielded but little to dilute nitric acid, indicative of the presence of but a small quantity of the oxyds of lanthanum and didymium.

42. *Lanthanite.*

This mineral was first observed in America by Mr. W. P. Blake, and described in this Journal, Sept. 1853; it was obtained by Mr. Blake from Bethlehem, Lehigh Co., Pa., where only one specimen had been found. It was handed to me for examination, and ascertained to be carbonate of lanthanum; the analysis made was given in the original description of the mineral. Since then I have made another analysis on a portion remaining in my possession, and although not differing from the former one, it is thought proper to insert it in this paper.

	1.	2.
Water,	24·21	24·09
Carbonic acid,	21·95	22·58
Protoxyd of Lanthanum (with some oxyd of Didymium),	55·03	54·90
	101·19	101·57

No. 2 is the analysis already published in the paper before mentioned. In both instances, there was an excess, owing to the peroxydation of a portion of the lanthanum,—a circumstance that cannot be avoided, nor do we know how to allow for it in our calculation. This mineral has the same formula as the artificial* carbonate, namely, $\dot{La}\ddot{C}+3\dot{H}$ = Carbonic acid 21·11, oxyd of lanthanum 52·94, water 25·95.

The only other known locality of this mineral is Bastnäs in Sweden; it is there found only as a coating to Cerite, and doubtless was not obtained in a perfectly pure state by Hisinger, who gave as its formula, $\dot{La}^3\ddot{C}+3\dot{H}$. I have no doubt as to the minerals being identical, and that whenever the Bastnäs variety is obtained crystallized, it will prove to have the same composition as the Bethlehem variety.

43. *Mangano-magnesian Alum from Utah.*

This alum was observed a few years ago by Dr. Gale, among specimens brought from the Salt Lake, in Utah, by Mr. Stansbury. It occurs at a place called Alum Point, and was considered altogether a manganese alum, of which Dr. Gale gave what he then stated he considered an imperfect analysis (Am. Journal Science, vol. xv, 2nd ser., 434):

$\dddot{S}$ 18·0 $\dot{Mn}$ 8·9 $\ddot{\bar{Al}}$ 4·0 $\dot{H}$ 78·0

Being desirous of having it more carefully analyzed, Dr. Gale placed in my hands the specimen which is the subject of the present investigation. It was not received as it occurs at the locality, but had been recrystallized and consisted of delicate needle-shaped crystals, adhering in small masses. It dissolves very readily in water; in fact so soluble is it that it is difficult to decide the amount of water requisite for its complete solution. It crystallizes from solution in the form of delicate crystals, with a plumose aggregation. On analysis it furnished:

Alumina,	10·40	10·65
Magnesia,	5·94	5·65
Manganese,	2·12	2·41
Sulphuric acid,	35·85	35·92
Oxyd of iron,	0·15	0·09
Potash,	0·20	0·20
Water,	46·00	46·75
	100·66	101·67

This analysis shows an amount of protoxyds a little too high for the requisites of the formula of alum, but this however, is of frequent occurrence in the natural alums, owing to admixture of impurities. This variety of alum has been before observed by Stromeyer, and was brought from a cave in Southern Africa. Its formula is,

$$(\dot{Mg},\dot{Mn})\dddot{S}+\ddot{\bar{Al}}\dddot{S}^3+24\dot{H}.$$

44. *Apophyllite.*

The specimen of this mineral examined, came from Lake Superior. It is eminently lamellar in its structure, and was placed in my hands as being possibly diaspore; its lustre is however much more pearly than this latter mineral. Its sp. grav. is 2·37, and its constitution,

Silica,	52·08
Lime,	25·30
Potash,	4·93
Fluor,	0·96
Water,	15·92
	99·19

45. *Schreibersite* (*of Patera*).

This meteoric mineral occurs in the American meteorites in more abundance than has usually been supposed, as was fully shown in a communication made to the Am. Assoc. for the Advancement of Science in April, 1854; and as that memoir will be published in full in this Journal, nothing farther than the mere statement of the analysis of this mineral is here given. G. = 7·017.

	1.	2.	3.
Iron,	57·22	56·04	56·53
Nickel,	25·82	26·43	28·02
Cobalt,	0·32	0·41	0·28
Copper,	*trace*, not estimated.		
Phosphorus,	13·92		14·86
Silica,	1·62	not estimated.	
Alumina,	1·63	not estimated.	
Lime,	*trace* not estima'd	not estimated.	
Chlorine,	0·13	not estimated.	
	100·66		99·69

Nos. 1 and 2 were separated mechanically from the meteoric iron; No. 3 chemically. The silica, alumina and lime, were almost entirely absent from No. 3; and in the other specimen it is due to a siliceous mineral that I have found attached in small particles to the Schreibersite, and of which I have preserved one or two small specimens.

The formula of Schreibersite I consider to be $Ni^2 Fe^4 P$.

		Pr. ct.
Phosphorus,	1 atom,	15·47
Nickel,	2 "	29·17
Iron,	4 "	55·36

Further particulars of this mineral will be found in the paper already referred to.

46. *Protosulphuret of Iron.*

This sulphuret is the one found in the meteoric irons of this country. The specimen examined came from Tennessee; its sp. gr. is 4·75. Its composition is different from that of magnetic

pyrites, although some authors consider the magnetic pyrites a protosulphuret, an inference not sustained by analysis. The mineral in question afforded me

Iron,	62·38
Sulphur,	35·67
Nickel,	0·32
Copper,	*trace*
Silica,	0 56
Lime,	0·08
	98·91

The formula Fe S requires Sulphur 36·36, Iron 63·64 = 100.

Further remarks on this mineral will be found in the paper on meteorites.

47. *Cuban.*

This variety of copper pyrites was first noticed by Breithaupt, as occurring among the copper ores of Cuba. Desiring to reëxamine it, specimens were obtained from Prof. Booth; they were massive and not perfectly pure, furnishing an insoluble residue consisting of silica and oxyd of iron which are very probably combined. Its sp. grav. was 4·180, and its composition—

	1.	2.	3.
Iron,	37·10		
Copper,	18·23	19·10	19·00
Sulphur,	39·57	39·20	39·30
Residue (silica and oxyd of iron),	4·23		
	99·13		

This seems to substantiate the formula already received, (agreeing with the analyses of Prof. Booth,) $Cu S + Fe^2 S^3$, pyrites being $Cu^2 S + Fe^2 S^3$ = Sulphur 42·28, Copper 20·82, Iron 36·90.

Art. XL.—*Correspondence of M. Jerome Nicklès, dated Paris, June* 28, 1854.

Academy of Sciences.—For some days, the Academy has been occupied repairing the loss experienced at the beginning of the year. In place of Dr. Roux, Dr. Claude Bernard has been named, well known for his discoveries in Physiology: in place of Admiral Roussin, an officer of the navy, M. Bravais, an admiral in science, although but a lieutenant in his official capacity. The labors of M. Bravais have not been confined to navigation and hydrography. He has published important mathematical works; his researches on halos, auroras, mirage, the rainbow, parhelia, and meteorology in general, have given him a prominent name in Physics, while his works on the arrangement of leaves on branches, the symmetry of inflorescence, the laws of growth of the Pinus sylvestris, have made him known to naturalists.

On the Phenomenon called "Spirit-rappings."—The time not occupied by the discussion of the titles of candidates, has been filled with communications, some of them of interest. The question also of "table-turnings," and "spirit-rappers," has engaged the attention of a physiologist of Francfort, Dr. Schiff, who has given in a full session, a demonstration showing that the noise of the "spirit-rappers" is not the result of a stroke of any part of the body on an external object; but that it is produced by means of the great peroneus muscle, the tendon of which passes behind the external malleolus, to which it is usually retained by a ligament. When this ligament fails, or when it is much relaxed, if the muscle is suddenly shortened, the tension of the tendon becomes so great that it slips suddenly from the malleolus, producing a noise similar to that of a stretched cord suddenly loosened.* M. Schiff by practice has become able to make these sounds at will; and he has given proof of it before the Academy: seated in a semi-circle, and in profound silence, they heard him produce the described sound for more than a minute. Restored from their surprise, the Academy hastened to change the subject, as if ashamed to occupy itself with the scientific explanation of a fact which for some time has occupied the popular imagination.

Electricity—*Electro-chemical action.*—Works on Electricity have specially occupied the attention of physicists in France. M. Becquerel, Sr., the inventor of the constant battery, and who was the first to form minerals by means of electricity, notwithstanding his advanced age, works with the activity of his youth. One of the ideas which constantly urges him onward in new efforts at progress, is the conviction that we have yet disengaged only a feeble part of the electricity which is associated with the molecules of matter, because of the recomposition which takes place through the contact of bodies. It is precisely a cause of this kind which interferes with the constancy of the batteries employed for chemical purposes. Plates of platinum used as electrodes become covered with gas, and take polarity; and consequently they give origin to contrary currents which necessarily diminish the effect of the pile. To avoid such disturbing causes, M. Becquerel has contrived two pieces of apparatus for constantly depolarizing the electrodes, making them at every moment to change their polarity. The author has exhibited his apparatus in action before the Academy, and has since applied it to the study of the principle which governs the disengagement of electricity in chemical action, a principle which he first brought out in 1823, and which since has been adopted in the science.

* Dr. Austin Flint of Buffalo, at the commencement of the "spirit-rapper" delusion, exposed the source of this noise in the same way precisely as Dr. Schiff.—Eds.

His depolarising apparatus gives him a more constant current than had been obtained, and serves as a means of verifying the results he before arrived at, which are for the most part confirmed. The following are his conclusions:

1. In the action of acids on metals, or on saline solutions, the acids or acid solutions take always an excess of positive electricity; the metals and alkaline solutions, a corresponding excess of negative electricity.

2. The disengagement of electricity in combustion is governed by the same principle; the combustible body disengages negative electricity, the supporter of combustion, positive.

3. Decompositions produce inverse electric effects.

4. There is no disengagement of electricity as long as the two bodies in hand are conductors of electricity: thus in the combination of a metal with oxygen, iodine, or dry bromine, electricity is not produced.

5. In the mixture of acids with water, or in their combination with it, water acts as a base; whilst it acts as an acid, as regards alkaline solutions.

6. Concentrated solutions of a neutral salt act with reference to water, as regards electrical effects produced, in the same manner as acids with reference to bases.

7. Acids in their combination or their mixture with other acids act in such a way that the acids the most oxydising are the most electro-positive; in their combinations with bases, the acids appear to retain the same property, so that in the reaction, in the case of the mixture of two solutions saturated with a neutral salt, the nitrate is positive with reference to a sulphate, the sulphate with reference to a phosphate, &c.

8. When several acid solutions, neutral or alkaline, are placed alongside of one another, so as to mix slowly, the electric effects produced are the resultant of the individual effects which take place at each surface of contact.

9. Contrary to the opinion of Volta, we may make an electric chain, or rather a closed circuit solely with liquids, in which an electric current will circulate, and by which phenomena of decomposition and recomposition may be obtained, if there are in the circuit bodies which are conductors of electricity. Living organised bodies present numerous examples of a circuit of this kind, giving place to electro-chemical effects, which have not yet been studied.

While occupied with the study of the principle governing the disengagement of electricity in chemical action, M. Becquerel has endeavored to bring the principle to a practical use. A long time since he applied it to the treatment of ores, and he has succeeded in extracting lead and silver from their respective ores without the intervention of heat, beyond what is necessary in a simple

roasting. An experiment has been made on more than 30,000 kilograms of ores from Mexico and different parts of the globe. The great solvent which he uses is common salt. We will give further details in our next communication.

Pyro-electric currents.—In the course of the preceding experiments, M. Becquerel observed that glass in fusion was a conductor of electricity. This conductibility begins to be sensible at 300° C. He has not been slow in finding a means of applying the heat lost in furnaces to the production of electricity. In view of this conductibility of glass in fusion, he substitutes it for acids and aqueous saline solutions in a battery. He obtains thus an element of a battery which remains constant, and which is sufficiently energetic to decompose water. Compared with a Bunsen's couple of the same size, it has about one-fourth the intensity.

These batteries, which in distinction from the hydro-electric and thermo-electric, M. Becquerel calls pyro-electric batteries, may be arranged in different ways. The following are two:

1. In a crucible placed in a furnace, pulverised glass is introduced, with 0·25 of carbonate of soda to hasten fusion; then a long bar of iron and another of copper are inserted in it, being fixed in a vertical position, and separated from one another. It is perceived that in this battery the soluble metal is the iron which is in fact oxydized; but this oxyd becomes dissolved in the mass in fusion, and the metal is constantly kept bright.

2. The barrel of a pistol is taken, and in it a tube of green glass is inserted enclosing a cylinder of copper; after having filled all the interstices of the barrel and of the tube with powdered glass, the whole is placed horizontally in a furnace, and the barrel and the copper cylinder are put in communication with apparatus for collecting the electricity.

These currents are not thermo-electric; for if the glass is removed, a galvanometer put in communication with the iron and copper rests at zero.

M. Becquerel considers it probable, in view of these pyro-electric currents, that terrestrial electric currents exist in contact with or near the junction of the solid part of the globe with the part in fusion, where there may be solid conducting substances partially empasted in the melted silicates in the manner of a pyro-electric couple.

On the electricity produced during the evaporation of salt-water.—For a long time it was admitted, on the researches of M. Pouillet, that the electricity produced during the evaporation of water containing a saline substance in solution, was due to the chemical segregation of the two substances, water and salt. Some years since, M. Riess and M. Reich showed that this electricity proceeded from the friction of the water against the sides

of the vase. This fact is proved anew by the researches just published of M. Gaugain, although the results obtained differ in the details from those of the German physicists.

Economical illumination by Electric light.—The last winter, the General Dock Company, hurried in the founding of its establishment, was obliged to work night and day. It undertook to remove in a short time the whole of a considerable hill: 1600 workmen, 800 at a time, were kept at work without interruption. In order to illuminate the works during the hours of the night, they proposed to use electric light. This mode of illumination has been often used in Paris in works at night; but in this case it was continued for 4 months, and proved to be an economical method of lighting. Fifty of Bunsen's elements were at once in action, and when the light after a while diminished, another 50 were substituted. Two electric lanterns served to light the space where the 800 workmen were employed. The expense per lantern was as follows:

Superintendent, per day,	4·50 francs.
Mercury,	5·00
Zinc,	4·50
Points of charcoal,	1·40
Nitric acid,	1·80
Sulphuric acid,	1·84
Total,	19·04

The cost, hence, of lighting the 800 workmen, was 38 francs 8 centimes per night, or 4½ centimes per man. This is a very considerable economy, and the work went on with a regularity which would have been impossible with any other mode of illumination; and besides it was accomplished without any danger, although the place was incessantly traversed by locomotives engaged in transporting the earth.

Decomposition of Kyanite by galvanic heat.—Another use has been made of electricity, and this of a chemical nature. On attaching to one of the charcoal points of a Bunsen's battery of 80 elements a small lamellar fragment of kyanite, which, as is well known, is very infusible, M. Duvivier has succeeded in fusing it in 3 or 4 minutes; the elements of which it consists were in part dispersed, and the aluminium, freed from oxygen, appeared at the surface of the substance in fusion. A small globule became fixed to the surface of the assay, which was flattened on cooling; and other globules remained imbedded in the melted mass. The author has extracted some of the supposed aluminium, but has not examined its physical properties, and we cannot say that it was pure; it may have contained silicium, proceeding from the silica. Has this deoxydation been produced by the heat

alone? Some physicists may think so. For ourselves, we believe that the reduction is due to the volatilised carbon; for it is well known that the luminous arc is never produced without a transfer of material, and the material transferred in this case is nothing else but incandescent carbon.

Electro-magnetic Machine.—On the Report of M. Becquerel, the Academy of Sciences has decreed to M. Marié-Davy, Professor in the Faculty of Sciences of Montpelier, a reward of 2000 francs for an electro-magnetic machine of his invention. In this machine, the electro-magnets act *by contact.* There is a cylindrical armature in communication with the axis which by its action it puts in motion, and which revolves in a circle carrying at intervals horse-shoe magnets. An analogous machine has been described and since patented in England by Mr. Talbot. We are ignorant of the force of Talbot's machine; that of Marié is very feeble, and we doubt if electricity will ever replace steam so long as the battery is not more economical. To increase the force of his motor wheel, the Academy has engaged M. Marié to replace the cylindrical armature by circular electro-magnets acting by opposite poles on the horse-shoe magnets. By this means, he will increase much the force without adding to the expense. M. Marié is now occupied with this improvement.

Magnetism by Rotation.—A German Journal relates the following experiment without mentioning the author. A watch-spring, not magnetic, suspended at centre by means of a fibre of silk, remains in equilibrium in any position, regardless of the earth's magnetism. But if a pistol containing a lead ball is fired directly beneath the spring, parallel to it, the spring becomes magnetic and takes the position of a magnetic needle. The author of the journal attributes this magnetism to the shock or undulation of the air produced by the passage of the ball.

We may give a simpler explanation, if we suppose the ball, which has a rotary movement in its passage, to become magnetic under this influence, as we have claimed in our remarks on the origin of the earth's magnetism.* Once magnetised, it induces magnetism in the steel spring, acting thus like an ordinary magnet. By interposing a screen between the spring and the line of the ball, it will still be magnetised if our explanation is correct, and also if the ball is fired above in a contrary direction the polarity should be the same, provided the ball has the same rotation in each case. Although this is only conjecture for the future to verify, I will take this opportunity to correct an opinion too formally expressed in the note on page 117 of this Journal, vol. xvii, written when discouraged after being disappointed in several trials; I hope soon to establish the contrary and without having recourse to the experiment alluded to above.

* This Journal, January, 1854, xvii, 116.

Experiments with reference to firing mines by electricity.—This subject which has received much attention, is to become of practical value through the efforts of Colonel Verdiè and Captain Savare of the Engineer Corps, who propose to substitute in place of Bunsen's battery for firing the powder, the machine of Ruhmkorff,† or that of Clarke. An interesting report on the subject made to the Academy by Marshal Vaillant, Minister of War and member of the Institute, announces the result as accomplished. But as the process for the purposes of war must be rendered familiar by practice to be of value, M. Vaillant does not consider that the time for using the process has yet come. He has ordered renewed trials, and to contribute toward it on his side, he has given the necessary orders that each School of Engineers shall have a Ruhmkorff's apparatus at its disposal. The processes employed by M. Verdiè and M. Savare differ somewhat, each in points of importance, but there is no space to describe them here.

Various Memoirs.—For want of space, we can only allude to the following papers:—An *Electric thermometer*, fitted for a boiler or an apartment kept at a constant temperature, by M. Maistre.—*Researches on the influence of Chloroform on the Sensitive Plant*, by M. Leclerc, showing that it is impressed by it perhaps like animals.—*Treatise on the relation which exists between the electro-motive force of the muscular current and that of different sources of dynamical electricity*, by Jules Regnauld.

In the science of Optics there have been several papers, among which we mention, *The determination of the emissive powers of bodies for light*, by MM. de la Provostaye and Desains; these experimenters have operated with incandescent bodies; platinum is more emissive than gold; and the emissive power of gold is 10 times more feeble than that of oxyd of copper.

Chemistry has as usual been richly represented. In the first place, M. Biot announces to the Academy the publication of the posthumous work of Laurent, entitled "Méthode de Chimie," and read on the occasion, the note with which he accompanies the work, and in which, under the form of *advice to the reader*, he points out the special end which Laurent proposed in his great work. A translation of this note is published in the latter part of this volume.—M. Rivot, superintendent of the Laboratory at the School of Mines, has brought forward *new methods of treating ores of copper.*—M. Frémy has communicated the results of his *researches on the ores of Platinum.*—The same chemist has presented two extended memoirs *on the composition of the eggs of different animals*, an investigation carried on in connection with M. Valenciennes, the zoologist.—M. Dessaignes is studying the *products of the transformation of creatine.*—M. C. Montrand has experimented *on the more economical manufacture of*

† This Journal, Jan., 1858.

phosphorus by treating phosphate of lime with carbon and chlorhydric acid.—Finally, the investigators of aluminium are giving themselves much labor, but still do not succeed in preparing this metal except at great expense.

Dilatation and Contraction of Metallic Plates.—The instruments for measuring dilatations of metallic plates are of great delicacy, giving results with very close precision. There are cases, however, in which a hundredth of a millimeter in difference of length may be of value, and this is the fact with the standard meter, the basis of the metric decimal system. M. Silbermann, Superintendent of the Conservatory of Arts and Trades, has just carried the precision to 3-thousandths of a millimeter. It is known that a rule suspended by one end becomes elongated thereby, and one standing on its end, owing to its weight, is shortened: and by placing the rule in a horizontal position again, it is supposed to take its original length. By employing his process, the germ of which is presented in a former work of this physicist,* M. Silbermann has shown that the rule that has been suspended retains its increased length when placed horizontally; and so with the rule that has stood on its end. The difference is only in thousandths of millimeters; still if it can be measured, this is sufficient reason why it should not be neglected.

New Greek Fire.—The war in the east has stimulated the zeal of those in Europe who are interested in improving the art of destruction. Projects the most remarkable and curious are proposed. Being persuaded that one of the means of preserving peace to humanity consists in perfecting our methods of destroying life, and not desiring that in this respect one nation should be more favored than others, we mention here some of the projects which rest on serious principles.

The Greek fire has at different times engaged attention without its being exactly known in what it consists. In 1755 a goldsmith of Paris, named Dupré, discovered an inflammable liquid which burned under water. Louis XV. allowed him to make experiments in the canal of Versailles, and then in different seaports, to try the power of the liquid in setting vessels on fire. It is said that the results produced were terrific. However the king believed it his duty to refuse the advantages which the invention promised. He withheld Dupré from publishing his discovery, and gave him a pension. Dupré died and carried off his secret.

In the month of April last, the photographer, Niepce de St. Victor, while studying benzine as an ingredient of a varnish, observed that this carburet,—which is very inflammable in the open air and at a low temperature by the simple contact of a small flame, while being insoluble in water and having a density

* This Journal, January and March, 1853.

of 0·85,—has eminently the property of burning on water. He then remarked that on throwing on water some benzine containing a fragment of potassium or of phosphuret of calcium, either of these substances set fire promptly to the benzine, by becoming inflamed through contact with the water.

In two experiments made each time with 300 grammes of benzine and half a gramme of potassium contained in glass vessels, the breaking of these vessels as they floated on the water, caused the benzine to spread over a large surface; the potassium taking fire produced an immense flame, which was very hot, and continued for about one minute, notwithstanding a strong wind in one case and a smart shower of rain in the second.

The first experiment was made on the 30th of April, on the Seine, and the second on May 2nd, in the basin of the Jardin du Palais Royal.

By request of the Minister of War, M. Niepce undertook to examine into the liquids susceptible of burning when used in the interior of hollow projectiles. In concert with M. Fontaine, a manufacturer of chemical products, he set himself to the work, and obtained the results here given. A mixture consisting of 3 parts of benzine and 1 of sulphuret of carbon, being put into a hand-grenade, previously heated to a temperature below that of boiling water, produced a disengagement of vapor which took fire on contact with a small flame; and a fine jet of flame was obtained much less smoky than that of pure benzine, and which continued to burn until the whole was consumed. For heating the hollow projectile, either a moment's immersion in boiling water, or contact with burning coals may be employed.

The mixture of benzine and sulphuret of carbon, of the proportions mentioned, floats on water, and its flame has remarkable burning qualities, when the sulphuret contains some phosphorus in solution; and it is proposed to use it in setting fire to wood. Oil of naphtha and oil of petroleum highly rectified are nearly as inflammable as benzine, and burn on water as readily. But their flames are not so hot. The oil of petroleum, benzine and sulphuret of carbon, as they are not expensive, it is proposed to use in war, either for burning an enemy's vessel, or for defending a place. We have read in the Journal "Cosmos," a programme prepared by a General of Engineers for defending a besieged town, and doing the greatest amount of mischief to the assailants.

Coupled Cannons.—This is another weapon of war, the effects of which may be terrible. It is brought forward by M. Ador. Two cannon have the same breech, and diverge at a given angle; they have a common charge of powder, a single touch-hole and a single cap. In each of these cannon, which are accurately bored and polished, a piston of a cylindrical form is fitted, having the same calibre as the cannon, carefully turned, polished

and greased. These two pistons are united together by an iron cord or wire when used with a musket, or by an iron chain from a meter to a hundred meters in length when with cannon. The pistons serve as projectiles; when fired, they straiten the chain between them, and flying through the air, they sweep every thing before them.

Photography—Heliographic engraving.—The following process invented by M. Baldus, appears to bring to perfection the method of engraving by the aid of the sun. The results obtained are very beautiful; and although the author has not described to us fully all the details, we know enough to give a general idea of his method.

On a plate of copper covered with petroleum a photographic proof on paper of the object to be engraved is placed; this proof is a positive, and will necessarily make a negative on the metal by the action of the light. After an exposure of a quarter of an hour to the sun, the image is reproduced on the resinous coating, but it is not yet visible; it is made to appear by washing the plate with a solvent which removes the parts not impressed by the light, and brings out a negative picture made by the resinous tracings of the bitumen. The designs are very delicate; the tracings receive solidity by an exposure during two days to the action of a diffuse light. When thus hardened, the plate of metal is plunged into a bath of sulphate of copper and is then connected with the pole of a battery; if with the negative pole, a layer of copper in relief is deposited on the parts of the metal not protected by the resinous coating; if with the positive pole, the metal is graved out in the same parts, and thus an etched engraving is obtained.

So that at will a raised or etched engraving may be made, the former to be printed like a wood-cut, the latter like ordinary copper plate engraving.

Collodion.—At one of the recent sessions of the Academy of Sciences, MM. Bisson brothers exhibited a photograph of the principal front of the Louvre; it was a positive on paper, 140 centimeters in length and 60 high, produced from a negative on collodionised glass. It consisted of 3 separate photographs, as similar in tone of coloring as if taken at a single operation. The operation was made with "collodion anticipé," the plates having been prepared in the workshop, and carried to the place after having been rendered sensitive; the authors affirm that these plates preserve their sensitiveness for several hours.

Société d'Encouragement pour l'Industrie Nationale.—We briefly allude to the recent annual session of this Society, held as usual for the distribution of medals to inventors who have become distinguished during the year by their inventions, and also to foremen who have been noted for their morality, intelligence,

and spirit of invention. Twenty-five among these latter have received bronze medals as well as books. All were distinguished for having made some improvements in the processes of their manufactures. Medals of bronze, of gold, or of platinum have been awarded to inventors, whose inventions have been successfully carried out. Some among these are already known to our readers; they are,—M. Dubrunfaut, for his economical production of alcohol from the juice of the beet;* MM. Girard and Aubert, for the impulse they have given to the caoutchouc industry;† M. Mirand,‡ for the successful application of the Electrical Telegraph to the wants or convenience of private life. We propose to describe another time his apparatus, which is already in use in several large houses.

We should also mention a dynamometer of excellent construction, for measuring the resistance to rupture of a band of tissues, an instrument which the public authorities, the manufacturers of tissues, and the marine propose to adopt, as it is admirably adapted for trying the strength of tissues, cordage, &c. The author of this dynamometer, M. Perreaux, received a platinum medal.

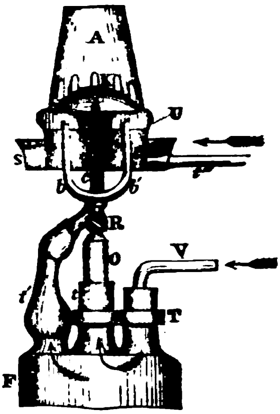
The session of the Academy was closed by a very fine address by the President, M. Dumas, on the past and future of Electricity. The poetic imagination of the orator more than once carried him too far. From reading the discourse one would seriously believe in the diamonds prepared by M. Despretz by means of the galvanic battery. The electro-magnetic machine of Mariè-Davy was represented as completed, and as realizing the force of a one-horse steam engine, at an expense of 2 francs per day: the public has believed it, and there will be a grievous disappointment for M. Mariè-Davy on the day when the machine he has proposed to construct, is put in action.

Economical Lamp for obtaining high temperatures.—M. H. St. Claire Deville, Professor of the Normal School, in order to carry on analysis by the dry way and the reduction of ores at this school, has been led to contrive an economical lamp capable of affording all the heat required. He burns a hydro-carburet purchased at little cost in the shops. Camphene would answer equally well.

In the lamp, the burning fluid used is brought to the state of vapor and inflamed before a blowpipe with a large aperture, the air of which is furnished by the bellows of an enameller's lamp. But a few seconds are required to raise a platinum crucible to the temperature of melting iron.

In the annexed figure, F is a reservoir furnished above with 3 tubulures, T, *t*, *t'*. By means of the bellows the air is injected into F through the tube V, which is inserted in T; *t* carries the

* This Journal, Sept. No., p. 274. † Ibid, p. 277. ‡ Ibid, Jan., 1853.



vertical tube O, which has a stopcock at R, and above divides into the two arms *b*, *b'*, which pass into a metallic box U, and terminate in its upper part in open extremities cut off obliquely. The box U contains the burning fluid *e*, partly filling it; it connects with a reservoir by *t''*, which is kept at a constant level. The centre of this box is a cylindrical tube, closed below, through which passes the blowpipe *e*, a continuation of the tube *t'*, the left tubulure (in the figure) of the flask F. The tube which is at the middle of the box U, and envelops the blowpipe *c*, has several small holes *u*, *u*, communicating with the empty (or upper) part of the box U.

Above the blowpipe, and resting in a furrow in the top of the box U, there is a copper cup K, pierced at the centre with a hole for the passage of the jet of vapor which escapes from the holes *u*, *u*, *u*, after the bellows are put in action.

To prevent the burning fluid from becoming too much heated, there is a trough S, containing water. Before lighting the lamp, the fluid in L is heated till the water in the trough boils; then the bellows are made to act, and the jet of vapor is lighted; after which the heat disengaged by the lamp is sufficient to continue the vaporisation of the fluid.

Above the box L, there is a chimney A, having a series of holes around, near its bottom, for drawing in air on the flame of the apparatus.

M. Deville observes that those hydro-carburets which give the densest vapors, and also have the lowest boiling point, afford the most heat.

Art. XLI.—*Observations on the Nomenclature of the metals contained in Columbite and Tantalite;* by Prof. A. Connell.*

In 1801 Mr. Hatchett announced the discovery of a new metallic substance, contained as an oxygen acid combined with oxyd of iron in an undescribed heavy black mineral from Connecticut. To this new metal Mr. Hatchett gave the name of *columbium*, and the ore in which he found it has usually in this country been called *columbite.* A year afterwards Ekeberg announced a new metal which he called *tantalum*, in two Swedish minerals, which he distinguished by the names of tantalite and yttrotantalite.

* Phil. Mag., June, 1854, p. 461.

succeeded in establishing that columbium and tantalum are identical; and this view was tacitly acquiesced in by the greater portion of the chemical public for many years, the metal and its ores usually obtaining in this country the names of columbium and columbite, and on the Continent the names of tantalum, and tantalite and yttrotantalite. A mineral was also discovered at Bodenmais, which was held to contain this same metal.

This state of things continued till about 1846, when M. H. Rose of Berlin published a series of researches on the ores from these different localities, from which, so far as I can understand the matter, he drew the following conclusions: first, that the metal in the Swedish tantalite is a distinct metal, with its peculiar oxygen acid and other combinations, and for this metal, the name of tantalum may be with great propriety reserved, being the metal discovered by Ekeberg, and by him called *tantalum;* secondly, that in the Bodenmais and American minerals two metals are contained, which M. Rose proposed to distinguish by the names of Niobium and Pelopium, the latter being supposed to be nearly allied to tantalum, but the former quite distinct in its characters.*

This view of Rose has more or less prevailed for the last eight years; although I confess it had always occurred to me, and occasionally I have spoken out the view, that Mr. Hatchett's memory had been rather hardly dealt with, since M. Rose had left him entirely out of view, although truly the first discoverer of the first known of these metals and minerals.

When cerium was ascertained not to be a pure metal, but to contain lanthanum and didymium mixed with it, no one thought of dropping entirely the name of cerium. It still belongs to an acknowledged metal, and the rights of its discoverers are unimpaired.

Precisely the same observation applies in regard to yttria and the new oxyds of erbium and terbium.

Other examples of the same kind might be quoted.

Now, on the authority of such precedents, when it was thought to be ascertained that the American columbite and the analogous Bodenmais mineral did not contain *one* new metal only, but at least two, justice seems to have required that the name of columbium should have been reserved for the more abundant of these two, just as the names of cerium and of yttrium have been preserved.

But how much more strongly does such a view hold good *now*, when it has been announced by M. Rose that the American and Bodenmais mineral contain only *one metal*, and for this metal he actually proposes the name of niobium?† Does it not follow very

* See Chemical Gazette, vol. iv, p. 349. † Ibid, vol. xii, p. 149.

clearly that this metal ought to have the name of *columbium?* M. Rose has now come to the same conclusion at which Mr. Hatchett arrived fifty years ago, when he announced that one new metal, to which he gave the name of columbium, existed in the American mineral columbite. If the countrymen of the latter most distinguished analytical chemist have any sense of justice or regard for the memory of an eminent man—one with whom I am proud to say I had a slight acquantanee, and from whom I received some kindness—they will now unite for the future in support of his just right not to be forgotten and entirely laid aside in this matter. There cannot be a better opportunity than the present for taking this step.

I am very far from wishing to overlook the important researches of M. Rose on this, as on so very many other interesting topics, and we shall always feel grateful for his further investigations regarding columbium and its various oxyds and other combinations. But we ought not to overlook what was done before him.

The matter is now reduced to a very simple issue.

We have *columbium* in the American and Bodenmais columbites, and probably now in some other minerals.

We have *tantalum* in Swedish tantalite and yttrotantalite, and probably in some other minerals.

Of all courses, one of the most ill-advised seems that followed by some English chemists, of giving occasionally the name of columbium to tantalum, which, as I understand the matter, is now quite ascertained to be different from any of the other metals. This course can only lead to confusion. Tantalum is not columbium.

Art. XLII.—*Murchison's Siluria.**

[This recent work by Sir R. I. Murchison is an able and instructive summary of the history of the earliest rocks that contain organic remains.

In 1839 Sir R. I. Murchison published his Silurian System in two parts, in a quarto of 768 pages copiously illustrated. This was followed in 1845 by another great work of 652 pages quarto, embellished with the most ample and beautiful views, sections, maps, &c., illustrating the geology of Russia in Europe and the Urals. As a companion, a second volume of 512 pages appeared at the same time, devoted to the Paleontology of the

* Siluria: The History of the oldest known Rocks containing Organic Remains with a brief sketch of the distribution of Gold over the earth. By Sir Roderick Impey Murchison, F.R.S., F.L.S., etc. etc. 523 pp. large 8vo, with 37 plates and many wood-cuts. *London*, 1854.

regions explored, illustrated by 50 quarto plates. These volumes on Russia and the Urals are the joint production of Sir R. I. Murchison, Mr. Edouard de Verneuil of France and Count Alexander von Keyserling of Russia.

The recent work of Sir R. I. Murchison is an able resumé of all that has been done by these writers, together with the results of other laborers in the science, among whom are Professor Sedgwick of Cambridge, England, the geologists of the Ordnance Survey in Great Britain, and many eminent men on the continent of Europe and in America. The observations on gold and its distribution, with which the volume closes, are of much practical value, although only incidentally connected with the main object of the work.

The important labors of the author of Siluria have been carried through with signal ability, and at a great outlay of time and money. Geology is largely indebted also to Prof. Sedgwick for his researches among the oldest fossiliferous rocks. But Murchison appears to have first made out the correct relations of these early strata. To him we owe the judicious exposition of the whole subject in the elaborate works which he has put forth. The candor and liberality manifested by the author are worthy of the works which they adorn. His investigations have extended above the coal, into the New Red Sandstone, the Permian beds, and the Triassic, and the present work embraces these limits. The full exhibition of these great zones of primeval life, from its earliest dawn through many successive ages, to the era of the first reptiles, is most interesting and instructive. A general review of the subject, partly historical and partly expository, is presented in the introductory chapter of the Siluria which we here cite. We take pleasure in thus showing our appreciation of the extended labors of the author.—B. S.]

The earliest condition of the earth is necessarily the darkest period of its geological history. The favorite hypothesis concerning the origin of the planet, founded on astronomical and physical analogies, is, that it assumed the form of a flattened spheroid from rotation on its axis when in a fluid state. Reasoning upon this idea, and looking to the structure of those rocks which either lie at great depths or have been extruded from beneath, the geologist has inferred that the crystalline masses, including granites which issued out from below all other rocks, and constitute possibly their existing substratum, were at one time in a molten state. The theory of a central heat, at first sufficiently intense to maintain the whole terrestrial mass in a state of fusion, but subsequently so far dissipated by radiation into space, as to allow the superficial portion to become solid, has been adopted by the greater number of philosophers who have grappled

with the difficult problem of the first conditions of our planet. Most of them likewise have believed that all the great outbursts of igneous matter, by which the crust has been penetrated and its surface diversified, were merely outward signs of the continued internal activity of that primordial heat, now much repressed by the accumulations of ages, and of which our present volcanoes are feeble indications. If, then, the mathematician has correctly explained the causes of the shape of the globe, the geologist confirms his views when, examining into the nature of its oldest massive crystalline rocks, he sees in them clear proofs of the effects of intense heat. This original crust of the earth was subsequently, we may believe, broken up by protruded masses, which issuing in a melted condition, constituted the axes and centres of mountain chains. Each great igneous eruption gave out substances that became, on cooling, solid rocks, which, when raised into the atmosphere, constituted lands that were exposed to innumerable wasting agencies; and thus afforded materials to be spread out as deposits upon the shores and bed of the ocean. In these hypothetical views concerning the production of the earliest sediments formed under water, we seem to reach a primary source; and once admitting that large superficial areas were originally occupied by igneous rocks, we have in them a basis from which the first sedimentary materials were obtained.

The earlier eruptions having necessarily occasioned elevations at some points and collapses or depressions at others, such changes of outline, aided by the grinding action of water, would occasion the formation of bands of sediment, which, adapting themselves to the inequalities of the surface, must have been of unequal dimensions in different parts of their range. In this way, we may imagine how, by a repetition of the processes of elevation and denudation, the earliest exterior rugosities of the earth would be in some places increased, while in others they would be placed beyond the influence of sedimentary accumulation. May we not also infer, that the numerous molten rocks of great dimensions which were suddenly evolved from the interior at subsequent periods, must have made enormous additions to the solid crust of the earth, and have constituted grand sources for the augmentation of new strata?

Turning from the igneous rocks to crystalline stratified deposits, we now know that a great portion of the micaceous schists, chloritic and quartzose rocks, clay-slates, and limestones, once called primary, were of later origin. Many of these are nothing more than subaqueous sediments of various epochs, which have been altered and crystallized at periods long subsequent to their accumulation. This inference has been deduced from positive observation. Rocks, for example, have been tracked from the districts where they are crystalline, to spots where the mechan-

latter to localities where the same strata are wholly unchanged, and contain organic remains. Transitions are thus seen from compact quartz rock, in which the grains of silica are scarcely discoverable with a powerful lens, to strata in which the sandy, gritty, and pebbly particles bespeak clearly that the whole range was originally accumulated under water. Other passages occur from crystalline, chloritic, and micaceous schists, to those clay-slates which are little more than consolidated mud, and from crystalline marble to common earthy limestone, in which organic remains abound. These and similar metamorphoses embrace the consideration of changes, like those, for example, by which ordinary limestone has been converted into dolomite and sulphate of lime or gypsum, or shale into mica-schist, as is seen in the secondary and tertiary rocks of the Alps.*

Elementary works will have, indeed, informed the student, that such changes of the original sediment have been generally accounted for by the influence of great heat proceeding from the interior of the earth, and which at different former periods manifested its power in the eruption of granites, syenites, porphyries, greenstones, and other substances formed by fusion. Let it, however, be understood, that the prodigious extent to which the metamorphism of the original strata has been carried in mountain-chains, and at different periods through all formations, though often probably connected with such igneous outbursts, must have resulted from a far mightier agency than that which was productive of the mere eruptions of molten matter or igneous rocks. The latter are, in fact, but partial excrescences in the vast spread of the stratified crystalline rocks,—symptoms only of the grand changes which resulted from deep-seated causes; probably from the combination of heat, steam, and electricity, acting together with an intensity very powerful in former periods.

Processes now going on in nature on a small scale, or imitated artificially by man, may enable us to comprehend imperfectly in what manner some of these infinitely grander ancient metamorphoses were effected; and the experimental science of chemistry when more extensively applied to the analysis of rocks, will, it is hoped, some day reveal still more important truths in this, which is still one of the most obscure points in the range of geological phenomena.

But speculations on such physical operations as those which have affected the surface of the earth, are not here called for. At all events, the earliest of the phenomena, with which alone we are at present concerned, or the first formation of the known crust of the planet, belongs to a period in which no definite order,

* See Alps, Appenines, &c., Quarterly Journal Geol. Soc. Lond., vol. v, p. 157, *et seq.*

—still less any trace of life,—has been deciphered by human labor.*

The design of this work is much more attainable. Its aim is to mark the most ancient strata in which the proofs of sedimentary or aqueous action are still visble,—to note the geological position of those beds which in various countries offer the first ascertained signs of life, and to develop the succession of deposits, where not obscured by metamorphism, that belong to such protozoic zones. In thus adhering only to subjects capable of being investigated, it will be seen, that geology, modern as she is among the sciences, has revealed to us, that during cycles long anterior to the creation of the human race, and while the surface of the globe was passing from one condition to another, whole races of animals—each group adapted to the physical conditions in which they lived—were successively created and exterminated. It is to the first stages only of this grand and long series of former accumulations, and to the creatures entombed in them, that attention is now directed.

The convictions at which I have arrived being the result of many years of research, I have been urged by numerous friends to give a condensed, and, as far as is practicable, a popular view of the oldest sedimentary rocks and of their chief organic remains, and thus to throw into one moderate-sized volume the essence of my large works,† as sustained by the publications of many other authors.

Geologists are now pretty generally agreed, that the oldest organic remains which are traceable, pertain to the lower division of the rocks termed Silurian; but before any description of these ancient deposits, or of those preceding them, is given, a few words are required, in explanation of the researches by which our acquaintance with the earliest vestiges of life and order in the protozoic world has been attained.

One of the chief steps which led to the present classification, as admitted by my contemporaries, was the establishment of the "Silurian System" of rocks and their imbedded fossils. Before the labors which terminated in the publication of the work so named, no one had unravelled the detailed sequence and characteristic fossils of any strata of a higher antiquity than the Old Red Sandstone; and even that formation was only known to be the natural base of the Carboniferous or Mountain limestone, and to contain a few undescribed fossil fishes. Not only were the re-

* The reader who desires to study the laws by which the superficial temperature of the earth has been regulated in the immensely long subsequent geological periods, will find them well explained in the profound essay of Mr. W. Hopkins, "On the causes of changes of *climate* at different geological periods," Quart. Journ. Geol. Soc. Lond., vol. viii, p. 56.

† See Silurian System, Murchison, 1839; and Russia in Europe and the Ural Mountains, by Murchison, de Verneuil, and de Keyserling (J. Murray, 1845).

many rocks which are now known to be younger
rian, were then considered to be of much more re
No one had then surmised, that the great series
with limestones and fossils, which have since been
nian, is an equivalent of the Old Red Sandstone
than, as well as distinct from, the deposits of the
rian era. On the contrary, British authorities b
was myself so taught) that the schistose and subc
of Devonshire and Cornwall were about the most
vast undigested heaps of greywacke. In short,
gists* of my early days were accustomed to leave
rocks, as constituting obscure heaps of sediment,
which no succession of "strata as identified by
could be detected. The result of research, how
the elimination of several well-defined groups, all
formerly merged in the unmeaning German term
(See Chapter 14.)

Desirous of throwing light on this dark subje
my valued friend and instructor, Dr. Buckland, a
most likely to afford evidences of order, and by hi
explored, in 1831, the banks of the Wye betv
Builth. Discovering a considerable tract in He
and Shropshire, wherein large masses of grey-col
out from beneath the Old Red Sandstone, and cor
fering from any which were known in the supe
began to classify these rocks. After four years
labor, I assigned to them (1835) the name Siluri
from the portion of England and Wales, in which
formations are clearly displayed, and wherein an
people, the Silures, under their king Caradoc (C
opposed a long and valorous resistance to the Ror
first, in the year 1833, separated these deposits† i
tions, and shown that each is characterized by p
remains, I next divided them (1834, 1835) into a
per group, both of which I hoped would be four
wide regions of the earth. After eight years of la
and closet, the proofs of the truth of those views
published in the work entitled the "Silurian Syst

* See those classical works, the first Geological Map of Mr.
Geology of England and Wales, by the Rev. W. D. Conybeare.

† For the first tabular view of these four formations, the bo
the unfossiliferous greywacke of the Longmynd, see Proceeding
vol. ii, p. 11, Jan. 1834. The characteristic fossil species were
ted, and hence the classification which is now sustained is esse
old. It had even been previously stated by me, that the lowes
ing formation, or the "*black trilobite schists and flags of Llandc
ed in thickness any of the superior groups.*"—Proc. Geol. Soc., v

During my early researches, it was shown that the lowest of these (1833) fossil-bearing strata reposed, in the west of Shropshire, on a very thick accumulation of still older sediment, as exposed in the ridge of the Stiper Stones, and the Longmynd mountain; and the strata of the latter not offering a vestige of former life, they were consequently termed unfossiliferous greywacke.

At that time it was also supposed, that the contiguous slaty region of North Wales, then under the examination of Professor Sedgwick, consisted of rocks, in part fossiliferous, and of an enormous thickness, which rose up, according to my friend and fellow-laborer, from beneath my Silurian types. Hence, another term, or that of Cambrian, was afterwards, or in the year 1836, applied to masses supposed to be inferior, before their true relations to the Silurian strata of Shropshire and Montgomeryshire had been ascertained. This assumed inferiority of position in the slaty rocks of North Wales being considered a fixed point, it was naturally thought, that such lower formations, the fossils of which were then undescribed, would be found to contain a set of organic remains, differing as a whole from those of my classified and published Silurian system. With other geologists, therefore, I waited for the production of the fossils which might typify such supposed older sediments; for in obtaining all the knowledge I had then acquired, by receding from upper strata whose contents were known to lower and previously unknown rocks, I had invariably found that the latter were characterized by many distinct and new organisms. This fact, which had been first established in the tertiary and secondary deposits, was thus proved to be universally applicable by the occurrence of similar distinctions in the Carboniferous, Old Red, and Silurian rocks.

It was, however, in vain that we looked to the production of a peculiar type of life from the "Cambrian" rocks. Silurian fossils were alone found in them; and the reason has since become manifest. The labors of many competent observers in the last fifteen years have proved that these rocks are not inferior in position, as they were supposed to be, to the lowest stratified rocks of my Silurian region of Shropshire and the adjacent parts of Montgomeryshire, *but are merely extensions of the same strata;* and hence the looked-for geological and zoological distinctions could never have been realized. In the following chapters it will be shown how Sir H. De la Beche, Professors Ramsay and E. Forbes, with Mr. Salter, and other geologists and palæontologists have demonstrated, that the fossil-bearing rocks of North Wales are both in their order and contents the absolute equivalents of the chief mass of the strata which had been described and named by me "Lower Silurian" in Shropshire and Montgomeryshire. These Government geologists have used my nomenclature in all

their works relating to North Wales, and have, in short, determined the question physically, as well as zoologically.*

But although in 1839, when my first work was completed, I held, in common with Professor Sedgwick, the erroneous idea of the infra-Silurian position of the rocks of North Wales, I soon saw reason to abandon that view, and to adopt (in the year 1841) the opinion which I have subsequently maintained. Thirteen years have elapsed since I was persuaded that the view I then took must be adhered to; first, because it had been ascertained that in Scandinavia, Russia, Bohemia, and other countries, the oldest traces of former life were the same as the lower Silurian types of the British Isles;—and next, because many of the fossils figured in my work as Lower Silurian had been detected in the slates of Snowdon, which were then considered to lie near the bottom of the so-called "Cambrian rocks."

The leading object, therefore, of the present work is, I repeat, to bring out the "Silurian System," not as a mere abridgment of its original form; but such as it finally became in the year 1849, when it was honored with the highest distinction which the Royal Society bestows,† and what it has proved to be, with the geographical and other additions made to it by the government surveyors at home, and by numerous geologists in other countries.

In extending my own researches to various distant lands, I found that as the true base of all rocks containing fossil remains was clear in Scandinavia, Russia, and Bohemia, and as the same fact was announced from North America, it was no longer difficult to describe the whole organic series *from a beginning*, and thus to record the succession of animals from their earliest known developments. In a word, as chroniclers of lost races, my associates and myself were enabled to register in our "Russia and the Ural Mountains," the types of former creatures from their apparent dawn. To the first chapters of that work, the reader is referred as fully explanatory of views which are here reiterated.‡

* See also Phillips, on the Malvern and Abberley Hills.—Memoirs, Geol. Surv., vol. ii, part 1, 1848.

† The Copley Medal.

‡ The reader who desires to consult the documents which explain how my induction was arrived at, is referred to a memoir entitled, "On the meaning attached to the term Silurian during the last ten years," which will indicate to him all my successive publications on this subject, including a geological map of England and Wales, published by the Society for the Diffusion of Useful Knowledge, in 1843. (Journal of Geol. Soc. Lond., vol. viii, p. 173. See also the memoir entitled, "On the meaning attached to the term 'Cambrian System,' and on the evidence since obtained of its being geologically synonymous with the previously established term 'Lower Silurian,'" Journ. Geol. Soc., Lond., vol. iii, p. 165.) At the same time that I must protest against the recent proposal to absorb my Lower Silurian into his Cambrian Rocks, let me record my high estimation of the original memoirs of Professor Sedgwick, especially those on North Wales, Cumberland, and the adjacent counties, which stand upon their own intrinsic merits. The publication on the palæozoic fossils of the Cambridge Museum, which he is bringing out in conjunction with

Then it was, that positive proofs, derived from a wide field of observation, enabled us to commence geological history, with an account of the entombment of the earliest animals recognizable in the crust of the globe; and also to indicate the successive conditions which prevailed upon the surface, in a long series of ages, and during the many changes of outline which preceded the present state of the planet. Then it was, that looking to the whole history of former life, as exhibited in the strata, it was demonstrated from phenomena in one great empire alone (as had to a great extent been shown in Britain), that during the formation of the sediments which compose the crust of the earth, the animal kingdom had been at least three times entirely renovated; the secondary and tertiary periods having each been as clearly characterized by a distinct fauna as the primeval series. In the work on Russia the sequence was thus followed out truly, from the most ancient fossil-bearing strata to the most recent stages in the geological series.

In this volume attention is chiefly restricted to what has proved to be the protozoic, or first era of life. The plan, therefore, pursued will be, so far, similar to that which was adopted in the earlier chapters of the work on Russia; and these first leaves of geological history will be written from the clear traces of a beginning,—a plan which, for want of knowledge, was impracticable in Britain when the "Silurian System" was published.

After a short sketch of the earliest and unfossiliferous sediments, full descriptions will be given of the Silurian rocks (Lower and Upper), followed by very brief accounts of the three overlying groups of palæozoic life, the Devonian, Carboniferous, and Permian.

The Devonian rocks were in previous years known only as the Old Red Sandstone, a name which has, indeed, become classical through the writings of Hugh Miller. These were termed Devonian, because the strata of that age in Devonshire, though very unlike the Old Red Sandstone of Scotland, Hereford, and the South Welsh counties, contain a much more copious and rich fossil fauna, and were demonstrated to occupy the same intermediate position between the previously described Silurian and Carboniferous rocks. At that time, however, none of the fossil fishes of the Scottish or English Old Red had been found in the sandstones, slates, schists, or limestones of Devonshire, or the

Professor M'Coy, will be, I doubt not, a lasting monument in the history of geological science. If that work had been published eighteen years ago, or in 1836, my friend, seeing that his Bala and my Llandeilo rocks were identical, might have proposed (although my fossils were first named and classified) that the Lower Silurian should be merged in the Cambrian. But, now that the terms Lower and Upper Silurian have been adopted in every country, the question is settled. My deep regret on the occasion of this difference of opinion has been expressed in the Preface; for in general views, as in private friendship, we are cordially united.

Rhine, and objections might have been raised to the opinion formed of the age of the deposits. But the discovery made in Russia,* and afterwards extended to Belgium and the Rhenish provinces, of Scottish ichthyolites being associated with numerous mollusca of the Devonshire rocks, firmly established the truth of the comparison.

The Carboniferous rocks, so elaborately and usefully developed in the British Isles, have been already well investigated by many writers, particularly by Professor Phillips, and have been found to extend, like the Silurian and Devonian, over immense regions in all quarters of the globe.

The great primeval or palæozoic series is now known to terminate upwards, in Europe, with certain deposits, for which, in the year 1841, I suggested the name of Permian. In the early days of geological science in England, this group was classed with the New Red Sandstone, of which it was supposed to form the base. But extended researches have shown from the character of its imbedded remains, that it is linked to the carboniferous deposit on which it rests, and is entirely distinct from the Trias, or New Red Sandstone, which, overlying it, forms the base of all the secondary rocks. The chief calcareous member of this Permian group was termed in England the Magnesian Limestone, in Germany the "Zechstein;" but as magnesian limestones are of all ages, and as the German "Zechstein" is but a part of a group, the other members of which are known as "Kupferschiefer" (copper slate), "Rothe todte liegende" (the Lower New Red of English geologists), &c., it was manifest that a single name for the whole was much needed. After showing how these variously named strata constituted one natural group, I therefore proposed to my fellow-laborers, de Verneuil and A. de Keyserling, that the vast Russian territory of Perm should furnish the required name. The term Permian† has, indeed, been adopted by several German authorities, and also by the Government Geological Surveyors of Britain.

In the opening chapter on the geology of Russia, we gave a general view of this palæozoic classification, as applied to Germany, France, Belgium, and North America; in all of which countries, as well as in Russia, it was shown, that a similar ascending order prevailed, from a base line of recognizable Silurian life, up through Devonian and Carboniferous deposits. In the nine years which have elapsed since the issue of that work, considerable additions have been made to our knowledge, and all of them sustain the truth of our generalization. We then scarcely knew of the existence of true Silurian deposits in Germany; nearly all the greywacke of the Rhenish provinces and the Hartz

* See Russia in Europe and the Ural Mountains, vol. i, p. 64.

† "Penéen" of D'Omalius d'Halloy. (See Chapter 12.)

having been assigned to the Devonian series. But since the opening out of the rich Silurian basin of Bohemia, which, in the hands of M. Barrande, has become the palæozoic centre of the continent, Thuringia and Saxony have been also found to contain Silurian rocks.

In Spain, several mountain chains have been shown by M. de Verneuil to consist of Silurian, followed by Devonian and Carboniferous rocks; whilst, in Portugal, Mr. Sharpe has described the first and last of these groups. Even Sardinia has exhibited, under the scrutiny of General A. della Marmora, her Silurian and superjacent coal deposits. Again, as Devonian and Carboniferous strata overlie older rocks in North Africa, and Devonian fossils occur towards Central Africa* and at the Cape of Good Hope, there are already fair grounds for believing, that a similar order pervades the axial lines or ancient mountains of that vast continent.

In northwestern Asia, the chief features of which are described by Humboldt and Rose, my colleagues and myself have explained how the Silurian rocks of the Ural chain are succeeded by younger palæozoic deposits, and Pierre de Tchihatcheff has indicated a great extension of similar formations over large tracts of Southern Siberia and the Altai mountains; whilst in northeastern Siberia, Adolf Erman has traced such rocks even to the Sea of Ochotsk.

In the giant Himalaya mountains, and in Hindostan, where till recently no systematic labors had been devoted to the older strata, we now know, that Silurian rocks, covered by secondary or mesozoic deposits, exist in those the highest mountains of the world; and that the Upper Punjaub contains a limestone charged with well-known carboniferous fossils, reposing, as in England, upon a red sandstone.† There is, indeed, every reason to believe that the mountain-chains of Tartary and China are composed, to a great extent, of these older rocks; for whilst extensive coal-fields have been long worked in the environs of the capital, Pekin, Devonian fossils of the very same species as those of England and the continent have recently been sent from Kwangsi, far to the south of Shangai. Other fossils, identified by de Koninck as Devonian forms, were brought by M. Itier, from the Yuennan province, one hundred leagues north of Canton.‡

* For North Africa, see Coquand, Bull. de la Soc. Géol. de France, 2nde Série, vol. iv, p. 1188. Some of the fossils collected by the enterprising traveller Overweg are also Devonian. For South Africa, the reader must consult a Memoir by Mr. Bain, not yet published in the Quart. Journ. Geol. Soc. Lond.

† The Himalayan data are described by Capt. R. Strachey; those of the Upper Punjaub, by Dr. A. Fleming. (Quart. Journ. Geol. Soc., vol. vii, p. 292, and vol. ix, p. 189.)

‡ See a description of the Chinese coal-field near Pekin, by Kovanko, Ann. des Mines de Russie, An. 1838, p. 191. No geologist can peruse Mr. Fortune's lively de-

In Australia, where a very short time since reference could be made only to rocks of the Carboniferous and Devonian age,* we hear of true Silurian strata containing fossils like those of the British Isles. Some species seem undistinguishable.†

In South America, the lofty Cordilleras and plateaux, whose mineral characters had been so admirably described by Humboldt, are shown by Alcide d'Orbigny to consist in great part of such ancient sediments. Still more clearly has North America been found to contain a vast succession of these palæozoic rocks, and especially of their lower members. Numerous geologists of the United States have demonstrated, that their ancient strata followed the same order on a very grand and usually unbroken scale (particularly in the western region); doubtless due to their having been exempted in such tracts from the intrusion of igneous rocks. Spread out in enormous sheets over the southern districts of Upper Canada, the Lower Silurian strata, invariably so called by every American geologist,‡ are there based on unfossiliferous slates, limestones and sandstones reposing on crystalline rocks, which, extending far northwards, are surmounted by other sedimentary masses similar to strata of the United States, and where Silurian fossils have been detected in limestones amid the polar ices. Adjacent to the southern end of this continent, similar remains have been collected by Darwin in the Falkland Islands.

In few of those regions, however, with the exception of North America (certainly not in the British Isles, where the strata are in many parts much obscured by igneous outbursts), is the sequence so undisturbed as in Scandinavia and European Russia. There, the successive primeval deposits extend over a large portion of the earth in regular sequence and in an unaltered state. Hence, though to the unskilled eye, Russia presents only a monotonous and undulating surface, chiefly occupied by accumulations of mud, sand, and erratic blocks, its framework, wherever it can be detected, exhibits a clear ascending series. The older sedimentary strata, deviating only slightly from horizontality, are there overlaid by widely-diffused masses of those Permian rocks which constitute the true termination of the long palæozoic period.

scription of the Bohea mountains, without suspecting, that a fine primeval succession may there be found. For the Chinese fossils, see Davidson, Quart. Journ. Geol. Soc. Lond., vol. ix, p. 353; and de Koninck, Bull. Acad. Roy. Soc. Belg., vol. xiii, pt. 2, p. 415.

* See Strzelecki's Australia, Foss. Fauna, Morris; M'Coy, Ann. Nat. Hist., 1847. [Also Dana's Rep. Geol. Expl. Exp., where many new species are described.—Eds.]

† Memoir by the Rev. W. B. Clarke, Quart. Journ. Geol. Soc. Lond., vol. viii; see also his collections, and those at the Government Museum.

‡ See particularly the works of James Hall and D. Dale Owen, the Reports of Logan—the chief geologist of Canada.

The following pages, as before said, will be chiefly devoted to the Silurian or first stages of this primeval series. They will be illustrated by wood-cuts representing the most important organic remains, and certain typical pictorial scenes, as well as vertical sections, chiefly taken from my original work. Faithful transfers from the original plates of the "Silurian System," will also be given, in a rearranged form, and with the modern nomenclature of the fossils.

If all the succeeding primeval rocks were to obtain the same amount of illustration as the Silurian, this work would be expanded far beyond the limits to which I must restrict it. The younger palæozoic, or the Devonian, Carboniferous, and Permian deposits, will therefore receive only such a description as may be sufficient to give the student a general view, and stimulate him to acquire a fuller acquaintance with them by consulting the various works wherein they are circumstantially described. But even the sketch of them in this volume will, it is hoped, suffice to show, that while the contiguous strata of two natural groups are intimately linked together by containing some species which are common to both, the principal fossils of each are certainly peculiar.

Although few mineral changes of the strata can be alluded to, an endeavor will be made to show, that gold, however it may now be spread over the surface, was originally accumulated in abundance in the older rocks only (especially in those which have been much altered), and in the associated eruptive masses.

Lastly, it is to be observed, that as the true sequence of the oldest fossiliferous strata was first detected in the British Isles, so the geological descriptions in this volume will be principally derived from our insular examples. At the same time, a general comparison will be instituted with the contemporaneous rocks of different quarters of the globe.

The importance of having, through patient surveys, mastered the obscurities which clouded the history of the earlier periods of animal life will thus, it is hoped, be rendered obvious, in showing that we have now obtained as correct an insight into the first fossil-bearing formations as we had previously acquired of the younger deposits.

Art. XLIII.—*On the Chemical Composition of Clintonite;* by Geo. J. Brush.

The name Clintonite was given some twenty-five years since by Fitch, Mather and Horton to a micaceous reddish-brown mineral occurring at Amity in New York; previous to this, the mineral had been called Bronzite.

In 1832 Clemson* investigated its chemical composition and finding it to differ from Bronzite, gave to it the new name of Seybertite; later, in 1836, it was analysed by Richardson† in Thomson's Laboratory, and apparently unaware of its previous history Thomson considered it a new species and named it Holmsite.

The chemical composition according to Clemson and Richardson is:

	$\dddot{Si}$	$\dddot{\overline{Al}}$	$\dddot{\overline{Fe}}$	$\dot{Mg}$	$\dot{Ca}$	$\dot{Mn}$	$\dddot{\overline{Zr}}$	$\dot{H}$	HF	
1,	17·0	37·6	5·0	24·3	10·7	—	—	3·6	—	*Clemson.*
2,	19·35	44·75	$\dot{Fe}$ 4·80	9·05	11·45	1·35	2·05	4·55	0·90	*Richardson.*

The unaccountable discrepancy between these analyses led the writer to an examination of its composition.

The specimens analysed were of a reddish-brown or copper-red color and had a sp. gr. of 3·148. H.=5·5. Alone, B.B. infusible, loses its brown color and becomes opaque; heated in a tube gives off a small amount of water, which has a neutral reaction. It is entirely decomposed by concentrated hydrochloric acid, without gelatinizing. A qualitative examination showed the presence of silica, alumina, iron, zirconia, magnesia, lime, potash and soda; no reaction for manganese by Crum's test.

A special examination was made to determine the state of oxydation of the iron; for this purpose the mineral was decomposed by hydrochloric acid in an atmosphere of carbonic acid, the result proved the iron to be peroxyd. Considerable care also was taken in ascertaining whether zirconia was present, as over two pr. ct. were found by Richardson; the iron obtained in the analyses was therefore reëxamined and in every instance an undoubted reaction for zirconia was obtained. On careful examination of the specimens with a magnifier, a dark brown mineral resembling zircon was found to be intimately associated with the Clintonite. A qualitative examination gave all the reactions of zircon,—the writer is therefore inclined to believe that the zircon obtained in the analyses may be due to the associated zircon and not essential to the mineral.

* This Journal [1], xxiv, 171.

† Rec. Gen. Sci. May, 1836, p. 332, and Jour. f. Prakt. Chem., xiv, 38.

In the quantitative analyses, the decomposition was effected by fusion with carbonate of soda; the alkalies were determined by Smith's method with carbonate of lime; the separation of zirconia and iron was made by the presence of tartaric acid in an ammoniacal solution, and the iron was precipitated by sulphid of ammonium. A particular examination was made to prove the purity of all the precipitates obtained. The following are the results of two analyses:

	1.	Oxygen.		2.	Oxygen.	
Silica,	20·24	10·74*		20·13	10·69	
Alumina,	39·13	18·29		38·68	18·07	
Peroxyd of Iron,	3·27	0·98	19·47	3·48	1·04	19·29
Zirconia,	0·75	0·20		0·68	0·18	
Lime,	13·69	3·89		13·35	3·80	
Magnesia,	20·84	8·34	12·55	21·65	8·66	12·78
Soda,	1·14	0·29		1·14	0·29	
Potash,	0·29	0·03		0·29	0·03	
Water,	1·04	0·92		1·05	0·93	
	100·39			100·45		

Only one determination was made of the alkalies. A third incomplete analysis gave, Silica 19·73, oxyd of iron and zirconia 4·15, lime 13·43, magnesia 20·81, water 1·09.

A great difference between the above analyses and those of Clemson and Richardson is in the amount of water. To determine this point the powdered mineral was dried over sulphuric acid and then very powerfully heated by a blast-lamp; in four experiments but a trifle over 1 per ct. was lost.

The analyses do not give a satisfactory formula, the oxygen ratios are:

	$\dddot{Si}$	$\dddot{\bar{R}}$	$\dot{R}$
No. 1,	10·74	19·47	12·55
No. 2,	10·69	19·29	12·78

The relation between $\dddot{\bar{R}}$ and $\dot{R}$ is 3 : 2 or 6 : 4, and the most simple ratio is 3 : 6 : 4, but the silica is in excess and the real ratio is nearer 3½ : 6 : 4. If in Clemson's analysis the 5 pr. ct. of iron be considered to be peroxyd ($5·0\ \dot{Fe} = 4·5\ \dddot{\bar{Fe}}$), we have the ratios

$\dddot{Si}$	$\dddot{\bar{R}}$	$\dot{R}$
9·02	18·92	13·76

which seems to render it probable that the true ratio for the mineral is 3 : 6 : 4, from which may be deduced the formula

$$\dot{R}\,\dddot{Si} + \dot{R}^3\,\dddot{\bar{Al}}^2.$$

The European species Xanthophyllite and Disterrite are very nearly related to Clintonite; in all physical characters except color they are identical with it. Their chemical composition as given by Meitzendorf and v. Kobell is:

* Atomic weight of silicium = 21·3.

	$\dddot{Si}$	$\dddot{Al}$	$\dot{Mg}$	$\dot{Ca}$	$\dot{Fe}$	$\dot{Na}$	$\dot{H}$	
Xanthophyllite,*	16·30	43·95	19·31	13·26	2·53	0·61	4·33	*Meitzendorf.*
Oxygen,	8·65	20·53	7·72	3·77	0·56	0·15	3·85	
					$\ddot{Fe}$			
Disterrite,†	20·00	43·22	25·01	4·00	3·60	0·57	3·60	*v. Kobell.*
Oxygen,	10·61	20·20	10·00	1·14	1·08	0·10	3·20	

These differ from Clintonite in containing about 4 pr. ct. of water; if this be considered as replacing magnesia in the manner suggested by Scheerer the relation of the oxygen in the sesquioxyds and protoxyds is nearly 6 : 4; but the ratio 3 : 6 : 4 is not in accordance with the amount of silica obtained in the analyses. If the same replacement be allowed in Disterrite and the iron be assumed to exist as protoxyd, the oxygen ratio will be—

$$\begin{array}{ccccc} \dddot{Si} & & \ddot{R} & & \dot{R} \\ 16{\cdot}61 & : & 20{\cdot}20 & : & 13{\cdot}20 \end{array}$$

or almost exactly 3 : 6 : 4.

From these remarks it seems likely that the three species may all be brought under the general formula $\dot{R}\dddot{Si}+\dot{R}^{4}\dddot{Al}^{2}$ = Silica 19·19, alumina 43·54, lime 11·86, magnesia 25·41.

Munich, August 7th, 1854.

Note by J. D. Dana.—It is important to note that the ratio between the oxygen of the protoxyds and peroxyds together and that of the silica (the water being disregarded), is the same for Clintonite and Disterrite. We thus obtain—

	$\dot{R}+\ddot{R}$		$\dddot{Si}$		
1. Clintonite, . . .	32·02	:	10·74	=	3 : 1·06
2. " . . .	32·07	:	10·69	=	3 : 1
3. Disterrite, . . .	32·50	:	10·61	=	3 : 0·98

corresponding each to the ratio 3 : 1. This therefore appears to be the fundamental ratio of the species. We here take the iron in Disterrite as peroxyd, as published by von Kobell, which is its condition in Clintonite; this gives for the oxygen of the protoxyds and peroxyds the ratio 11·24 : 21·26. The formula $(\dot{R}^{3},\ddot{R})\dddot{Si}^{\frac{1}{3}}$ expresses the ratio 3 : 1. In the Clintonite, $\dot{R}^{3}$ is to $\ddot{R}$ as 2 : 3; in the Disterrite nearly as 1 : 2. Expressing the ratio 2 : 3 for the Clintonite, this formula becomes $(\frac{2}{5}\dot{R}^{3}+\frac{3}{5}\ddot{R})\dddot{Si}^{\frac{1}{3}}$.

If we regard one-third of the peroxyd in Mr. Brush's analysis as replacing silica, the oxygen ratio for $\dot{R}$, $\ddot{R}$, ($\dddot{Si}$) becomes 12·78 : 12·86 : 17·12, or between the bases and the rest 25·64 : 17·12 = 3 : 2, giving the formula on page 129 of this volume

$$(\tfrac{1}{2}\dot{R}^{3}+\tfrac{1}{2}\ddot{R})(\dddot{Si},\dddot{Al})^{\frac{2}{3}}.$$

* The mean from four analyses by Meitzendorf, from the 1st Supplement of Rammelsberg's Handw. Min., p. 158.

† Jour. für prakt. Chem., xli, 156.

ART. XLIV.—*Contributions to Mineralogy;* by Dr. F. A. GENTH of Philadelphia.

1. *Pyrophyllite.*

THIS interesting mineral was reported by Prof. C. U. Shepard to occur near Crowder's Mountain in North Carolina; the exact locality in that state is however "Cotton Stone Mountain," Mecklenburg county. My specimens from this locality were not pure enough for analysis; I give therefore in the following only the results of the examination of the same mineral from Chesterfield District, S. C.

B.B. it exfoliates into a fan-like opaque white mass of more than twenty times its original bulk; fuses with great difficulty into a white blebby slag. The lustre of the N. C. specimens is pearly; that of those from S. C. inclining to greasy.

The following are the results of my analyses:

Silica,	64·82	66·01
Alumina,	28·48	28·52
Sesquioxyd of iron,	0·96	0·87
Magnesia,	0·33	0·18
Lime,	0·55	0·23
Water,	5·25	5·22

2. *Chrysotile.*

The beautiful fibrous mineral of a yellowish white color and silky lustre, which occurs in small veins in serpentine at Abbottsville, N. J., has been examined by Mr. Edwin L. Reakirt.

B.B. it whitens, becomes brittle and fuses with difficulty into a white slag; with cobalt solution, flesh-colored. It contains:

Silica,	42·52	42·72
Alumina,	not det.	0·38
Sesquioxyd of iron,	not det.	0·30
Magnesia,	42·35	42·99
Water,	14·31	14·18

3. *Scolecite.*

Lyman Wilder, Esq., of Hoosick Falls, N. Y., kindly furnished me with the material for analysis of a mineral from the East Indies. It consisted of globular masses 5 to 6 inches in diameter of a radiated structure. Sometimes there was found between the radii, which have a vitreous lustre, the same mineral of a reticulated structure with pearly lustre.

B.B. it fuses with intumescence easily to a blebby glass. Mr. Wm. J. Taylor analyzed it and found it to contain:

Lime,	13·80
Soda,	0·45
Potash,	0·13
Water,	13·46

4. *Owenite identical with Thuringite.*

In a previous paper (Am. Journ. Sc., 2d Series, vol. xvi, p. 165), I described a mineral from Harper's Ferry, Va., under the name "*Owenite*" as *new*, remarking, however, that the difference between it and Thuringite could be detected *only* by a chemical examination. I was unable at that time to obtain any genuine Thuringite for a comparative analysis, and, I must confess, I had *too much* confidence in Prof. Rammelsberg's analysis, to think it would need a repetition, since the difference was about 16 pr. ct. of alumina. In the meantime Prof. J. L. Smith, (Am. Journ. Sc., 2d Series, vol. xvi, p. 131), announced the identity of Owenite and Thuringite, but, the material for his analysis, as he told me, having been slightly altered by oxydation, it was desirable to reëxamine the fresh mineral. Jos. A. Clay, Esq., with the greatest liberality permitted me to take from his specimen a sufficient quantity for analysis. The original label of Dr. Krantz of Bonn, which was still with it, gave the locality "Schmiedeberg near Saalfeld in Thuringia." The material for the following analyses made by Mr. Peter Keyser, was not in the least degree altered, and the results, which he obtained show the identity in composition of Thuringite and Owenite, which latter name I therefore withdraw. For comparison I give besides the analyses of the Thuringite from Schmiedeberg also that of the same mineral from Harper's Ferry, Virginia (the Owenite).

	I.	II.	III.	IV.	V.
	From Schmiedeberg.				From Harper's Ferry. (Owenite.)
Silica,	24·36	23·09	23·19		23·21
Alumina,	15·69	15·86	15·34		15·59
Sesquioxyd of iron,	13·08	14·31	14·03		13·89
Protoxyd of iron,				34·20	34·58
Magnesia,	1·26	1·29	1·87		1·26
Lime,	0·00	0·00	0·00		0·36
Soda,	*trace*	*trace*	*trace*		0·41
Potash,					0·08
Water,				10·57	10·59

(*To be continued.*)

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the so-called Benzoëoxyd and some other conjugate compounds.* —List and Limpricht have studied the crystalline body obtained nine years since by Ettling and Stenhouse by the dry distillation of benzoate of copper, and which has been supposed to have the formula $C_{14}H_5O_2$. The authors found for pure and well crystallized substances the formula $C_{13}H_5O_2$, which agrees well with the analyses of Ettling but not with those of Will, Stenhouse and Gerhardt. This formula being doubled becomes $C_{26}H_{10}O_4$ or $C_{12}H_5O+C_{14}H_5O_3$, and the reactions clearly shew that the body in question is really the benzoate of oxyd of phenyl. An aqueous solution of caustic potash does not decompose the benzoate of phenyl when boiled with it under atmospheric pressure, but when heated to 150°–170° in a closed tube with a concentrated solution of the alkali the benzoate is dissolved, and when the solution is saturated with sulphuric acid, benzoic acid separates and an oily body rises, easily recognized to be phenyl-alcohol or carbonic acid. An alcoholic solution of caustic potash readily decomposes the benzoate, yielding of course the same products. Heated in a closed tube with ammonia the benzoate yields benzamid and carbolic acid. Bromine acts readily upon the benzoate of phenyl yielding products of substitution in which 1, 2, and 3 equivalents of hydrogen are replaced by as many of bromine. Decomposition of these compounds with caustic potash proves that it is the hydrogen of the oxyd of phenyl alone which is replaced and not that of the benzoic acid. Chlorine acts in a similar manner. A cold mixture of 1 part of nitric acid and 2 parts of sulphuric acid converts the benzoate of phenyl into a yellow crystalline body in which 3 equivalents of hydrogen are replaced by 3 of NO_4: in this case, however, the decomposition with caustic potash shews that 1 eq. of hydrogen is replaced in the benzoic acid, and 2 eq. in the oxyd of phenyl. Setting out from the theory that many organic bodies now regarded as aldehyds, &c., are in reality ethers, and have twice the equivalent usually attributed to them, the authors point to the fact that Ettling's Parasalicyl is a compound of anhydrous benzoic and anhydrous salicylous acids, and further that oil of bitter almonds and cuminol are both ethers and not aldehyds, since the former yields with an alcoholic solution of potash, benzoic acid and benzoic alcohol; the latter cuminic acid and cuminic alcohol.—*Ann. der Chemie und Pharmacie*, xc, 190.

2. *Researches on different questions in Organic Chemistry.*—Strecker has communicated to the Academy of Sciences in Paris a memoir with this title, containing the solution of several important questions. We shall give the principle results of these investigations under separate captions.

Composition of tannic acid.—Strecker finds that pure tannic acid is represented by the formula $C_{54}H_{22}O_{34}$, and that in this formula 3 eqs. of water are basic, so that the anhydrous acid is $C_{54}H_{19}O_{31}$. The lead salts of tannic acid contain from 3 to 10 eqs. of oxyd of lead.

The combinations of tannic acid with sulphuric and chlorhydric acids mentioned by Berzelius are in reality mechanical mixtures and are not constant in composition. The splitting of tannic acid into gallic acid and glucose, discovered two years since by Strecker, may be represented by the equation $C_{54}H_{22}O_{34}+8HO=3(C_{14}H_6O_{10})+C_{12}H_{10}O_{10}$. Gallic acid, according to Strecker, is also a tribasic acid and is represented by the formula $C_{14}H_3O_7+3HO$.

Decomposition of brucine by nitric acid.—The action of nitric acid upon brucine has been studied by Laurent, Gerhardt, Liebig and Rosengarten, but the products of this action have never been satisfactorily ascertained. Strecker finds that these products are cacotheline, nitrite of methyl, oxalic acid and water. The reaction is represented by the equation

$$C_{46}H_{26}N_2O_8+5NO_5.HO=C_{40}H_{22}N_4O_{18}+C_2H_3O,\ NO_3+2C_2O_3,\ HO+4HO.$$

The constitution of cacotheline was determined by means of its platinum salt; Laurent assigned to it the formula $C_{42}H_{22}N_4O_{20}$.

Hydrocyanaldin.—When a mixture of aldehyd-ammonia, prussic acid and an excess of muriatic acid are allowed to stand for some days without heat no alanin is formed, but colorless crystals of a new body which Strecker terms cyanaldin, are deposited. It is neutral, insipid, insoluble in water, alcohol and ether; it fuses and sublimes at a moderate heat; potash decomposes it with evolution of ammonia. The formula of this body is $C_9H_6N_2$ or $C_{18}H_{12}N_4$; it does not appear to possess basic properties.

Production of propionic acid in fermentation.—In preparing lactic acid by the fermentation of sugar by Bensch's process, Strecker observed that when the temperature of the mixture was kept low for several months, large quantities of mannite were formed. A mixture which deposited crusts of lactate of lime was abandoned during several months in summer in a place where the temperature did not exceed 22° C., the water being renewed from time to time. The volatile acids formed being isolated proved to be propionic and acetic acids, both in very large quantity.

3. *On some combinations of hydrargyro-methyl and hydrargyrethyl.*—The researches of Frankland on the compounds of methyl and amyl with mercury are well known. Strecker has succeeded—and without any knowledge of Frankland's experiments—in preparing the corresponding compound of mercury and ethyl, by simply exposing a mixture of mercury and iodid of ethyl to diffuse light at ordinary temperatures. After some time, crystals are formed which increase till the whole liquid becomes solid. The crystals are readily recrystallized from boiling ether or alcohol, and separate in thin, colorless very brilliant laminæ. They sublime at 100° C., but fuse only at a higher temperature: they are insoluble in water but soluble without decomposition in ammonia and solution of caustic potash. The formula of these crystals is $C_4H_5 . Hg_2I$. The nitrate crystallizes in colorless prisms and is represented by the formula C_4H_5O, NO_5. The chlorid corresponds to the iodid. These compounds are all decomposed by light, which is the reason why Frankland could not obtain them.

Constitution of quinine.—Strecker has at length established the constitution of this important base, and finds it to be represented by

sulphates are $C_{40}H_{24}N_2O_4, HO.SO_3$ and $C_{40}H_{24}N_2O_4, HO.SO_3 + HO, SO_3$. A mixture of iodid of ethyl and quinine dissolved in ether give crystals of iodid of ethyl-quinine or iodid of quinine in which 1 eq. of hydrogen is replaced by 1 eq. of ethyl; iodid of methyl produces a similar iodid of methyl-quinine; both these compounds are powerful bases. The author concludes that ethyl-quinine belongs to Hofmann's fourth class of bases and corresponds to ammonium NH_4. Quinine is itself a nitrid base or an ammonia, and contains in its molecule 3 compound radicals replacing hydrogen.

Artificial production of oil of cinnamon.—STRECKER shewed some years since that styrone is the alcohol of cinnamic acid; he now finds that it may be readily transformed into its aldehyd by the action of air and platinum black. This aldehyd is pure oil of cinnamon.

Artificial production of taurine.—After several unsuccessful attempts, STRECKER has succeeded in producing this remarkable body by heating isethionate of ammonia to a temperature of 230°, until the salt has lost 11 per cent. of its weight. The reaction is represented by the equation

$$NH_4O.C_4H_5O.2SO_3 = C_4H_7NO_6S_2 + 2HO.$$

The fused mass dissolved in water, precipitated by alcohol and then redissolved in water, gave large crystals identical with the taurine obtained from the bile.—*Comptes Rendus*, xxxix, 49, *July*, 1854.

4. *Contributions to the chemical history of Glucina.*—WEEREN has published an investigation of glucina, which, although simply introductory to a more extended study of the salts of this metal, contains many interesting results. The author first examined the different methods which have been proposed for the separation of glucina from alumina. The result of this examination shewed that glucina cannot be completely separated from alumina either by means of carbonate of ammonia, caustic potash, sulphurous acid, or carbonate of baryta. Berzelius's method, by means of sal-ammoniac, is the only process which can be relied upon. In the execution of the separation by this method, however, the auther found it necessary to observe the following precautions. 1. To precipitate the two earths with ammonia after adding a very concentrated solution of sal-ammoniac. 2 To boil the mixture a long time. 3. To avoid too great an evaporation of the liquid. 4. To precipitate the glucina with sulphid of ammonium. The author considers the hydrate of glucina to be $G_2O_3 + 3HO$, though it is very readily decomposed, and loses a portion of its water at 110°, which renders its analysis very difficult. The numerous analyses of the carbonates of glucina lead to very improbable formulas, apparently either from their instability, or because the carbonates are mechanically mixed with variable proportions of hydrate. The sulphate of glucina has the formula $G_2O_3, 3SO_3 + 12HO$ as given by Awdejew; it loses 11 eq. of water between 100° and 110° C. but retains the 12th till a much higher temperature. The author has redetermined the equivalent of glucina by the careful analysis of the sulphate, and finds as a mean of five determinations the number 157·64, if we consider the glucina as GO, or 472·90 if we consider it G_2O_3. He prefers, however, the mean

the equivalent of the metal 86·50—taking glucina as G_2O_3, or 57·68 taking glucina as GO.—*Pogg. Ann.*, xcii, 91. W. G.

5. *On a new Test for Zirconia;* by G. J. Brush, (J. f. pr. Chem., lxii, 7, 1854.)—In a recent examination of some American minerals, one of them regarded as rutile was decomposed by fusion with caustic potash, the fused mass dissolved in diluted hydrochloric acid, and the solution boiled with tin. Finding no reaction for titanium, the solution was tested with turmeric paper to ascertain whether it were alkaline or not. The paper was immediately colored orange-red. On dissolving a portion with still more acid, the same color was obtained, although with litmus paper the solution afforded an acid reaction.

On further analysis the mineral proved to be zircon, and it was found that the orange color was due to zirconia. To try the delicacy of the test, different zirconia minerals were examined, as zircons of Ceylon, the Ural and New York, Eudialyte, Wöhlerite and Catapleiite, and they all gave the orange color. Acid solutions containing the alkaline earths, alkalies, manganese, iron, zinc, tin, but not zirconia, were tested with turmeric paper, but no reaction was obtained except from a strong solution of sesquichlorid of iron, which in consequence of its own deep color discolored the paper. This may be avoided by reducing this chlorid to the protochlorid. An acid solution containing all but two of the above-mentioned substances, with a large excess of iron was reduced by tin. The solution gave no reaction with turmeric paper; but on adding a small portion of zirconia the color of the paper became orange-red. The presence of boracic acid wholly disguises that of zirconia. A known zirconia compound (a hydrate) was dissolved in hydrochloric acid and diluted with 3000 parts of water. The solution gave the deep orange-red color of zirconia; with 2000 parts more of water, the action was still distinct.

It appears therefore that turmeric paper is a simple and characteristic test of the presence of zirconia in an acid solution, when boracic acid is not present; if the solution is a very weak one, the paper should be left in it from half to one minute. It is to be observed that the solution should not be so acid that the acid itself will act on the paper and discolor it.

6. *On the Electricity of the Atmosphere;* by M. L. Palmieri, (Bib. Univ. Genève, xxvi, 1854, 105.)—M. Palmieri shows that the method of ascertaining the electrical condition of the atmosphere by the two ordinary methods, a fixed conductor, and a moveable electroscope (Peltier's method,) are unsatisfactory. The latter will often show indications when none is perceived with the former; and is also itself uncertain from difficulties of manipulating or the influence of the observer. M. Palmieri proposes therefore a new method by means of *a moveable conductor;* it is in use under the direction of the king of Naples at the Meteorological Observatory of Vesuvius at a height of 590 metres above the sea-level. The conductor extends above the roof of the building, and is arranged so as to be raised or depressed at will; its connections are of a nature to insulate it; and in the room below, it connects with an electroscope or other instruments as desired.

On elevating the conductor, the electric tension is measured by an electroscope; it is ascertained whether it be positive or negative by an electroscope of Bohnenberger; and in case of electricity in a dynamic state, a galvanometer may be connected with it by one wire whilst the other passes to the soil. An ordinary fixed conductor gives no indications if the earth and space about it are in equal electrical states, the condition then being one of equilibrium: But with Palmieri's arrangement, the conductor is raised, and the influence of the superior atmospheric beds become predominant; the electrometer will indicate tension according to the vertical distance passed through and the efficiency of the opposite electricities. Moreover the fixed conductor if there is much rain, is discharged as rapidly as the electricity is received; while the moveable conductor charges itself much more rapidly and gives decided indications.

M. Palmieri has ascertained some points of interest respecting atmospheric electricity.

He has verified on some serene days a diurnal periodicity in the electricity, as remarked by Schubler and others.

He finds that the interior even of storm clouds is always positive, except during the shock of lightning, or in the case of rain, hail or snow. In case of a rain storm commencing a long distance from the place of observation, and carried over this place by the wind,—he observes that when the storm is far off the electricity is positive, gradually increasing with the progress of the storm; when the storm comes near, the electricity is strongly negative, often producing sparks. When the rain has reached the place of observation, it is again positive; then strongly negative just after it passes, and finally positive, its usual condition. Between each period there is a momentary neutral point. The distance at which the rain begins to induce *negative* electricity is very variable, sometimes at 30 miles, and other times when the first drops begin to fall. The rain hence occupies a region charged with positive electricity surrounded by a zone of negative electricity. When there are several successive showers of rain, these results will be much complicated. Hence the idea of negative or neutral rain storms must be abandoned; negative clouds do not exist, and negative electricity is found in the atmosphere only in case of rain, hail or snow. During these three periods the effects are vastly the most energetic.

7. *A Vacuum made by Chemical means;* by M. C. Brunner.—The method is that by means of carbonic acid. The process is as follows: A tubulated bell glass is placed on a plate of ground glass, over a dish of sulphuric acid, above which acid on a support there is some quicklime; the bell glass is then filled through the tubulure with carbonic acid, the tube supplying this gas reaching to the bottom of the bell glass; after the atmospheric air is thus excluded, the tubulure is hermetically closed by a stopper through which passes a tube having a bulb containing water at the exterior end, while the interior extremity is near the lime. There is no action of the lime while it is dry, but on warming the bulb, water falls on the lime, and soon after, the carbonic acid is absorbed, while at the same time the moisture is taken up by the sulphuric acid.

Ammonia may also be employed, with some modifications of the process.—*Bib. Univ. de Gen.*, xxiv, 164.

II. Mineralogy and Geology.

1. *Contributions to Mineralogy;* by James D. Dana.—The following are figures of crystals of some species of American minerals.

Figs. 1, 2, *Compound Crystals of Copper Glance.* In fig. 1, composition is parallel to I, or the prism of 119° 35′; also parallel to $2\breve{i}$, producing thus a cruciform twin with the angle of intersection 125° 28′. In fig. 2, the composition is parallel to a face $\frac{1}{2}$, and the prisms cross at angles of 92° 5′ and 87° 55′.

Fig. 3, *Pyrites*, a crystal from Rossie, New York, in the cabinet of W. T. Vaux, Esq., of Philadelphia. The crystal is nearly an inch in diameter, and is peculiarly fine, although the surfaces, while very smooth, are not polished.

Fig. 4, *Mispickel* (Danaite). From a crystal in the cabinet of the late J. E. Teschemacher of Boston. It presents the unusual planes $1\bar{2}$, 3, and $3\frac{\bar{3}}{2}$. It is from Franconia, New Hampshire.

Figs. 5, 6, *Quartz.* Figure 5 represents crystals from Quebec, furnished the author for examination by Mr. T. S. Hunt. They were from a half to three-fourths of an inch in length. The planes $-\frac{1}{2}$ are delicately etched, being covered with small triangular prominences, whose sides are parallel to the basal and two terminal edges of the plane $-\frac{1}{2}$. The other faces were shining. Other crystals from the same place were the inverse of that figured. Fig. 6 is taken from a slender crystal about one-eighth of an inch long, in the cabinet of J. E. Teschemacher. The narrow plane $-\frac{1}{2}$ instead of being plane, is excavated through its length with a neat triangular channel bordered by a sharp edge. The planes 22 and $3\frac{3}{2}$ are hardly separated by a well-defined edge, and are somewhat uneven in surface. The other planes are bright. The inclination of $\frac{8}{8}\frac{8}{8}$ on R gave on measurement 175°.

Figure 7, *Pyroxene.* A fine grass-green crystal an inch long with bright surfaces, from Long Pond, New York.

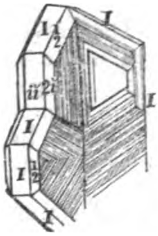
Figure 8, *Spodumene.* This is a new figure of the same crystal before roughly drawn by the author. The planes –22 and x appear to be hemihedral: but whether this is an accidental distortion or not is uncertain.

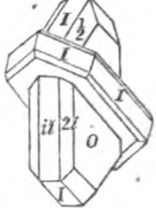
Figure 9, *Babingtonite?* The figure represents small polished black crystals occurring in mica slate at Athol, Massachusetts, referred to Babingtonite by Prof. C. U. Shepard. The form is triclinic, and the following are approximately the angles:

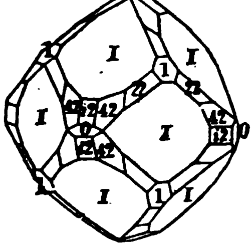
$O : I = 90°\text{–}91°$,	$I : I' = 110°\ 30'$ and $69°\ 30'$.
$O : I' = 85°$,	$I : i\breve{3} = 129°$.
$O : \frac{1}{2}' = 153°\ 20'$,	$I' : i\breve{3} = 120°\ 30'$.
$O : -1 = 135°\ 40'$,	$I : \frac{1}{2}' = 95°\ 30'$.
$O : 1 = 135°\ 30'$	$O : i\breve{3} = 95°\ 30'$.

It resembles Epidote, and it is possible, that O, $\frac{1}{2}'$ and I', correspond respectively to O, $\frac{1}{2}i$ and $2i$ in Epidote.

Figure 10, *Zircon*, from McDowall Co., North Carolina. The crystals are found in the sands of the Gold region, and are seldom over a sixth of an inch in length.







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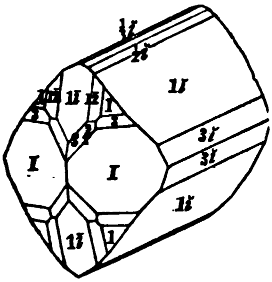

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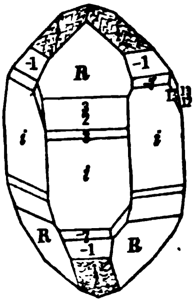

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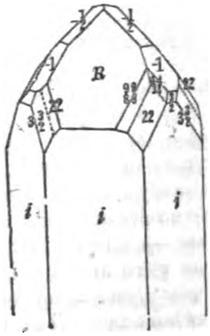

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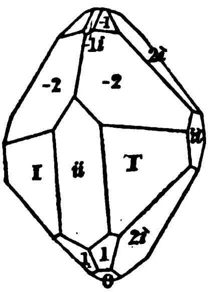

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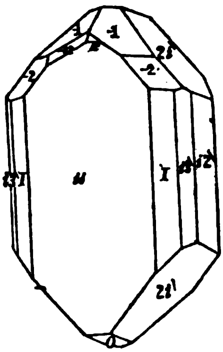

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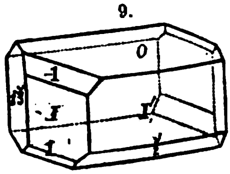

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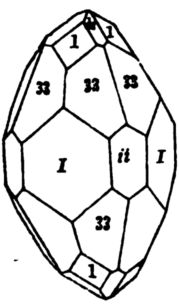

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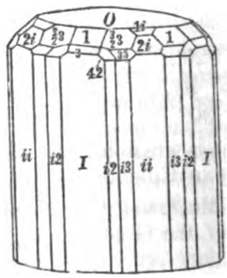

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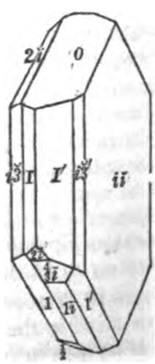


and the faces are all bright. The Idocrase occurs massive or subcolumnar at that place, and there are often cavities of fine crystals. The crystals are sometimes thinly sprinkled over with dark green crystals of pyroxene, hardly a line long, which are loosely attached to the idocrase and are evidently of subsequent origin, apparently a result of its alteration. The crystals of idocrase undergo ready alteration, and often the surface peals off in layers, over the whole summit; and the alteration has proceeded inward with such exactness that the removed layer leaves behind the same number of small secondary planes as in the complete crystal, and all equally brilliant in polish. This is the more wonderful since there are no cleavages parallel to all or any of these small secondary planes. The color of the idocrase is brown and greenish-brown. In one specimen having a columnar structure, the columns along a straight line two inches in length met at an angle of 56° to 60°, indicating composition parallel probably to the plane $4i$.

Figure 12, *Albite* from the Middletown feldspar quarry. The crystals are half to one inch long, and in part transparent. They are mostly in twins parallel to $i\bar{i}$, but so united that the plane O and $1\bar{i}$ are in the same plane nearly. From one crystal the author obtained for $O : i\bar{i}$, 92° 40′ and 87° 20′.

Figure 13. The so-called *Loxoclase* from Hammond, St. Lawrence Co., N. Y., a feldspar shown by Smith and Brush to be orthoclase. The crystals are an inch long or larger, and of a nearly white color.

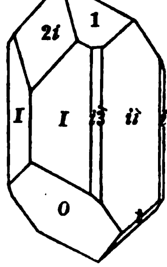
Figure 14. *Topaz* from Trumbull, Ct. The crystal is transparent and a fourth of an inch in diameter. The most of the terminal planes are rough.

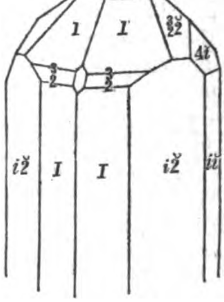
Figures 15, 16, 17, *Tourmaline*. Figure 15 represents an upper view of the termination of a slender transparent crystal of a light yellowish color, from London Grove, near Unionville, Pennsylvania. It is from the cabinet of T. F. Seal, of Unionville. Figure 16 is from a black crystal one inch in length from Northern New York. Fig. 17 is a brown tourmaline from Canada, received from T. S. Hunt of the Geological Commission, Canada. R is the rhombohedron of 103°.

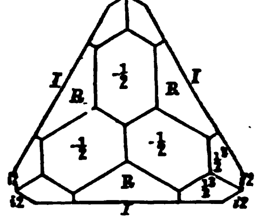
Figure 18, *Apophyllite* from the Cliff mine, Lake Superior region, Michigan. The crystals are from one-fourth to half an inch in length and transparent. They are generally tabular, but occasionally octahedral. The plane $i3$ is not the prism usually noted on crystals of this species.

Figures 19, 20, *Barytes* or Heavy Spar.—Figure 19 represents a crystal from Cheshire, Conn.; figure 20, one from the Eldridge gold mine, Buckingham Co., Va., sent the author by Dr. F. A. Genth. The size varies between one-fourth and three-fourths of an inch. All the planes but the striated vertical planes are highly polished. The terminal face is often cavernous, the polished plane O being often finished at different elevations short of the actual summit of the crystal.

Figures 21, 22, 23, *Anglesite*, from different crystals from the Wheatley mine, Phœnixville, Pa., furnished the author by the proprietor, C. M. Wheatley. The crystals present the same planes nearly, but are elongated in different directions. They are brilliant, glassy, often transparent, and sometimes an inch or more in length.







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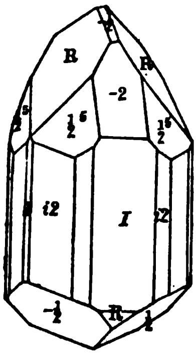

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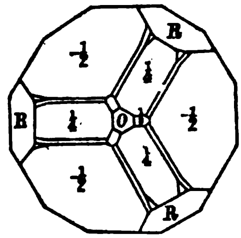

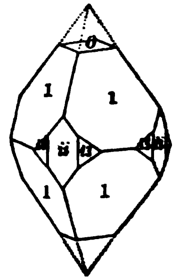

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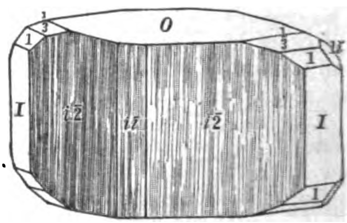

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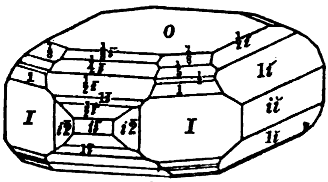

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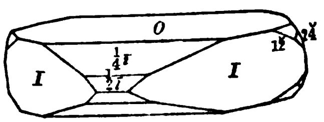

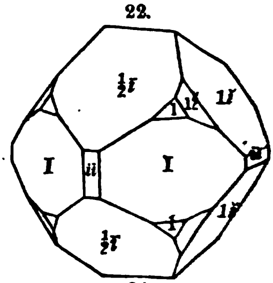

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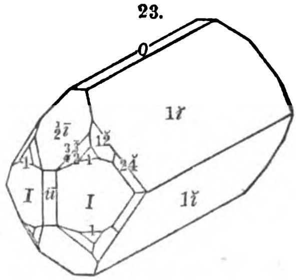

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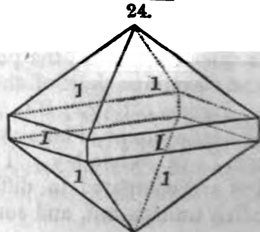


from crystals received from Prof. Lewis R. Gibbes, of Charleston, South Carolina.

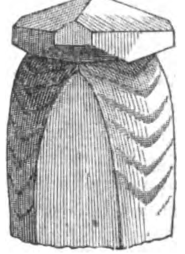


Figure 25. *Calcite.*—From the Copper mine, Bristol, Connecticut. The crystals are implanted thickly over the rock, and are about half an inch long. The six-sided prism after a gradually accumination to a three-sided summit by oscillation with a scalenohedral form, (probably 1^3,) is subsequently terminated by a crystal of the nail-head form, making literally a nail-head crystal. The planes of the summit are the rhombohedral $-\frac{1}{2}$R.

26.

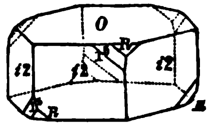


Figure 26. *Dolomite*, from Hoboken, New Jersey. The crystals are glassy, a third to a quarter of an inch across, and occur in cavities in massive dolomite.

2. *Beiträge zur Kenntniss der Eisenhohofen-Schlacken, nebst einem geologischen Anhange*, (on Iron Furnace Slags, etc.); by J. F. L. Hausmann. 102 pp. 8vo. (From Studien des Gött. Ver. Bergm. Freunde.) Göttingen, 1854.—Prof. Hausmann has contributed largely to our knowledge of the slags of furnaces, and presented the bearing of the subject on the origin of minerals. This recent paper contains descriptions and analyses of various furnace products, and an application of the facts to Geology.

(1.) *A slag called Kieselschmelz* (Siliceous Enamel).—This slag is described by M. Koch, (Beit. zur Kenntniss kryst. Hüttenprodukte, Göttingen, 1822, 8vo, p. 40–81). It occurs in 6-sided prisms, tabular or elongated, with the terminal edges sometimes removed, producing rarely a six-sided pyramidal termination. Color whitish, gray, yellowish, brownish, greenish. G.=2·72. H.=·7. Translucent, to subtranslucent. Lustre waxy, weak. Crystals sometimes glassy at centre.

Analyses of specimens from iron-furnaces: 1, 2, by A. Knop, different specimens of same slag from the Hartz; 3, Dr. Limpricht, another slag from the Hartz; 4, Knop, from Neuwerk, of the Brunswick Hartz:

	1.	2.	3.	4.
Silica,	55·78	59·45	54·27	55·41
Alumina,	15·28	15·43	9·40	11·52
Lime,	29·21	25·12	29·30	28·10
Magnesia,	——	——	1·15	1·89
Protox. iron,	—— =100·27	—— =100	6·58=100·70	3·08

Analyses 2, 3, and 4, give for the oxygen ratio of $\dot{R}$, $\ddot{\bar{R}}$, $\dddot{Si}$,

2,	7·18 : 7·21 : 31·50 = 1 : 1 : 4 (nearly).
3,	10·29 : 4·39 : 28·75 = 2·34 : 1 : 6·55
4,	9·47 : 5·38 : 29·20 = 1·76 : 1 : 5·43

These ratios afford for the oxygen of all the bases and the silica the ratio nearly of 1 : 2, which is characteristic of both pyroxene and beryl; Hausmann deduces the formula $\dot{C}a^3 \dddot{Si}^2 + \ddot{\bar{A}}l \dddot{Si}^3$, and points out the close similarity to beryl, if glucina be taken as a protoxyd, the difference

being that lime here replaces the glucina. Analysis 2 affords very nearly this formula.*

The form is related to that of beryl. The inclination of the pyramidal planes on the lateral faces of the hexagonal prism according to Koch is 103° 15′ 46″, or on *O* (the base) 166° 44′ 14″. This plane referred to the beryl type is, the plane $\frac{4}{5}$.†

(2.) *Gehlenite Slag.*—Among the slags of iron furnaces, 4-sided rectangular prisms or tables are not uncommon. D. Forbes first pointed out a slag in 4-sided prisms which was near Humboldtilite. Hausmann has described similar slags. Rammelsberg refers a slag from Oldbury, England, described by Percy, to Gehlenite, it giving the composition $3\dot{R}^3\,\dddot{S}i + \bar{A}l^3\,\dddot{S}i$. A slag from near Homberg, analyzed by Bunsen has a similar constitution. It occurs in tabular or oblong prisms, of a pearl gray color, glassy or pearly lustre, sometimes translucent; G.=2·876, H.=6. B.B. in the forceps fuses with some difficulty to a grayish slag. Bunsen's analysis afforded

$\dddot{S}i$	$\bar{A}l$	$\dot{C}a$	$\dot{M}g$	$\dot{M}n$	$\dot{K}$	$\dot{N}a$
32·22	17·81	17·35	5·57	2·67	3·05	11·30=99·97

whence the formula $2\dot{R}^3\,\dddot{S}i + \bar{A}l^3\,\dddot{S}i$.

Rammelsberg has analyzed and described some slags from Mägdesprung in the Anhalt Hartz,‡ where the ores are spathic and specular iron and limonite. He obtained for the formula $\dot{R}^3\,(\dddot{S}i, \bar{A}l)^2$, adopting Scheerer's view that $3\bar{A}l$ replace $2\dddot{S}i$. This is similar to some aluminous pyroxenes. The crystallized and glassy slags gave the same composition. Both dimetric (Humboldtilite-like,) and augitic crystallizations occur at Mägdesprung, and it appears probable that the two are a case of dimorphism. Von Kobell formerly wrote for the formula of Gehlenite $\dot{R}^3\,(\dddot{S}i, \bar{A}l)^2$; and this is analogous to the formula of pyroxene.§

* The special formula above given is correct only for analysis 2. The general formula for the whole may be written $(\dot{R}^3, \overline{R})\,\dddot{S}i^2$, with which the results accord very closely, as Hausmann shows. It is the formula of the Augite Section of Silicates, and includes pyroxene and beryl, these species being dimorphous. The slags are very near *Wichtyne* in composition, which the writer has placed in the Augite Section and may be the same species; the formula for Wichtyne is the same as for No. 2, viz. $\dot{R}^3\,\dddot{S}i^2 + \overline{R}\,\dddot{S}i^2$; it differs mainly in containing protoxyd of iron in place of lime. The slags may therefore afford evidence that *Wichtyne* is hexagonal in crystallization.—J. D. D.

† It is also of interest to compare the form with that of Nepheline. In Nepheline O : $\frac{1}{2}$=166° 25$\frac{1}{2}$′, which differs 18$\frac{1}{2}$′ from the inclination of the pyramidal plane in the slag. It may be also noted that O : $\frac{1}{2}$2 in beryl is 165° 59$\frac{1}{2}$′.—J. D. D.

‡ Poggendorff's Annalen, 1848, lxxiv, 95, and Lehrbuch der Chem. Metallurgie.

§ The analyses of the Mädgesprung slags by Rammelsberg gave rather varying ratios. The per-centage of silica was about 40; and the ratio between the oxygen of all the bases and that of the silica, was nearly 3 : 4, instead of 6 : 4, the Gehlenite ratio. Rammelsberg deduces for the ratio between the oxygen of the protoxyds and that of the silica and alumina ($3\bar{A}l$ replacing $2\dddot{S}i$), 1 : 1·7, to 1 : 2·0; 1 : 1·83 is the mean of his results. For his formula, 1 : 2 is taken as the ratio.

Gehlenite has the oxygen ratio for $\dot{R}$, $\overline{R}$, $\dddot{S}i$, 3 : 3 : 4, which gives 6 : 4 or 3 : 2 for the oxygen of the bases and silica; while Humboldtilite has the oxygen ratio 6 : 3 : 9, corresponding to 1 : 1, between the bases and silica, thus differing widely from Gehlenite. The formula of Gehlenite based on the ratio 3 : 2 would be

$(\frac{1}{2}\dot{R}^3+\frac{1}{2}\overline{R})\,\dddot{S}i^{\frac{4}{3}}=\dot{R}^3\,\dddot{S}i^{\frac{4}{3}}+\overline{R}\dddot{S}i^{\frac{4}{3}}$, and that of Humboldtilite $(\frac{2}{3}\dot{R}^3+\frac{1}{3}\overline{R})\,\dddot{S}i=2\dot{R}^3\,\dddot{S}i+\overline{R}\,\dddot{S}i$. There is as yet no good evidence that alumina replaces silica in Gehlenite. Pyroxene, Humboldtilite and Gehlenite appear rather to belong to different sections of silicates, the first, to the Augite, the second, to the Garnet, to which Scapolite pertains, and the third, to the Andalusite section.—J. D. D.

(3.) *Pyroxene Slag from Stolberg in the Hartz.*—The slag is part glassy and part enamel-like. The crystals are yellowish-white, with the lustre weak resinous; they are either 4 to 6-sided prisms or oblique octahedral. The form may be referred to the Augite type. The occurring vertical prism has the angle 115°, corresponding to $i\frac{4}{3}$ ($\infty P\frac{4}{3}$) of pyroxene. Analysis afforded M. Levi:

$\dddot{S}i$	$\bar{A}l$	$\dot{F}e$	$\dot{M}n$	$\dot{C}a$	$\dot{M}g$	$\dot{K}$	S
42·75	9·09	2·77	4·64	38·19	0·74	0·39	0·73=99·30

Taking $3\bar{A}l$ as replacing $2\dddot{S}i$, as Hausmann states, the oxygen ratio for the protoxyds and silica is nearly 1 : 2. The slag is related to those examined by Rammelsberg from Mägdesprung.

(4.) *Feldspar Slag from near Stolberg in the Hartz.*—This slag has a gray color and is part blebby and part crystalline. The crystalline slag has a massive base through which crystals are disseminated. The crystals are thin, long prisms, (about a line thick) and they appear to have a rectangular cleavage; color white, with a resino-vitreous lustre; G.=2·35; H.=6. B.B. fuses rather easily with much intumescence to a white translucent blebby glass. Insoluble in muriatic acid. Analysis gave:

$\dddot{S}i$	$\bar{A}l$	$\dot{F}e$	$\dot{M}n$	$\dot{C}a$
66·2	10·4	1·9	0·1	21·0 = 99·6

giving the oxygen ratio for $\dot{R}$, $\bar{R}$, $\dddot{S}i$, 6·44 : 4·86 : 35·07, which Hausmann observes corresponds nearly to $\dot{R}\,\dddot{S}i+\bar{A}l\,\dddot{S}i^3$, the formula of Orthoclase, from which it differs in containing lime in place of potash.*

(5.) *On the blue color of glassy slags.*—The author adopts the view of Fournet (Ann. des Mines, [4], ii, 57), that the cause of the blue color is not chemical. The green color of bottle glass changes to blue as the glass begins to pass to the condition of enamel or an earthy state; and the color is produced in this incipient stage by a new grouping of the minute particles of the mass. Another kind of blue slag, having lavender-blue to ultramarine or indigo-blue color, is supposed to derive its color (like ultramarine) from the presence of a trace of sulphur, as found on analysis.

After an enumeration of the crystalline silicates in slags, (1, Pyroxene species; 2, Gehlenite; 3, Humboldtilite; 4, Feldspar; 5, Chrysolite; 6, Beryl-like; 7, Chytophyllite,)† the author reviews the characters of the different varieties of slags, which he divides into, (I) the Crystalline Slags; II, Porcelain and Stony Slags; III, Glassy Slags; IV, Porphyritic (1, with the base crystalline; 2, with a porcellanous or stony base; 3, with a glassy base); V, Variolite Slag; VI, Blebby and Scoriaceous Slags; VII, Filamentous Slag; VIII, Capillary Slag; IX, Pseudomorphous Slag.

* This formula corresponds to the oxygen ratio 1 : 3 : 12, while the slag gives 1 : 0·75 : 5·45. The two are nearly alike in having for the oxygen ratio of the bases and silica 1 : 3. But from the shape of the crystals, the absence of the feldspar-habit, as well as the divergence from the feldspar oxygen ratio between the protoxyds and peroxyds (1 : 3), it seems probable that the slag is more nearly related to Edelforsite ($\dot{C}a\,\dddot{S}i$ or $\dot{C}a^3\,\dddot{S}i^2$) than to Feldspar. The formula may be ($\dot{C}a^3$, $\bar{A}l$) $\dddot{S}i^3$, analogous to that of Edelforsite, in which $\dot{C}a^3$ is to $\bar{A}l$ as 4 : 3.—J. D. D.

† See a notice of Hausmann's description of this species in this Journal, vol. xii, p. 394.

Hausmann next compares the specific gravity of certain slags in the stony and glassy states. He then proceeds to apply the subject to Geology, comparing the furnace products with volcanic ejections and other igneous rocks. The minerals of slags, pyroxene, chrysolite, feldspar, humboldtilite, are observed to characterise the Vesuvian lavas. He distinguishes between the volcanic, vulcanoidic and Plutonic rocks, the last never running into glassy forms and abounding in silica; the second kind an intermediate class between the volcanic and the Plutonic.

3. *Temperature of the interior of the Earth.*—An artesian well has been dug at Naples at the Royal Palace under the direction of MM. Melloni and Cangiano. The opening is about 20·98 meters above the level of the sea. After descending 15·2 meters, a volcanic tufa was reached which was 84 meters thick; at the end of December, 1845, they had gone beyond this 60 meters through different imperfectly aggregated volcanic materials, below which the engineer Cangiano expected to find the Jurassic formation, which extends from the Promontory of Castellamare and Nocera. M. Melloni ascertained that the temperature varied from 14°·6 to 15°·5 C. (58·3 to 60° F.) at 30 meters below the surface. It was 18°·3 C. (65° F.) at a depth of 190 meters, which gives an increase of 1° C. for 50 meters (or 1° F. for about 30 yards), a slower rate of increase than has elsewhere been observed, which Melloni attributes to the poor conductibility of volcanic tufa for heat. In a well sunk in the Tuscan marshes, the increase of temperature was 3 times more rapid.—*Atti del Instituto Veneto*, v, 234–237, and *Bib. Univ. de Genève*, *June*, 1854, 177.

4. *A System of Mineralogy, comprising the most Recent Discoveries:* including full Descriptions of Species and their Localities, Chemical Analyses and formulas, Tables for the Determination of minerals, with a Treatise on Mathematical Crystallography and the Drawing of Figures of crystals. Illustrated by six hundred wood-cuts. By James D. Dana, Fourth edition, rewritten, rearranged and enlarged. 2 vols., 320 and 534 pp. 8vo. New York and London: Published by George P. Putnam & Co. 1854.—As a notice of the new edition of this work we cite here the Author's Preface.

In the Preface to the last edition of this Treatise, the classification of Minerals then adopted was announced as only a temporary expedient. The system of Mohs, valuable in its day, had subserved its end, and in throwing off its shackles for the more consistent principles flowing from recent views in chemistry, the many difficulties in the way of perfecting a new classification led the author to an arrangement which should "serve the convenience of the student without pretending to strict science."

A classification on chemical principles was however proposed in the latter part of the volume, in which the Berzelian method was coupled with crystallography, in a manner calculated to display the relations of species in composition as well as form, and prominently "exhibit the various cases of isomorphism and pleomorphism among Minerals." The progress of Science has afforded the means of giving greater precision and simplicity to this arrangement, until now it seems entitled to become the authorized method of a System of Mineralogy. Whether regarded from a physical or chemical point of view, the groupings ap-

The mind uneducated in Science may revolt at seeing a metallic mineral, as galena side by side with one of unmetallic lustre, as blende; and some systems, in accordance with this prejudice, place these species in separate orders. Like the jeweller, without as good reason, the same works have the diamond and sapphire in a common group. But it is one of the sublime lessons taught in the very portals of Chemistry, that nature rests no grand distinctions on lustre, hardness, or color, which are mere externals, and this truth should be acknowledged by the Mineralogist rather than defied. Others, while recognizing the close relations of the carbonates of lime, iron, zinc and manganese, (calcite, spathic iron, smithsonite and diallogite,) or of the silicates of lime, iron, manganese, (wollastonite, augite, rhodonite,) are somewhat startled by finding silicate of zinc, or silicate of copper among the silicates of the earths or of other oxyds. But the distinction of "useful" and "useless," or "ores" and "stones," although bearing on "economy," is not Science.

The advantages which the arrangement of the last edition afforded those interested in mining and metallurgy, is secured in the present volumes by an index to the useful ores, in which their distinctive characters and their relative importance in the Arts are mentioned, and references are given to the pages where the full descriptions are to be found.

During the four years since the appearance of the last edition, the Science of Mineralogy has increased in species from 625 to 660; and this notwithstanding the bankruptcy of some 45 of the number. The important work of Rammelsberg on Chemical Mineralogy, has been continued in a fifth Supplement, issued in 1853. A similar review of the Progress of the Science by Dr. Gustav Adolph Kenngott, conducted with like thoroughness, though with less criticism, has appeared in Vienna, and already two large volumes have been issued, one reviewing the Science for the years 1844 to 1849, the other, for 1850 and 1851. During this period also, Professor Gustav Rose has published his Krystallo-chemische Mineral-System (1853); Professor von Kobell, a work on Mineralogical Nomenclature (1853), and a new edition of his excellent Tables for the Determination of Minerals, (1853); Dr. Franz Leydolt and Professor Adolf Machatschek, of Vienna, their Elements of Mineralogy based on the system of Mohs (1853); Dr. Kenngott of Vienna, "Das Mohs'sche Mineralsystem" (1853), and also a portfolio of plates of figures for the construction of Models of crystals (1854); Professor Quenstedt, of Tubingen, the first part of a Treatise on Mineralogy (1854); Dr. C. F. Naumann, a revised edition of his invaluable Elements of Crystallography (1854); Dr. Friederich Pfaff, of Erlangen, Elements of the Mathematical Relations of Crystals (1853); [illegible] H. Schröder, of Clausthal, Dr. Rammelsberg of Berlin, and Jos. Pecirka of Prague, smaller Manuals on the same subject, (1852, 1853); Dr. J. Zimmermann of Stuttgard, a small "Taschenbuch der Mineralogie" (1852); Nicolai von Kokscharov, the able Crystallographer of St. Petersburg, the first numbers of his "Mineralogie Russlands," in quarto, (1853, 1854); H. J. Brooke and W. H.

Miller, a new and original Treatise under the title of Phillips's Mineralogy (1852); C. F. Plattner, an enlarged edition of his extended Treatise on the Blowpipe (1853): besides the great work of Dr. Gustav Bischof, on Chemical and Physical Geology, begun in 1846, now numbering 2950 pages, (the last issue in 1853), and yet wanting another part to be complete; also G. H. Otto Volger's Essays on the Development of Minerals, (Studien zur Entwicklungsgeschichte der Mineralien,) as the basis of Scientific Geology and a rational Mineral Chemistry, (Zürich, 1854); and von Waltershausen's Treatise on the Volcanic Rocks of Sicily and Iceland. Moreover, various valuable papers have been issued in Scientific Journals and Transactions abroad, by Haidinger, Rammelsberg, Breithaupt, Scheerer, von Kobell, Rose, Bunsen, Hermann, von Rath, Hausmann, Sandberger, Wöhler, Baer, Kenngott, Schabus, Kokscharov, Scacchi, Meneghini, Delesse, Damour, Deville, Descloizeaux, Senarmont, Chapman, Mallet, Scott, Percy, and other able investigators. In this country have appeared Part I. of the third edition of Prof. C. U. Shepard's Mineralogy (1852); Foster & Whitney's Report on the Geology and Mineralogy of the Lake Superior Region (1851 and 1853); and J. D. Whitney's Mineral Wealth of the United States (1854).* Moreover, Dr. J. Lawrence Smith and G. J. Brush, have labored with important results in American Mineralogy, clearing away many doubtful species; and other researches have been published by T. S. Hunt, F. A. Genth, J. C. Booth, J. D. Whitney, C. U. Shepard, J. W. Mallet, W. P. Blake, M. H. Boye and T. H. Garrett.

Of all these publications, Bischof's "Lehrbuch" stands first in importance. Mineralogy was well nigh a lifeless Science, having only powers of increase by accretion, like the objects of which it treats,—the addition of a new Mineral now and then being the great event of interest in its progress. Bischof, by his elaborate researches and profound views, has given it a new impulse. He makes every analysis of a Mineral an important element in the study of Mineral history, showing the necessity of their multiplication, and well exposing the leanness of Chemical formulas when given as a substitute for analyses. The associations and collocations of Minerals, their changes from exposure to atmospheric and other agencies, and even the infinitesimal ingredients in their constitution, are all made to bear on the question of the origin and progress of Mineral and Rock Formations. A Mineral species is shown to have a history of its own,—its perfect state, its liabilities to alteration and decay, its successive changes, and again its renovation or its metamorphosis into a new species. These views taken in their wide extent, constitute the proper basis of the Science of Geology, and should have their full exposition in a work on that Science. But the elements of the subject are with propriety indicated in a Mineralogical Treatise. While dwelling with deserved emphasis on the researches of Bischof, we should not forget that others have labored in the same department, prominent among whom, are Haidinger, Volger, Breithaupt, Blum, Bunsen and Delesse.

* Logan's Reports on the Geology of Canada, 1849–1853, should here be added, as they contain much that is valuable in Mineralogy as well as Geology.

The work next in importance, more especially in its bearing on the crystallization of Minerals, is the "Elementary Introduction to Mineralogy," by Brooke & Miller. It stands preëminent for its original measurements, and its thorough revision of the angles of Crystals, and will remain a permanent source of information on these points.

In the preparation of the present edition, the author takes pleasure in making special acknowledgements to the work of Bischof, for facts and principles relating to the Chemistry of the alteration of Minerals; to Rammelsberg's Supplement to his Chemical Mineralogy, a work whose earlier parts contributed largely to the preceding edition of this Treatise; to Kenngott's and Kokscharov's publications; and to the critical observations in the "Mineralsystem" of G. Rose. Frequent use has also been made of the work of Brooke & Miller, in the crystallography of the species, from which the angles and planes of crystals have often been cited. The various Scientific Periodicals of Russia, Germany, Italy, France and Britain, some of them down to June last, have been searched for their facts, and every effort has been made to post the work up to the day of publication.

American Mineralogy owes much to the careful revision it has received at the hands of Messrs. Smith & Brush; and the author would express his special personal obligations to each of these Chemists. From Dr. F. A. Genth, of Philadelphia, he has derived generous aid both in suggestions and results of researches. Mr. T. S. Hunt has kindly contributed several new analyses throwing much light on the minerals of Canada; and valuable observations and analyses have been received from J. D. Whitney and Professor Booth. Many and various have been the favors, in the way of new facts, opinions and recent discoveries, which the author owes to Mr. Louis Sæmann of Paris. He is also largely indebted to Robert P. Greg, Jr., of Manchester, England, for information respecting the Mineralogy of Great Britain, liberally furnished from a work by him and W. G. Lettsom, now in the press. * * * *

In the preparation of this edition, the subject of Crystallography has been revised and simplified. A system of notation for the figures of crystals, both brief and simple, has been adopted; and many new and original figures have been introduced. The homœomorphous relations of mineral species have been worked out with considerable care, in order to arrive at their true fundamental forms, and trace the bearing of the subject on their composition and classification. The Table of atomic weights has been corrected according to the most recent results, and the percentages of the formulas have been recalculated to correspond with it. The subject of pseudomorphs is treated at some length, and along with the descriptions of the species, a paragraph is devoted to the altered forms which each presents. These changes, together with the remodeling of the classification, and the large additions throughout, render the Treatise more properly a new work, than a revised edition.

5. *Lanthanite.*—Prof. J. LAWRENCE SMITH, as an addition to his account of this species, on page 378, mentions that the specific gravity is 2·843, a little higher than the determination of W. P. Blake. Prof. Smith freed the mineral from air by boiling it and then using the air-pump.

III. Botany and Zoology.

1. *Victoria Regia; or the Great Water Lily of America; with a brief account of its discovery and introduction into cultivation; with illustrations by William Sharp, from specimens grown at Salem, Massachusetts, U. S. A.;* by John Fisk Allen. Boston, 1854.—This regal Water-Lily, whose gigantic size baffles the ordinary resources of the cultivator, was, as is well known, first raised and flowered in this country by Caleb Cope, Esq., of Philadelphia, who, aided by our sunnier climate, succeeded in giving the plant an ampler development than it had attained in England. Mr. Allen of Salem, a gentleman of great enthusiasm and success in cultivation, has the honor of having followed this example, and of flowering the *Victoria* last summer, for the first time in New England. By a skillful arrangement, Mr. Allen was able to bring the plant to great perfection in a tank of moderate size, the borders of which were enlarged from time to time as the successive leaves increased in magnitude. This year new plants have been raised in an ample tank constructed for the purpose, and already its huge blossoms are beginning to appear. Not content with thus rivalling the royal and princely conservatories of Kew, Syon, and Chatsworth, in the cultivation of the great Water Lily of the Amazon, Mr. Allen has now emulated Sir Wm. Hooker's magnificent work, illustrating the *Victoria* by colored figures mostly of the natural size, and in the highest style of art. Mr. Allen's treatise is an equally sumptuous work, of about the same gigantic size, the largest elephant folio, and the paper, typography and colored plates will compare favorably with the English work, except in the want of botanical details and dissections. Unsparing of expense, Mr. Allen has given six plates; while Sir Wm. Hooker has only four. And in 16 pages of text of the same huge dimensions, Mr. Allen has given a condensed abstract of the botanical history of the plant and of its introduction into cultivation in England, followed by a more detailed account of its cultivation into this country by Mr. Cope and himself, and a description of all its parts, as exhibited in the individual under his attentive observation. A. G.

2. *Plantæ Junghuhnianæ: Enumeratio Plantarum quas in Insulis Java et Sumatra detexit Fr. Junghuhn.* Fasc. III. (Leyden, 1854. pp. 271–394. 8vo.)—The excellent Dutch botanists are now very active, especially Prof. Miquel, Prof. De Vriese (who is still occupied with the elaboration of the Laurineæ for De Candolle's Prodromus), and Drs. Dozy and Molkenboer, who have worked up the *Musci Frondosi* (the Div. *Acrocarpi*) of Junghuhn's collection in the present fasciculus. Some new collaborators appear in the work: Among them M. Buse, who has elaborated the *Gramineæ*, and proposed several new genera; and Dr. Bruyn, who has given the *Polygonaceæ*. There are also the *Lycopodineæ* by Spring, and several small orders by Miquel. A. G.

3. *Steudel's Synopsis Plantarum Glumacearum:* fasc. III. comprises most of the *Agrostideæ* (with 171 species of Agrostis!), the *Arundinaceæ*, *Pappophoreæ*, *Chlorideæ*, and the greater part of the *Avenaceæ*. The work will be useful as bringing together the vast number of Grasses published since Kunth's Enumeration; but is of no more critical value than that work. A. G.

4. *Seemann's Botany of the Voyage of the Herald.*—Part V, finishes the *Compositæ* of the Flora of the Isthmus of Panama, and continues this flora down to the *Piperaceæ*, which, as well as the *Artocarpeæ*, are elaborated by Professor Miquel of Amsterdam, the able monographer of these families. Mr. Seemann has reëstablished the order *Crescentiaceæ*, with a new character, and referred to it nine genera and about thirty known species; all natives of tropical and subtropical America and Africa. His views were expounded last winter in a paper read before the Linnæan Society of London, when he divided the order into two sections, the *Crescentieæ* and the *Tanæcieæ*. In the September No. of Hooker's Journal of Botany, this author has given an able revision of the principal genera and species of the *Crescentieæ*. Among the plates are fine illustrations of the Ivory-Nut Palm, the *Phytelephas macrocarpa* of Ruiz and Pavon. A. G.

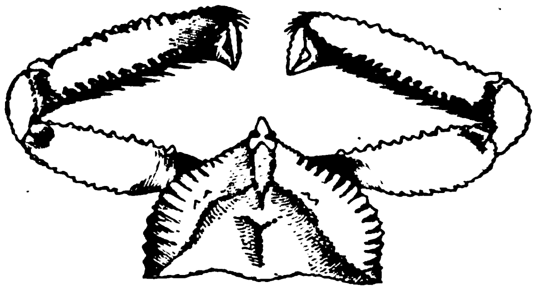
The *Pandaneæ*, or Screw Pines, are being investigated by Professor De Vriese of Leyden, who is about to publish Nova Genera Pandanearum, with illustrations. Meanwhile, some remarks on the family, and characters of two new genera, read before the Academy of Sciences at Amsterdam, are published in Hooker's Journal of Botany for September. A. G.

Three Botanists of considerable distinction have died during the past summer, namely, Dr. Fischer of St. Petersburgh, long the Director of the Imperial Botanic Garden, who deceased on the 5th of June, aged 73. M. Moricand of Geneva, the author of *Plantes Nouvelles d'Amérique*, also well known as a conchologist; he died June 26th. Philip Barker Webb, Esq., a distinguished English Botanist, long resident abroad, chiefly in Paris, where he will be greatly missed and deeply regretted, not only by all the French Naturalists, but by a wide circle of friends and correspondents, who will long remember his generous hospitality and kindness. He died suddenly, of cholera, on the 31st of August. Mr. Webb was an excellent classical scholar as well as a general naturalist. His most extensive work is the *Histoire Naturelle des Iles Canaries*, written in conjunction with M. Berthelot; almost every page of which reveals something of the vast and varied knowledge of Mr. Webb. Among his publications are the *Otia Hispanica*, a folio volume of plates and descriptions of Spanish plants; the *Spicilegia Gorgonea*, an account of the botany of the Cape de Verd Islands, contributed to Hooker's Niger Flora; and the *Fragmenta Florulæ Æthiopico-Ægyptianæ*, recently published. Mr. Webb had accumulated one of the largest private herbaria in the world. This, we learn he bequeathed to his "dear friend, the Duke of Tuscany." Its acquisition will render the Florentine herbarium,—the foundation of which was recently laid by his friend, the zealous Parlatore—one of first-rate importance.

To this list should be added the name of the late King of Saxony (recently killed by a fall from his carriage), one of the Foreign Honorary Members of the Linnæan Society of London, a botanist of much zeal and no mean acquirements. A. G.

5. *Description of a New Species of Cryptopodia from California;* by James D. Dana.

Cryptopodia occidentalis. Rostrum parvulum, subspiniforme. Carapax postice rectangulatus, angulis postero-lateralibus acutis, margine postico rectiusculo, transverso, margine antero-laterali arcuato, lateralique denticulatis. Chelæ prælongæ, bene trigonæ, angulis tenuibus inæquè subspinulosis, manu latitudinem carapacis longitudine æquante, superficie supernâ planâ. Pedes 8 postici tenuiter compressi, articulis 3tio 4to 5toque bialato, tarso tenui, 4-alato. Long. 1″.



Cryptopodia occidentalis.

The carapax has a triangular sharp-edged prominence on the medial line behind the middle; a doubly curving subtuberculate ridge extending to the posterior angles; a small denticulate ridge, extending from near the middle to either side of the base of the beak, the two enclosing a narrow area. The breadth of the carapax is $1\frac{1}{4}$ inches, the length of the hand $1\frac{1}{8}$ in.; of the carpus 5 lines; of the arm 1 inch. The animal appears to have had when alive a villous coat over the carapax and upper surface of the hand. From Monterey, where it was obtained by Wm. Rich, Esq.

IV. Astronomy.

1. *New Planets,* (Astron. Journ., 75.)—Mr. J. R. Hind announces the discovery of another asteroid, on the 22d of July, at $11^h\ 25^m$, at Mr. Bishop's Observatory in Regent's Park. It appears as a star of the 9·10 magnitude. Its position at that time was R. A. $21^h\ 10^m$ and N. P. D. 106° 20′. On the 1st of Sept., Mr. James Ferguson of the Washington Observatory discovered a new asteroid near *Egeria*, and nearly equaling it in brightness. The next day it preceded *Egeria* 24^s, and was 52″ farther North. Professor Reuel Keith has computed the following elements from the Washington observations of Sept. 2, 6, and 10. They satisfy the middle place perfectly.

Mean Equinox 1854.0 Epoch, Sept. 2·721 Greenwich M. T.

Mean anomaly,	13° 36′ 33″·3
Long. perihelion,	352 5 50 ·6
" asc. node,	33 29 21 ·7
Inclination,	22 39 13 ·6
Angle of excentricity,	4 22 30 ·2
Log. semi-axis major,	0·469530
" mean daily motion,	2·845712

2. *New Comet,* (Astron. Journ., 76.)—A new comet was discovered on the 13th of Sept. by Mr. Robert Van Arsdale at Newark, N. J. Its position, Sept. 3d, $9^h\ 50^m\ 30^s$ was R. A. $8^h\ 21^m\ 36^s$, and Decl. +74° 30′.

V. Miscellaneous Intelligence.

1. *Correspondence of M. Jerome Nicklès, dated Paris, Sept.* 2, 1854.

Death of Dr. Lallemand and of Melloni.—Science has experienced a great loss in the deaths of Dr. Lallemand and the Physicist Melloni. The former died at the age of 65 years, leaving a great void in medico-chirurgical science, of which he was one of the most illustrious representatives. Melloni died suddenly from an attack of cholera, when he was just giving his last stroke to a work on electrostatic induction. His death took place on the 7th of August, while he was living on a small farm where he had retired since the king of Naples had taken from him the direction of the Naples Observatory and deprived him of the means of making researches. His age was 56 years. We have not had time to collect together an account of the checkered life and labors of these eminent men, and will return to them again in our next communication.

Weights and Measures.—A Turk, M. Bilizidkdji, has called the attention of the Academy of Sciences to the many defects in the present system of weights and measures used in Turkey, representing the necessity of establishing there a uniform system cerresponding as nearly as possible with the metric of France. The weights and measures in Turkey vary not only from Province to Province and town to town, but also according to the different professions, and the nature of products. It is nearly as it was in France before the Revolution. On the Report of General Morin, the Academy expressed the desire that the Ottoman government, as soon as the war ceased or gave leisure for it, should take up the subject and establish a plan on the metric system.

We observe in this connection that two governments have recently adopted this system, the Republic of Mexico and that of New Grenada, and orders were given to M. Silbermann to supply the standards of the system, (a metre, kilogram and liter.) These units have been finished by the late Gambey; and before delivering them over they were compared with the fundamental standards by Silbermann in the manner explained in this Journal (January and May, 1853, and page 388 of the present volume,) and verified nearly to the hundredth of a milligram or a millimetre. There are now 17 of these units at the Conservatory of Arts and Trades; they are intended for those governments that order them, or that offer their own standards of weights and measures in exchange. A score of setts have already been distributed, to the United States, Spain, different states of Germany, and Italy. England and Austria are now the only nations of the first order that do not possess them. Prussia and Russia received them long since.

Researches on Colored Impressions produced by the Chemical action of Light.—It is more than six years since M. Edmond Becquerel succeeded in preparing a surface chemically impressible to light, such that it would take the color of the luminous rays which fell upon it. He has recently returned to the subject and perfected his methods; and he now describes with details his processes which enable him to realize a species of artificial retina which is of great sensitiveness, and is acted

upon only by the visible rays of the spectrum. These rays preserve their shade of color with only a slight modification; the orange rays, for which the luminous intensity is at its maximum, are the first to impress their image, but he has not succeeded in fixing them. The sensitive material used is a chlorid of silver containing less chlorine than the ordinary chlorid, and ôften found mixed with the latter.

The method most successful in preparing this sensitive chlorid, consists in decomposing rapidly by an electric current a solution of chlorhydric acid in water and causing the chlorine to come in contact with the plate of silver while the latter is placed in contact with the positive pole of a battery. As it is impossible to enter into the details of these important researches, we give only these general indications of his method.

Protection against Hail.—The second volume of the works of Arago have called attention to several points in Meteorology, among which is the subject of hail and the means of protecting fields from this evil. In the chapter which he devotes to this important subject he states that in 1847, two small agricultural districts of Bourgogne had lost by hail crops to the value of a million and a half francs. Certain of the proprietors from the neighborhood went to consult Arago on the means of protecting themselves from like disasters. Resting on the hypothesis of the electric origin of the hail, he suggested the discharge of the electricity of the clouds by balloons communicating by a metallic wire with the soil, as mentioned in a preceding number of this Journal (Jan. 1853, p. 111). These projects however were not carried out; and in view of the doubts as to the electric origin of hail, he proposed to investigate the subject anew. He had not the time to bring out any results; but he persisted in believing in the effectiveness of the method proposed.

Another subject is discussed in this volume. Arago enquires whether the firing of cannon can dissipate storms. He cites several cases in its favor and others which seem to oppose it; but he concludes by recommending it to his successors. Whilst Arago was propounding these questions, a man not conversant in Science, the poet Méry, was collecting facts supporting the view; and since the publication of the second volume of Arago's work, he has been led to give his results to the public. In a remarkable pamphlet entitled "Paris futur," he concludes strongly on the efficaciousness of the firing of cannon in dissipating storms, and mentions numerous observations in support of it. He says that his attention was called to the subject in 1828, while an assistant at the "Ecole de tir" of Vincennes. Having observed that there was never any rain on the morning of the exercise of firing, he was led to examine the annals of military and revolutionary science, and he found there, as he says, facts which justified the expressions which became common, such as "Le soleil d'Austerlitz," "Le soleil de Juillet" upon the morning of the revolution of July, and he concluded by proposing to construct around Paris 12 towers of great height which he calls "tours imbrifuges," (imbrifugal towers,) each carrying 100 cannons, which should be discharged into the air on the approach of a storm. Although this pamphlet was the offspring of a man of imagination instead of a scientific man, it has attracted attention, giving occasion to some

ent month (August); to-day the subject is forgotten; and why has it deserved to pass so soon into oblivion? It was in consequence of a negative fact. The 14th of August was a fine day. On the 15th, the *fête* of the empire, the sun shone out, the cannon thundered all day long, fire-works and illuminations were blazing from 9 o'clock in the evening. Every thing conspired to verify the hypothesis of M. Méry, and chase away storms for a long time. But towards 11 in the evening a torrent of rain burst upon Paris in spite of the pretended influence of the discharge of cannon, and gave an occasion for the mobile Gallic mind to turn its attention in other directions.

Diseases of Plants.—Communications on the cholera, the disease of the vine, of the potato, and of plants in general, have multiplied rapidly in consequence of the prizes that have been offered. Memoirs from Germany continue to come in numbers; but they have not yet presented any fact worth mentioning. A first Report on this subject was recently made by the botanist M. Montagne, who has studied the various communications with care. None are satisfactory; the theories are vague and uncertain; the methods of prevention are in general ridiculous or impracticable; and the subject is still left for the future to solve.

This is not the opinion of the "Société d'Encouragement" who have more seriously verified the facts, and according to whom, important results have already accrued to agriculture. If we cannot yet avoid the disease of the vine altogether, we know at least how to cure it; this being the result of experiments made on a very great scale at Thomery in Bourgogne where the vineyards were much infected by the disease. Injections of sulphur skilfully made, have saved the crop, and enabled the Commune of Thomery alone to send to Paris in 1853 more than a million kilograms of the white grape, ("chasselas") of excellent quality. The sulphur was applied three times in the year; it is reduced to powder, and by the aid of a bellows of peculiar contrivance it is thrown upon the vine; the application of it is made in different directions in order that the sulphur may be brought in contact with the whole surface of the plant. The first application is made when the shoots are several centimeters long, the second after flowering, the third before the grape reaches maturity. The operation succeeds best in the sun, the time taken being always from noon to 2 P. M., and 20 to 23 kilograms of sulphur are employed per hectar. In hot-houses the method is more simple, as it is sufficient to spread the sulphur on the heating tubes, when the vapor rises and answers the purpose desired.

While admitting that much remains to be accomplished, the Society has done justice to several works treating of the vine disease; that most highly honored, is the treatise of a modest agriculturalist, *M. Gontier*, who suggested the use of sulphur, an idea which he put in practice, and for which he contrived a peculiar kind of bellows. The subject is up for a prize of 20,000 francs, next year.

Dimorphism.—In the number of this Journal for May, of the present year, at page 414, I have spoken of one of my papers having for its object the determination of the influence which the medium may

exert on crystals in process of formation. I showed that by varying a saline solution by means of a substance foreign to the definite compound, we may by this means vary the molecular forces which preside in crystallization and cause a change in the crystalline type of the substance, in case the substance is susceptible of becoming dimorphous. M. Pasteur, Professor of Chemistry at the Faculty of Sciences at Strasburg, has obtained remarkable results of this nature, with dimorphous substances presenting left and right (or non-superposable) hemihedral crystals; using the two hemihedral forms of one and the same compound, as the neutral tartrate of ammonia, spoken of in preceding numbers of this Journal. These two forms belong to the system of the right rhombic prism (trimetric), and differ only in their opposite hemihedrism. But M. Pasteur has obtained with each of these two forms a second form, wholly unrelated to the first, and crystallizing in the oblique rhomboidal system [monoclinic ?], which make in all four hemihedral forms not superposable obtained with one substance.

To obtain this result, it is only necessary to take a solution of one or the other variety of this trimetric tartrate, the right or left, and add a small quantity of neutral malate of ammonia; the malate does not appear to enter into the compound; it exerts an action of presence which changes the condition of equilibrium of the molecules. M. Pasteur calls this new kind of hemihedrism, tetartohedrism; and the forms, tetartohedral.

Coloring matter of Flowers.—This question has been studied by several chemists, and still it is beyond doubt, one of the most obscure subjects in vegetable chemistry. Botanists have long admitted that flowers owe their color to two coloring principles, a blue, called *cyanic*, and the other yellow called *xanthic*. For some time the blue color of blue flowers was attributed to the presence of indigo; but M. Chevreul showed that this blue is always reddened by acids, which fact set the indigo theory aside.

MM. Frémy and Cloez have isolated the blue principle and they call it *cyanine*. To obtain it, they treat with boiling alcohol the petals of the violet or iris, until the flower is colorless and the liquid takes a fine blue tint. This tint disappears soon, but reappears on evaporating the alcohol in the air; on pouring water into the product of this evaporation, a resinous substance separates; the coloring matter remains in solution, and may be precipitated by acetate of lead; the precipitate is green; it is washed with a large amount of water and treated with sulphuretted hydrogen which removes the lead and leaves the cyanine in solution. It is gently evaporated in a water-bath, absolute alcohol is added, and then the cyanine is precipitated in bluish flocks by ether.

This coloring matter is uncrystallizable; acids turn it red, alkalies green; it combines with lime, baryta, etc; sulphurous, phosphorous and other acids discolor it; it resumes its blue color through the presence of the oxygen of the air.

The *coloring* material of roses, peonies, some dahlias, &c., is a modification of cyanine; the vegetable juices have an acid reaction (which changes the blue cyanine to red), while the juices of blue flowers are neutral. In the presence of alkalies, the rose color becomes first blue and then green.

very soluble, *xantheine*; the former is analogous to the resins, and along with cyanine it produces in flowers an orange color, a scarlet, and a red. The xantheine combines easily with oxyds; alkalies change it to brown of a very rich color, and of considerable strength; but acids cause the brown color to disappear.

These are the three principal coloring ingredients of flowers. M. Filhol, Professor in the Faculty of Sciences at Toulouse, who has studied this subject, confirms in general the results of MM. Frémy and Cloez. He has however found that these coloring matters may be disguised or even destroyed by mixture with the juices of white flowers.

M. Pepin, "Chef des cultures" at the Jardin des Plantes of Paris, has made some curious observations, on the change of color which culture produces in flowers. He has found that cultivated annuals experience a change of tint more promptly than perennial plants, for each year they are renewed through the seeds. Such a change is however sometimes produced in biennials and perennials, and rarely ever in ligneous species.

The annual plants of Chili, Texas and California, have a strong tendency to produce varieties with white flowers, especially when their flowers present either of the primary colors, red, yellow or blue. The same is true of many other species introdued into France. Thus the *Clarkia pulchella* and *C. elegans* whose flowers have a violet tint, have produced white and rose-red flowers; the *Gilia*, blue and tri-colored; the *Leptosiphon* having red flowers, has produced pure white. The varieties with a white color are first produced, and afterwards the variegated.

Various Memoirs.—Among the more important papers read before the Academy of Sciences during the last two months, we must first notice, an important memoir by M. Daubrée, Professor of Mineralogy in the Faculty of Sciences of Strasburg, on *the Artificial Production of Minerals of the family of silicates and aluminates, by the reaction of vapors on rocks.* By the reaction of chlorid of silicium in the state of vapor, and at the temperature of red heat on the bases which enter into the constitution of rocks, he obtains by double decomposition and according to the nature of the base, *crystallized quartz, chrysolite, kyanite, idocrase, garnet, zircon, etc.* A memoir of MM. Malaguti and Durocher, on *the resistance of hydraulic lime and cements to the destructive action of seawater.* A memoir of M. Bechamp, Professor at the School of Pharmacy of Strasburg, on *the action which chlorid of iron exerts on different nitrated substances*, in continuation of a work mentioned in a former number of this Journal. *New observations on butylic alcohol*, by M. Wurtz,—the discovery of which is mentioned in the number of this Journal for Jan., 1853, p. 112. *New observations of caprilic alcohol*, by M. Bonis, a subject also alluded to in a former number. M. Deville communicates *new processes for preparing Aluminium*, and he accords in these processes with those which M. Bunsen has recently described in Poggendorff's Annalen. M. Melloni, just before his death, sent in the first part of his *Researches on Electro-static induction;* and M. Abria, Professor in the Faculty of Sciences at Bor-

3. The greater part of the heat is utilised, which was before carried off by the steam and gas and totally lost.

4. The use of metallic furnaces renders it easy to multiply the heating surfaces, and at little cost.

5. The heating is regular, the temperature very equal, and the products obtained are uniform.

6. The best heating effects are secured by the arrangement for bringing the hot air under the grating."

The committee hence recommend an appropriation to enable the powder establishment of Esquerdes to make these arrangements. The appropriations have been authorized. We propose hereafter to speak of the fabrication of sugar and of distilling by this method.

Stereoscopy.—The invention of the refractive stereoscope has quite generally been attributed to Sir David Brewster, especially in France. A recent writer has corrected the error. The Abbé Moigno, in giving an account of a visit to England, in his Journal, Le Cosmos, observes that he saw in the hands of Mr. Wheatstone a letter written by Brewster, dated September 27, 1838, containing besides other things, the sentence, "I have also stated [to Lord Rosse] that you promised to order for me your stereoscope, both with reflectors and prisms." The stereoscope by refraction, says M. Moigno, as well as that by reflection, is Wheatstone's. The refracting stereoscope invented by Sir David, is a form in which the two prisms are the halves of a lens.

Photography—Painting transparent photographic images.—The coloring of photographic portraits has often been attempted; but the photograph is obliterated in the process, and after all only an ordinary painting is obtained.

M. Minotto, and also MM. Soulier and Clouzard, have succeeded in this art, by applying the color under the image. This method of coloring was used in 1824 at Strasburg, with lithographs, under the name of "oleocaleographie" and "lithrochromie." But it is especially applicable to photographs on glass, paper, tissue, and generally all transparent substances.

Photographs of monuments in Judea.—The state of art among the Jews has usually been lightly spoken of, and archeologists will not admit that they were artists. When M. de Saulcy, a French archæologist, returned from Palestine with a portfolio of crayon sketches representing the monuments of the Jews, his sketches were treated with a smile of incredulity. A photographer, M. Saltzmann of Colmar, has recently confounded the sceptic by taking the sun as his collaborator. His plates confirm the designs of M. Saulcy. They consist of 200 photographs, 50 of Jewish subjects, the rest of Roman, Byzantine, Latin, Arabic and Turkish monuments in Jerusalem.

New Collodion.—In the body of a silk worm just about to make its cocoon is found an organ full of the material which is to become silk. M. Legray has extracted from it a substance equal to albumen and collodion for photographic proofs. He proceeds thus:—He puts in a porcelain capsule the organs in question taken from 50 worms: 200 grams of distilled water are poured on it, containing 4 p. c. of carbonate of soda; the capsule is then heated, while agitating with a glass rod; after 10 to 20 minutes of ebullition, the small bags empty

themselves; they are then transferred to a piece of fine linen and pressed. The liquid is collected on glass and left to evaporate, when a pellicle forms like that of collodion. It should be used within 24 hours, as it afterwards becomes spongy and insoluble in water, alcohol and ether.

Impression by heat, or Thermotypy.—This process proposed by M. Abate is very simple, and is based on the destructive action exerted by chlorhydric acid on organic substances. Suppose for example we have a slice of wood of which we wish a faithful impression. The wood is exposed for some minutes, to the action of cold vapors of chlorhydric or sulphuric acid, or it is wet lightly with either of these acids diluted. The slice of wood, wiped with care, is placed on a piece of paper, cotton cloth, or white wood, in a press, and a blow struck. The impression is at first invisible; but on exposing it to a strong heat, it gradually appears, and exhibits a perfect picture of the wood. This operation may be repeated indefinitely. For oak, maple, hazel, &c., the picture is of the color of the wood; but for mahogany, rose wood and many others, the color is modified, and if a perfect colored picture is desired, it should be taken on a previously prepared tint of the required kind.

It occurs to us that, by taking an impression on a plate of zinc, tin, marble or other substance attacked by acids, a negative relief might be obtained, which would serve for reproducing pictures of the impressions. Finally, if the image of M. Abate is obtained with a substance which is a conductor of electricity, it may then be reproduced by electrotypy.

Electric illumination.—There have been recently some attempts made at Paris towards illuminating the bottom beneath water. At the lake d'Enghien, M. Duboscq, the successor of Soleil, performed an experiment of this kind before many competent observers. The electrodes of carbon were placed in a glass globe, being connected with one of Duboscq's regulators, which communicated with the battery by a copper wire covered with gutta percha. The globe submerged to a depth of 5 meters, spread light over a circumference of about 10 meters radius, and it remained constant for two hours, after which the carbon required replacing.

The idea of this process was suggested to Duboscq by an agent of the Company engaged in exploring the bottom of the Mediterranean where the battle of Navarino took place. The diver usually remained beneath the water three quarters of an hour, after which he came up to breathe and rest; his light was an oil lamp, placed on the head of the diver, and fed with air proceeding from his respiration, whence, it was in a variable current and was often extinguished, requiring him to go up and relight. Duboscq's arrangement was devised to avoid these inconveniences. It is light so that the diver may carry it in his hand, and at the same time it is strong and well secured hermetically, to resist a pressure of 50 to 60 meters of seawater. It consists of a cylinder of strong glass secured to a brass foot, and surrounded with a gutta percha sac. The light passes out through a large plano-convex lens, the convexity inward, the focus being so arranged that the rays escape nearly parallel. As the lamp is moveable, the diver walks about with it and places it where he wishes to make any search; and as it is only

necessary to bring the electrodes near one another to light it, the diver need only turn a small screw to continue the light for two hours, which is more than twice as long as he can remain at the bottom.

To illumine the bottom at small depths, Deleuil uses a Fresnel lens, and this is daily in operation in a bathing establishment—the baths of Henry IV, constructed on the Seine in the heart of Paris. The regulator and also the light are 10 meters above the surface of the water, and the light penetrates sufficiently far to enable us to see the swimmer at a depth of 2 to 3 meters, and follow all his movements.

2. *Notice of the "Fountain of Blood" in Honduras.*—The following letter from E. G. SQUIER addressed to B. SILLIMAN, Jr., refers to a remarkable phenomenon in Central America, the details of which are sufficiently given in the letter of Mr. Squier. The bottle of colored liquid which was placed by this gentleman in our hands has suffered the same fate as its predecessors, and its contents were so far changed by decomposition as to preclude all attempts at an accurate examination. The color of the fluid was dark brown, exhaling an offensive odor, and having a sediment somewhat copious in which the microscope detected no distinct forms of organization, although filaments of organic matter were abundant. The most probable conjecture as to the origin of this fluid, appears to be that which refers its color to the presence of some highly colored species of infusoria. A microscopic examination on the spot or a portion of the material in alcohol would easily settle the question. Meanwhile the following facts will be read with interest.

"*My Dear Sir*:—I send you herewith a bottle of a remarkable liquid obtained from what is called "*Mina ó Fuente de Sangre*," Mine or Fountain of Blood, in Central America. The locality is a small cavern, near the little town of Virtud, Department of Gracias, State of Honduras, on the western or Pacific slope of the Cordilleras. It has long been known, not only in its immediate vicinity, but in connection with various superstitious hypotheses, throughout all Central America. Mention is made of it in publications,* dating back more than a hundred years. The following extracts from the '*Gaceta de Honduras*,' of the 20th of February, 1853 will serve to give the essential facts concerning it, so far as they are known:

'*Fuente de Sangre.*—A little to the south of the town of Virtud, Department of Gracias, is a small cavern (*gruta*) which is visited during the day by buzzards and *gabilanes*, and at night by a vast number of large bats (*vampires*), for the purpose of feeding on a kind of liquid which exudes from the rocks, and which has the color, smell, and taste of blood. A rivulet flows near this grot, which is constantly reddened by a small flow of the liquid. A person approaching the grot observes a disagreeable odor, and when it is reached, he sees several pools of the blood, in a state of coagulation. Dogs eat it eagerly. The late Don Rafael Osejo undertook to send some bottles of this liquid to London for analysis, but it corrupted within twenty-four hours, bursting the bottles.'

At my request Don Victoriano Castillanos, a gentleman of an observing turn living not many leagues from Virtud, sent me two bottles of this liquid, largely diluted with water, to avoid the catastrophe which happened to Gr. Osejo, and to all others who had attempted to carry

that the results may not be uninteresting to the readers of the Journal.
New York, May 1, 1854."

3. *Nature doing her own engraving.*—In the fifth volume of the Denkschriften of the Royal Academy of Sciences at Vienna (Mathematico-Natural History Class), there is a paper by Alois Auer, and numerous plates, illustrating a new style of engraving. The plates represent leaves, plants from a herbarium, lace and other objects, and in each case, the object appears to be *on the paper*, the surface being raised and the coloring perfect. The deception is so complete that without a magnifying glass it is almost impossible in one or two instances to be sure that the object itself is not there. The process employed is the following: The pressed plant, or other object, is placed between a plate of copper and one of lead and subjected to pressure; the original thus produces a strong impression on the lead plate. By inserting the requisite colors with a point, in the depressions, a figure colored to nature, with different colors in its different parts, may be obtained at a single printing. From the lead plate copies may be taken by stereotype or galvanism, and copper plates are thus obtained more durable than those of lead. Gutta percha may be used in place of the lead, and by covering it with a deposit from a silver solution, the impression may be used for stereotyping or electrotyping.

4. *Mount Ararat and places in the Caspian Basin.*—Mount Ararat was ascended by Col. Chodzko of Russia in 1850. He found the height to be 15,912 French feet; and for Little Ararat 3852 feet less in elevation. M. Fedoroff found in 1829, 16,069 for the former and 12,232 for the latter. Parrot obtained 16,251 and 12,271 feet.

Lake Goktchaï near Erivan is not less than 5,510 feet (French) above the sea; and on its borders to the south and southeast there are ancient volcanoes 8000, 10,000 and 11,000 feet in height.

The height of the city of Teheran above the sea is 3579 French feet; of Meched 2865; that of the high mountain Schemrunn 12,247 feet; the famous peak of Demavend 18,846 feet, (which Frazer had made 10,000 feet, T. Thomson 14,000, Texier 4548 meters, and Humboldt in his *Central Asia*, vol. iii, 3066 toises.

The *lake Aral* derived its name from the string of islands on the east and north sides, the word in the Kirghis dialect meaning island. The lake is 50 leagues (French) broad and 100 long and about 109 times the surface of the lake of Geneva. The waters have the saltness of Finland, 25 leagues west of Cronstadt. The greatest depth, about 37 toises, is on the west side. It does not contain seals or crabs like the Caspian, nor various large species of fish which occur in that sea.—*Bib. Univ. de Genève, June*, 1854.

5. *Zodiacal Light.*—By letters from Rev. George Jones, U.S.N., now of the Japan Expedition, we learn that he has made numerous observations on the zodiacal light, leading to important conclusions. He remarks that "the light never fails to be seen when the moon or clouds do not interfere," a statement which accords with the observations of the writer. His results will be published on the return of the Expedition.—J. D. D.

6. *Notes on California;* by W. P. Blake, (from a letter to one of the Editors.)—I found a greater number of intruded igneous rocks in the Gold region than I anticipated. They vary in character and are probably of different ages. The prevailing trend is N. to N. 45° W., and when traversing slates they are generally comformable to the bedding. Quartz veins bearing gold are usually found connected with these intrusions and either traverse their mass or constitute a wall between them and the adjoining slates. Quartz veins traversing the slates *conformably* and *obliquely* without any apparent connection with the igneous intrusions are also common.

Some of the auriferous quartz veins are worked with great profit. Of this, I am satisfied from careful examination, and I have many interesting details on this subject.

The elevated placer deposits are very extensive, and are worked with much skill and success.

The numerous exploring shafts sunk in all parts of the country on the tops of the hills, have developed many interesting facts concerning auriferous drift. There is in most places where placer mining is being conducted above the present rivers, a thickness of two hundred feet or more of stratified materials that appear to have been laid in comparatively quiet water. The peculiarities of these deposits are so various and they are so different from those generally known as drift, that a wide field is opened for investigation and many detailed observations will be required, before we can understand the changes that have taken place in this part of the continent during and since the "drift period."

Since I wrote you about the gold and platinum from Port Orford, I have examined several other samples and find that the percentage of platinum is variable, and that iridosmine is generally in large proportion. I have now several ounces of the mixed metals separated from the gold. The grains are very hard but are probably too small to be used in the manufacture of pens.

7. *Homœography.*—A new method of copying pages of a printed work by transfer, invented by M. Edward Boyer in France, is thus named. It is claimed that any book or engraving may be thus copied with little expense, and copies multiplied indefinitely, so that a book, however rare, never need be out of print. It is done rapidly, without injuring the original, and so exactly that the most practised eye cannot tell the difference.

8. *Sketch of the Life and Labors of Dr. Thomas Thomson*, F.R.S., &c., Prof. Chem. Univ. Glasgow, etc., delivered at the opening meeting of the Glasgow Philosophical Society of Nov. 5, 1852, by Walter Crum, F.R.S., Vice-President of the Society.—Dr. Thomson died on the 2d of July, 1852, at 80 years of age, after long service in the cause of science. Mr. Crum gives the following facts showing the part Dr. Thomson took in the promulgation of the Atomic Theory.

"On the 26th of August, 1804, Dr. Thomson went to Manchester, and saw for a day or two much of Mr. Dalton, who explained to him his views on the composition of bodies. He saw at a glance, as he tells us, the immense importance of such a theory, and was delighted with the new light which immediately struck his mind. He wrote down at the time the opinions which were offered, and three years later, when

about to publish the third edition of his System of Chemistry, he obtained Dalton's permission to insert the sketch he had taken, before Dalton himself had given it to the world. The theory was at that time very slenderly supported by facts, for chemists possessed few experiments which could be considered as even approaching to accuracy. Up to this time when Thomson published the sketch, he seems to have been Dalton's only convert. Perhaps no other chemist had taken the trouble to listen to it, if we except Dr. Henry of Manchester, who was Dalton's frequent visitor, but there is no probability that even he at so early a period accepted the theory, for he speaks of it, so late as 1810, in rather doubtful terms, in the sixth edition of his "Elements."

Thomson's paper on the oxalates, read to the Royal Society in 1807, contained the first direct example of the application of the Daltonian theory to supersalts. He there shows that oxalic acid unites with strontian as well as with potash in two different proportions, and that the quantity of acid combined with each of these bases in their superoxalates, is just double of that which saturates the same quantity of base in their neutral compounds. During the same year Dr. Wollaston read his famous paper on the oxalate, binoxalate, and quadroxalate of potash, and he commences it with a relation of what Thomson had already done. He states that he had remarked the same law to prevail in various other instances of superacid and subacid salts, and that he had intended to pursue the subject so as to learn the cause of so regular a relation; but that such a pursuit was rendered superfluous by the appearance of Dalton's theory, as explained and illustrated by Thomson. He shows also that the bicarbonate of soda loses one-half its carbonic acid by exposure to a red heat—that the potash in supersulphate of potash is united to twice as much acid as the same quantity of potash in the neutral sulphate, and that potash unites with three different quantities of oxalic acid, which bear to each other the relation of 1, 2, and 4. Dr. Thomson always said, that in the absence of Dalton, Wollaston would have been, very soon, the discoverer of the atomic theory.

These facts gradually drew the attention of chemists to Mr. Dalton's views. Sir Humphry Davy, however, and others of our most eminent chemists, were hostile to them. In the autumn of 1807, Dr. Thomson had a long conversation with Mr. Davy at the Royal Institution, during which he attempted in vain to convince him that there was any truth in the new hypothesis. A few days after, he dined with him at the Royal Society Club at the Crown and Anchor in the Strand. Dr. Wollaston was also present. After dinner every member left the tavern, except Dr. Wollaston, Mr. Davy, and himself, who all remained behind, and sat an hour and a-half conversing upon the atomic theory. Wollaston and Thomson tried to convince Davy of the inaccuracy of his opinions; but he went away more prejudiced than ever. Soon after, Davy met Mr. Davies Gilbert, the President of the Royal Society, and exhibited to him the atomic theory in so ridiculous a light, as to make Mr. Gilbert call afterwards on Dr. Wollaston, to learn, probably, what could have induced a man of his sagacity and caution to adopt such opinions. Dr. Wollaston begged of Mr. Gilbert to sit down and listen to a few facts which he would state to him. He then went over the principal facts, at the time known, respecting the salts in which the proportion of one

of the constituents increases in a regular ratio; and the relations also which Dalton had found carbon to bear to hydrogen in olefiant gas and carburetted hydrogen. Mr. Gilbert went away a convert to the truth of the atomic theory, and had soon the merit of convincing Sir Humphry Davy, who ever after was a strenuous supporter of it.

Instead of Dalton's term "atom," which Thomson adopted, Davy always used the word "proportion," and Wollaston "equivalent," which was much better; but whatever term we employ, now that the thing itself is understood, there can be no doubt that the use of the word "atom," (which conveys at once the idea of an ultimate indivisible particle,) greatly contributed to the reception of the doctrine of definite proportions. In 1808 Mr. Dalton published a volume of his own, in which not more than five pages, widely printed, and one plate with explanations, were devoted to the announcement and illustration of the atomic theory. This treatise, if such it can be called, is little more copious than that which had been given the year before from Dr. Thomson's notes."

9. *Prefatory Notice of Laurent's Méthode de Chemie;* by J. B. Biot.—This work, rich in new ideas, which have often proved fruitful to the author himself, offers us the deep convictions of a man who has enriched science with numerous and unexpected discoveries. It is the review of the thoughts of his life; and he felt so deep an interest in leaving behind him this bequest, that he labored to complete it even while in the arms of death. These reasons demand that the work should be received with serious consideration, and a mind free from previous prejudices. But to read it with profit and a just appreciation, it is important to bear in mind the *end* which Laurent had in view in its composition. He desired to place in the hands of chemists a collection of analogies drawn from experiment, which should guide them by the strongest probability if not with entire certainty, in the explanations to which they are continually obliged to have recourse. The operations of chemical analysis, whether applied to native or to artificial products, make known only the nature and relative proportions of the elements which compose them. They cannot teach us whether the molecules of the constituent elements are combined in all cases according to a single general mode alike for all; or whether they are distributed into distinct groups, combined among themselves without individual decomposition, and coexisting with their peculiar properties in the complete product. This question it is all important to decide, and to ascertain the special conditions in either case. For we should expect that the reactions of a substance would differ according as its molecular constitution is homogeneous or heterogeneous; and in the latter case according to the nature of the groups therein associated. Thus there are abundant examples of bodies, formed from the same simple elements and in the same proportions of weight, which are wholly diverse in chemical and physical properties. On this point, the most elevated in chemical reasoning, analysis gives no direct information, since its results only distinguish the simple elements separated from each compound, and affirm nothing as to whether they are isolated, or combined in groups whose preëxistence cannot be affirmed. Hence it may be justly said that it judges of bodies only after they have ceased to exist.

The preëxisting state of such bodies can therefore be determined only by induction, and must rest upon the analogies of properties and reactions; or else upon theoretical views which by giving a simple conception for each body under consideration connects it by probable marks with those substances with which it appears to have the closest relations in molecular constitution.

Now this latitude which each chemist allows to himself in each particular series of his investigations, has introduced great confusion into the science which must still increase; particularly in proportion as we advance in the study of organic products, where the combinations of a small number of simple substances, always the same, present a diversity almost infinite. Laurent has had in view to control the exercise of this liberty by subjecting it to general and uniform laws. Among the great number of formulas by which the bodies made known by chemical analysis can be theoretically represented, he proposed to seek out those which in the present state of our knowledge should be preferred, as offering the most general advantages for classification and the practical study of compounds; and by bringing together analogous species and separating those which are unlike by characters both numerous and striking, to enable one to foresee, from a careful inspection of the symbolic formulæ, the greatest possible number of reactions which they may exert and the products which may be thereby deduced from them. In a word, he has attempted by a comprehensive method, to do that for the collected researches of the chemical science of the day, which each chemist is trying to do in his own line of research, with limited and arbitrary views. Has he succeeded in this great task, for which perhaps chemical science hardly offers sufficient material in the way of well ascertained facts? This no one will be bold enough to affirm, nor unjust enough to demand. The inquiry we should make in reading his work is, whether, in the majority of cases on which he has relied to sustain himself, his views are conformable to experience; so that each may have a chance of finding them fruitful in his own case, as they have been to Laurent and others. If they have this effect, even within these limits, he will not fail to applaud, while laboring to carry the principles to perfection. To repel or reject them at first view because they are strange or perhaps announced with too great boldness of expression, would be a policy little likely to advance science. If our author sometimes attacks rather rudely the edifice of chemical science which has been formed by slow and successive additions, it is because, seeing the incoherence of the accumulated materials composing it, he has believed it to be more profitable to labor for its reconstruction, than strive to preserve it as it is. He has aimed only to assist in the undertaking, by pointing out the relations of types and symbols, which, in the absence of more accurate notions, offer generally some reliable grounds for grouping or separating species. In a majority of cases, chemistry will thus be able to escape from that empiricism in which until lately it has remained.

The power of rotating polarized light exercised by a great number of bodies, as far as known exclusively organic, furnishes a definite character by which the abstract speculations based upon the constitution of the compounds of which they form a part may be either confirmed

tions in rational chemistry, of the kind which Laurent has treated. But the employment of this method is as yet but little extended, although it has always been fruitful for those who have employed it in their researches.

10. *Smithsonian Contributions to Knowledge.* Vol. VI. 1854.—In the volumes of able papers which are issued under the Smithsonian fund, this Institution is conferring a lasting benefit on the cause of knowledge through the land and through the civilized world. Works, the result of profound research, which would fail of a publisher because not fitted to command a ready "cash" return, here find encouragement and the means of an honorable introduction to the libraries of the land, and directly or indirectly, the views, new principles, and results of researches and explorations over this and other lands, gradually pass into general circulation. The volume just now issued, the sixth of the series, contains the following memoirs:—

Plantæ Fremontianæ, or Descriptions of Plants, collected by Col. J. C. Fremont in California; by JOHN TORREY. 24 pp. and ten plates.

Observations on the Batis maritima of Linnæus, by JOHN TORREY. 8 pp. and one plate.

On the Darlingtonia Californica, a new Pitcher Plant, from Northern California; by JOHN TORREY. 8 pp. and one plate.

Synopsis of the Marine Invertebrata of Grand Manan, or the Region around the Bay of Fundy, New Brunswick; by WM. STIMPSON. 68 pp. and three plates.

On the Winds of the Northern Hemisphere; by JAMES H. COFFIN, Prof. of Mathematics and Natural Philosophy in Lafayette College, Pa. 200 pp. and 13 plates.

The Ancient Fauna of Nebraska, or a Description of Remains of Extinct Mammalia and Chelonia from the Mauvaises Terres of Nebraska; by JOSEPH LEIDY, M.D., Prof. Anat. Univ. of Pennsylvania. 124 pp. and 25 plates.

Appendix: Occultations of Planets and Stars by the Moon, during the year 1853, computed by JOHN DOWNES.

The long paper by Prof. COFFIN consists largely of tables, presenting abstracts of observations, bearing on the winds and temperature of the different zones and regions of the globe. It embodies also deductions from the observations, which are of great interest, although of course liable to modifications in some cases, as facts are further multiplied. The following are some of these conclusions:

(1.) In the Arctic regions of North America, within the Polar circle, the mean direction of the wind is about *north-northwest*, and well defined.

(2.) Between the parallels of latitude—60° and 66° there appears to be a belt of easterly or northeasterly winds.

(3.) South of this belt, there is a zone of westerly winds, (as now commonly recognised) which encircles the globe, and is about 23½° in breadth. Near the limits which divide this zone from the Polar winds on the north and from the equatorial on the south (particularly the latter)

the progressive motion is very small. The progressive motion moreover is less in Europe than in America.

(4.) South of this zone and contiguous to it, the winds in the United States and on the Atlantic are on the whole easterly, though quite irregular, with small progressive motion.

(5.) Further south, there are the well known northeasterly trade winds, stronger between 10° and 25° than nearer the equator. In the eastern Atlantic near Africa, the winds incline towards the great Desert. In southwestern Asia, they are very irregular, and defy all attempts to reduce them to system.

The system of winds of the northern hemisphere are therefore regarded by the author, as (1) a southerly in direction in the high northern latitudes, but veering towards the west as they approach a limit ranging from about latitude 56° on the western continent to about latitude 68° on the eastern, when they become irregular and disappear; the area of this zone is about 11,800,000 miles; (2) a zone of westerly winds around the earth less than 2000 miles broad; area about 25,870,000 square miles; (3) south of this last zone, a zone of easterly winds—area about 60,760,000 square miles.

(6.) On each side of the Atlantic there is a system of winds possessing monsoon features. Drawing curved lines to represent the mean annual and the monthly tracks, the curves for the warmer months, fall inside of the mean in the warmer months and outside in the winter, showing a deflection towards the land in the former and towards the sea in the latter—an effect which the author alleges to be "most convincing proof of the influence of heat in the production of winds and that too upon an extensive scale." These monsoon winds are stated to be analogous to land and sea breezes, only on a grander scale. The influence of the northern lakes on the direction of the wind, and also of deserts, is discussed in this connection.

These are some of the points brought out in Professor Coffin's paper.

The extended paper on the Nebraska fossil Mammalia by Prof. Leidy, contains elaborate descriptions and illustrations of the ancient Fauna of the remarkable Mauvaises Terres, an earlier less complete paper on which by the same author has already been noticed in a former number of this Journal. The Mammalia all belong to the order Ungulata or hoofed Mammalia, excepting a single carnivorous animal of the feline genus Machairodus. Of Chelonian Fossils, there are five species all of the genus Testudo. The following are the names of the Ungulata described.

Poebrotherium Wilsonii, Leidy—most nearly allied to the musks.

Agriochœrus antiquus, Leidy.—In structure between the ordinary Ruminants and the anomalous Anoplotherium.

Oreodon, Leidy.—Also between ordinary Ruminants and the Anoplotherium. Species Oreodon Culbertsonii, about the size of the wolf of Pennsylvania; *O. major*, larger than the Culbertsonii.

Eucrotaphus, Leidy.—Probably related most nearly to Oreodon. Species, *E. Jacksoni* and *E. auritus*.

Archæotherium, Leidy.—A genus of Suilline Ungulates.

Anchitherium, Meyer.—Related to Palæotherium.

Species *Anchitherium Bairdii.*

Titanotherium, Leidy.—Related to Palæotherium. Species *T. Proutii*, (this Journal, 1847, iii, 248.)

Palæotherium giganteum, Leidy.—Twice the size of the P. magnum.

Rhinoceros occidentalis, Leidy.—Three-fourths as large as the R. indicus. *R. Nebrascencis*, one-fourth smaller than the R. occidentalis.

The species of Carnivora described is the *Machairodus primævus*, an animal a little smaller than the American Panther.

The plates illustrating this paper are admirable. The Mauvaises Terres or Bad Lands, are situated near latitude 42° N. and longitude 26° west from Washington, or 103° west from Greenwich.

11. *Verd Antique Marble.*—The papers state that the new City Hall of New York is to be built of Verd Antique or Serpentine marble, either from Vermont or the quarries of Milford, Connecticut. The latter is a miserable material for all out-door use, though elegant for in-door purposes. When the Milford quarries were first opened some 35 to 40 years since, the polished slabs were used as monuments in the New Haven Cemetery; and now they are as gray and rough as if remnants of Assyrian antiquity. The Vermont material is more purely a serpentine rock, and will not wear as unevenly. But unpolished, it is a dull, blackish, gloomy stone, turning brownish gray on exposure, and fit only for a prison. It is quite time that in the selection of building material for public structures in the United States, some reference should be had to the quality and fitness of the rock. The Greeks were wise in using material which 2000 years have not wasted nor diminished in beauty.

12. *Mastodon.*—A skeleton of a Mastodon has been recently discovered buried in a marsh about two miles from Poughkeepsie, New-York. Its state of perfection is not known, as it is yet but partly exhumed. This is the second skeleton obtained from the vicinity of this city.

13. *British Association.*—The British Association commenced its session for the present year at Liverpool, on the 20th of September last.

14. *Ueber das Iridium und seine Verbindungen.* Inaugural-Dissertation zur Erlangung der Philosophischen Doctorwürde von Ezequiel Uricoechea, aus Bogota. 38 pp. 8vo. Göttingen, 1854.—The author of this Inaugural Dissertation, Mr. Uricoechea, from Bogota, has just graduated with honors at the Göttingen University, in the Department of Philosophy. A history of the discovery of the Platinum metals is first given, in the course of which he observes that, the word Platina, although derived from the Spanish *Plata*, silver, is not properly a diminutive of that word, but signifies more correctly *silver-like*, or like silver. He cites the sentence from Scaliger, of the 16th century, in which he alludes to this metal, as follows: "Præterea scito, in funduribus qui tractatus est inter Mexicum et Dariem, fodinas esse orichalci quod nullo igni, nullis Hispanibus artibus, hactenus liquescere potuit;" and also, the subsequent notice of it in the Relacion historica, &c. of Ulloa, "Donde la Platina (piedra de tanta resistencia que no es fácil

ni el arbitrio para extraer el metal que encierra, sino á expensas de mucho trabajo i costo." The author ably reviews also the scientific history of the platinum metals, and especially of Iridium, and closes with some results of his own investigations on the Phosphate of oxyd of Iridium, Bromid of Iridium and Sodium, Bromid of Iridium, Sulphates of oxyd of Iridium and Chlorid of Iridium and Magnesium.

15. *The Chemistry of Common Life;* by JAMES F. A. JOHNSTON, M.A., F.R.S., F.G.S., &c. 12mo. New York, 1854. D. Appleton & Co. Nos. I, II, and III.—The subjects treated of in these three Nos. are "The Air we Breathe," "The Water we Drink," "The Soil we Cultivate," "The Plants we Rear," "The Bread we Eat," "The Beef we Cook," "The Beverages we Infuse," "The Sweets we Extract," "The Liquors we Ferment," and first part of "The Narcotics we Indulge in." In all 270 pages 12mo.—Science is here brought to bear successfully and attractively on the common processes of domestic and outdoor life. The three numbers issued are about half the whole work.

16. *Scenery, Science and Art, being Extracts from the Notebook of a Geologist and Mining Engineer;* by Professor D. T. ANSTED, M.A., F.R.S., &c. 324 pp. 8vo. London, 1854. J. Van. Voorst.—Prof. Ansted's work contains brief but animated descriptions of the people and country met with in his travels, ranging through portions of France, Switzerland, Germany, Spain, Sardinia, Algiers, and the United States; and many excellent views, part in lithotints, illustrate the scenes of which it treats. A large part of the volume is devoted to the mines, mining resources and geology of the regions visited, and these add largely to the substantial value of the work.

17. *Souvenirs d'un Naturaliste;* par A. DE QUATREFAGES, Membre de l'Institut. In 2 vols. 18mo. Paris, 1854. Victor Masson.—The author of this work, A. De Quatrefages, is one of the most active and thorough Zoologists of France. These volumes are in part a popular Journal of his various excursions to the Mediterranean and other regions, and partly reflections and discussions on scientific topics, of more or less general interest. His object, as he states, in his Preface, was to present the great truths and principles of zoology in a manner that should make their true bearing, interest and value apparent to the public at large. Whether speaking of the incidents of his tours, the people among whom he is cast, or of science, the work is interesting and instructive.

18. *The Principal Forms of the Skeleton, and of the Teeth;* by Professor R. OWEN, F.R.S., &c. 330 pp. 12mo. Philadelphia, 1854. Blanchard & Lea. This book is by the most eminent Comparative Anatomist of Britain. It was written as an introduction to his favorite Science, and reviews the structure of the principal forms of the Skeleton, and of the Teeth in the Vertebrata. It is illustrated by many wood-cuts.

19. *Principles of Comparative Physiology;* by WM. B. CARPENTER, M.D., F.R.S., &c. 752 pp. 8vo, with 390 wood-cuts. A new American, from the 4th and revised London edition. Philadelphia, 1854. Blanchard & Lea. The whole range of organic nature, both vegetable

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language, that covers the same ground ir

20. *Human Physiology*, designed for C
in Schools, and for general reading; by
390 pp. 12mo, with nearly 200 wood-cu
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struction and general reading. His worl
distinctions of organized and unorganized
animals and plants, and man's relations t
ture; second, of the Human Structure as
third, the uses for which the structure is d

21. *Science and Mechanism Illustrate
York Exhibition*, 1853–54; including
most important contributions in the variou
tions and notes relative to the Progress
Science and the useful Arts. Edited by
by Professors HALL and SILLIMAN, and
men. New York, G. P. Putnam & Co. 1
merous illustrations.

This long expected volume has at last
a uniform style with the Illustrated Recor
a vast variety of information, much of it
wide range of topics covered by its 31 Cl
Mineralogy, Geology and Mining (Class
ceutical products (Class II.) and to Philosc
are those most interesting to men of scien
mit at this moment any extended abstracts
occasion to refer to it again at an early da

22. *History of the Fishes of Massachu*
STORER.—We have just received a secon
extending from page 91 to 130. It embr
the Genera Blennius, Gunnellus, Zoarces,
nectes, Batrachus, Ctenolabrus, Tautoga,
somus, Hypsolepis, Cheilonemus, Argyreu
and is illustrated with plates 17 to 23, con

23. *The Principles of Animal and Ve*
lar Treatise on the Functions and Phenon
is prefixed a general view of the Great D
edge; by J. STEVENSON BUSHNAN, M.D.
tan Free Hospital, &c. 234 pp. 12mo
delphia, 1854. Blanchard & Lea.—Th
thing in this small duodecimo volume.

24. *System der thierischen Morpholog*
Prof. der vergleichenden Anatomie in Lei
wood-cuts. Leipzig, 1853. Wm. Engelm
Comparative Anatomist of Leipzig, discu
sophical and profound, the general princi
the animal kingdom. The structures of th
organs are described with comprehensive

tures presented by the different types of animal forms. The work closes with chapters on the fundamental relations of type series, and the essential idea of the animal structure.

25. *Atlas von Nord America, nach den neuesten Materialen*, in 18 Blättern mit erläuterndem Texte, herausgegeben von HENRY LANGE. Braunschweig, 1854. G. Westermann.—This Atlas of North America contains 18 plates.—The first is a general map of North America; the next twelve are devoted to the several states of the United States; the 14th to British America; the 15th is an ethnographical chart; the 16th illustrates the distribution of mammalia over North America; the 17th the distribution of plants; the 18th is an enlarged map of San Francisco, the Sacramento and San Joaquin. The maps, although small, are admirably executed, and are remarkable for the fidelity with which they give recent results. The form of the Atlas is a broad quarto, and with each plate there is a leaf of text containing statistical details.

26. *Of the Plurality of Worlds, an Essay.* 279 pp. 18mo. London, J. Parker & Son.—The author of this able essay, whose name does not appear on the title page, endeavors to prove that our own world is the only one in space which is inhabited by rational beings. The argument is conducted with consummate skill and great power, usually with fairness, although sometimes sophistical when direct reasoning was insufficient, and in all parts with ennobling thoughts of man's relations and destiny. With regard to the planets of our system, the evidence is very nearly conclusive that the earth is nearly or quite alone in being tenanted by man. Jupiter, as its density is but 1·1, or little above that of water, is regarded as mostly liquid; Saturn, which is not heavier than cork, as made up mainly of liquid and vapor; and thus all the outer planets are stated to be unfitted from their nature as well as the absence of light and heat, for the higher orders of life. The Earth, the largest of the *solid* planets, is in the temperate zone of the Planetary system, and air, earth and water have here their most equable relations. The argument respecting the fixed stars carries far less, we should say, very little, probability with it. The work has been republished in this country.

27. *More Worlds than one;* by Sir DAVID BREWSTER, is the title of a work written in reply to "The Plurality of Worlds."—The distinguished author rests much on the ground that the making of a world is waste labor unless the surface is afterward stocked with inhabitants; and without a very strict appeal to the analogies furnished by science, he counts much on what Infinite power *may* do in adapting rational creatures to conditions of all possible kinds. The sun, the lava-covered moon, and even Neptune which rolls through space in a perpetual Arctic night, are supposed to have their inhabitants. In these deductions, the work goes to an extreme the opposite of that of "The Plurality of Worlds."

28. *Sixty-seventh Annual Report of the Regents of the University of the State af New York.* 316 pp. 8vo. Albany, 1854.—This Report, like its predecessors, contains much information in the department of Meteorology, besides the various School Reports.

29. *Seventh Annual Report of the Regents of the University of the State of New York, on the Condition of the State Cabinet of Natural History and the Historical and Antiquarian Collection annexed thereto.* 124 pp. 8vo. Albany, 1854.—In addition to lists of recent additions to the State Collections, this volume contains analyses of specimens of salt from different salt mines, domestic and foreign, and a memoir with two plates, on the Serpents of New York, by SPENCER F. BAIRD.

30. *Popular Lectures on Drawing and Design;* by WM. MINNIFIE, Professor of Drawing in the School of Design of the Maryland Institute. 53 pp. 12mo. Baltimore, 1854.—This small pamphlet is made up mostly of popular addresses on some public occasions by the author.

ERASMUS WILSON, F.R.S., Healthy Skin: A Popular Treatise on the Skin and Hair, their preservation and management. 2d American, from the fourth and revised London edition, with illustrations. 292 pp. *Philadelphia*, 1854. Blanchard & Lea. 75 cents.

MÉMOIRES de l'Académie Royale des Sciences de Belgique, Tome 27, 1858; Mémoires couronnés et Mémoires des Savants étrangers, Tome 25, 1851–1853. This last volume contains a memoir of 326 pages and 38 fine plates, on the Fossils of the Secondary Formations of Luxembourg, by M. F. Chaupuis and M. G. Delwaque.

JOURNAL OF THE UNITED STATES AGRICULTURAL SOCIETY. 279 pp. 4to. *Boston*, 1854. Published quarterly.

REPORT OF THE TWENTY-THIRD MEETING OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, held at Hull in September, 1853. 212 and 142 pages, 8vo. 1854.

DR. HERMANN SCHILDENER: Der Process der Weltgeschichte als Grundlage der Metaphysik oder Wissen des Wissens ist Wissen der Geschichte. 228 pp. 8vo. Greifswald, 1854. G. A. Koch'sche Verlags-Buchhandlung, Th. Kunike.

PROCEEDINGS ACAD. NAT. SCI. PHILADELPHIA. Vol. VII, No. IV.—p. 122. Description of new species of Viviparous marine and freshwater fishes from California; *W. P. Gbibons.*—p. 128. Note on Entophyta; *J. Leidy.*—p. 129. Descriptions of New Fishes collected by Dr. A. L. Heermann, Naturalist attached to the Survey of the Pacific Railroad route, under Lieut. R. S. Williamson, U. S. A.; also (p. 141) of Marine Fishes from San Francisco, collected by Dr. Kennerly, attached to the Survey under Lieut. A. W. Whipple; also (p. 142) of Fishes from the Pacific coast collected by Lieut. W. P. Trowbridge, U.S.A.; *C. Girard.*—p. 156. Synopsis of Extinct Mammalia, the remains of which have been discovered in the Eocene formations of Nebraska; *Dr. Leidy.*—p. 158. Notice of a new genus of Cyprinidæ; *S. F. Baird* and *C. Girard.*—Synopsis of the Erotylidæ of the United States; *J. L. LeConte.*—p. 163. Descriptions of new fossil species from the Cretaceous formation of Sage Creek, Nebraska, collected by the North Pacific Railroad Expedition, under Gov. J. J. Stevens; also from the freshwater formation of Nebraska; *J. Evans* and *B. F. Shumard.*

COMPLETE WORKS OF M. ARAGO. *Paris*, chez Hector Bossange.—Two volumes have appeared; the first containing historic eulogies (of Young, Fresnel, Watt, Volta, Fourier, Carnot), and also the history "de ma jeunesse;" the 2d containing Notes on Thunder, the Aurora borealis, Electro-magnetism, Magnetism of rotation discovered by Arago, and some words on Table Turnings.

The following works also are published by H. Bossange:

DE LA BAGUETTE DIVINATOIRE, DU PENDULE DIT EXPLORATEUR, ET DES TABLES TOURNANTES, au point de vue de l'Histoire, de la Critique et de la Méthode expérimentale; par M. Chevreul, Membre de l'Institut. Volume in-8, 1854. Prix: 5 francs.

BOILEAU, Professeur de Mécanique appliquée à l'Ecole d'application de l'Artillerie et du Génie.—Traité de la mesure des eaux courantes, ou Expériences, Observations et Méthodes concernant les lois des vitesses, le jaugeage et l'évaluation de la force mécanique des cours d'eau de toute grandeur; le débit des pertuis des usines, des fortifications et des canaux d'irrigation; et l'action dynamique des courants sur les corps en repos. In-4, avec 7 pl.; 1854. 20 *fr.*

YVON VILLARCEAU, astronome à l'Observatoire de Paris.—Sur l'Établissement des Arches de Pont, envisagé au point de vue de la plus grande stabilité, et Tables pour faciliter les applications numériques. In-4 avec figures dans le texte et 2 planches; 1854. 12 *fr.*

INDEX TO VOLUME XVIII.

ERRATUM.—Vol. xvii, p. 431, 16th line, after ilmenite, add "to calcite and other hexagonal species."


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